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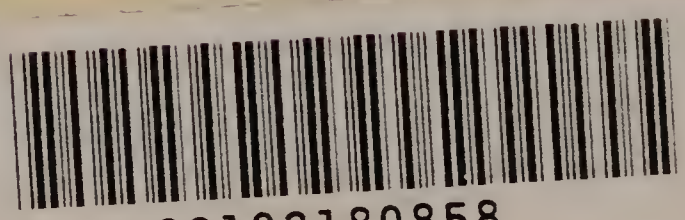
January, 1874.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THE

CHEMICAL NEWS

AND

JOURNAL OF PHYSICAL SCIENCE.

(WITH WHICH IS INCORPORATED THE "CHEMICAL GAZETTE.")

A Journal of Practical Chemistry

IN ALL ITS APPLICATIONS TO

PHARMACY, ARTS, AND MANUFACTURES.

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WILLIAM CROOKES, F.R.S., &c.

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THE CHEMICAL NEWS.

VOLUME XVII.

No. 422.—January 3, 1868.

ON THE MANUFACTURE OF GLASS FOR VESSELS EMPLOYED IN CHEMICAL RESEARCHES.

By Professor J. S. STAS.

In my researches "*on the Reciprocal Relations of Atomic Weights*" I have stated that the ordinary glass of which retorts, flasks, &c., are made, gives up to nitric and hydrochloric acids at the ordinary temperature traces of the metals it contains. In such vessels it is impossible to evaporate the purest acid to dryness without leaving a saline residue. Hard Bohemian glass, known as *refractory*, and in general all glasses having no alumina, and containing an excess of silica, resist for an almost indefinite time the action of hot concentrated acids; but the manufacture of balloons, flasks, and retorts in refractory glass presents great difficulties, the most skilful workmen not being always able to work in it when articles are required of an extra large size. I have had this difficulty frequently brought before me.

Wishing to ascertain what should be the composition of glass which would be at the same time unaffected by acids and sufficiently fusible to be manipulated with no great difficulty, I decided to carry out some experiments on this manufacture in an actual glass house. These researches showed me that a glass having for bases sodium and calcium, if it contains a sufficient excess of silica, resists acids almost as well as refractory Bohemian glass, having for bases potassium and calcium. But it is known that a mixture of equal molecular weights of the carbonates of sodium and potassium is much more fusible than the most fusible of either carbonate by itself; starting from this fact, I have been led to the endeavour to replace, in the composition of refractory glass unattacked by acids, a portion of the potassium by an equivalent quantity of sodium. The result has completely verified my anticipations.

I started from this fact, that to obtain a glass very refractory and unattacked by acids, having for bases potassium and calcium, it should contain about—

Silica	75·00
Oxide of potassium	15·00
Oxide of calcium	10·00
	100·00

Upon replacing in such a glass half of the potassium by its equivalent of sodium, we have—

Silica	77·00
Oxide of potassium	7·70
Oxide of sodium	5·00
Oxide of calcium	10·30
	100·00

In this glass the bases are in the proportion of one atom of calcium (Ca″=40) to one atom of potassium and one atom of sodium.

With these data I made some trials on a manufacturing scale; using for this purpose fine, pure sand employed in the manufacture of crystal glass, monocarbonate of potassium as pure as it comes from the English works, purified bicarbonate of sodium, and carbonate of calcium in the form of white marble, finely pulverised and passed through a silk sieve. These materials, in appropriate quantities, were intimately mixed with ten or twelve per cent of their weight of arsenious anhydride, and were then submitted in very refractory crucibles to a heat strong enough to bring them to a sufficient state of fusion to enable the glass to be worked. The addition of this enormous quantity of arsenious anhydride was made by the superintendent of the glass works, with the object of more readily determining the liquefaction of the mass. I confess I cannot understand the action of this; it produced, however, no other inconvenience than filling the air with torrents of poisonous matter, and analysis has satisfied me that no trace of the arsenic employed remains in the glass produced.

Operating with the proper proportions, two meltings were effected upon tolerably large quantities. With the glass obtained I had balloons with long necks, matrasses, small flasks, cylinders, &c., blown. The largest balloon which an excellent workman succeeded in making held about four litres; the capacity of the other balloons varied from one to three litres. The sides of the sphere were kept thick enough to be able to resist the traction to which the glass would be exposed by the shrinking experienced by nitrates when solidifying after fusion.

This glass had a yellowish reflection; it was excessively hard, but little elastic, and as free from hygrometric properties as the best refractory glass of Bohemia.

I took the trouble to submit to analysis some fragments of two balloons from different batches; these balloons were broken after having been used for my experiments. They contained—

Silica	76·4	77·3
Oxide of potassium	7·1	6·2
Oxide of sodium	5·9	6·5
Oxide of calcium	10·6	10·0
	100·0	100·0

In these analyses I determined directly the silica, and the oxides of potassium and calcium. The oxide of sodium was estimated by difference. The glass also contained a little alumina derived from the crucible; I did not weigh it; the numbers for the sodium are therefore that much in excess.

ON THE ESTIMATION OF CARBONIC ACID IN MINERAL WATERS.

By Professor FRESENIUS.

WHEN ammoniacal solution of chloride of calcium or barium is mixed with carbonic acid, the metals are not precipitated immediately in the state of carbonates. The author formerly stated that this was due to the previous formation of carbamate of ammonia, whilst M. Carius attributed it to the solubility of the precipitated carbonates in sal-ammoniac. The author has repeated several experiments in support of his view of the case; when chloride of calcium is added to a freshly prepared solution of carbonate of ammonia, the liquid is at first clear, but gradually becomes turbid. If the solution has been made about half an hour, the precipitate forms immediately.

To an ammoniacal solution of chloride of calcium, add twice its volume of water charged with carbonic acid, and it remains clear for a quarter of an hour; after twenty hours the precipitation was not complete and the filtered fluid immediately precipitated with a diluted solution of carbonate of ammonia. A diluted ammoniacal solution of chloride of calcium was treated by a current of carbonic acid for five minutes; it did not become turbid at once. When two drops of a solution of carbonate of ammonia, in 20 parts of water, were added, the first precipitate dissolved when agitated, but a third drop rendered it permament.

Dr. Fresenius concludes from these experiments that there is at first a formation of ammonical carbamate, which only changes slowly into carbonate; this transformation is rendered very rapid by heat. The presence of chloride of ammonium has no influence in these phenomena.

ON A NEW PROCESS OF ELEMENTARY ORGANIC ANALYSIS, FOUNDED ON THE ANALYSIS OF THE GASEOUS PRODUCTS.

By M. F. SCHULZE.*

Burn the substance to be analysed with chlorate of potash in a tube, having previously sealed and exhausted it; then submit to analysis the gaseous mixture produced. The advantage of this method is the small amount of material necessary; the analyses cited as examples were performed with from 5 to 13 milligr. of matter. M. Schulze introduces the mixture, together with rather more than enough chlorate for complete combustion, into a combustion tube, sealing it at one end, and drawing it out of the other; after having exhausted and measured the pressure of the remaining air, he seals the tube, shuts it up in a gun barrel, and heats it to a dull red heat for twenty minutes. When cold, he breaks the point of the tube under mercury and collects the gas in a eudiometer. By measuring the quantity of carbonic acid formed, and absorbing it by potash, are to be found all the elements necessary for the calculation of the composition of an organic matter containing only carbon, hydrogen, and oxygen. If the carbon absorbs its proper amount of oxygen, and the compound is a body corresponding to a hydrate of carbon, such as starch, the gaseous material obtained is exactly equal to the amount of oxygen supplied by the chlorate of potash used. If more gas be found, it is because the body contains more oxygen than is needed to burn all its hydrogen; if, on the contrary, there is less gas, it is because the body contained an excess of hydrogen with respect to its oxygen in the constitution of water. 5.5 milligr. of cholesterine, burned with 60 milligr. of chlorate, gave (after allowing for the air remaining in the tube) 9·6667 c.c. of gas (at 0° under one metre pressure). The 60 milligr. of chlorate furnished 12·483 c.c. of oxygen; the difference, 2·8177 c.c., is the quantity of oxygen corresponding to the quantity of hydrogen which was not burnt by the oxygen belonging to the cholesterine. Again, 2·8177 c.c. of oxygen burns 0·6664 milligr. of hydrogen (say 12·1 per cent.) The amount of gass aborbable by potash is 16·566 milligr. of carbonic acid, corresponding to 4·518 milligr. of carbon, say 82·145 per cent. The complement, that is to say

$$100-(12{\cdot}1+82{\cdot}145)=5{\cdot}775,$$

corresponds to the water formed by the oxygen in the cholesterine, and a part of its hydrogen, say 5·133 per cent. of oxygen and 0·639 of hydrogen; on adding this last number to 12·1, there remains 12·74 per cent of hydrogen. [These numbers, compared with those calculated from the composition of cholesterine,

$$(C=83{\cdot}8;\ H=11{,}8;\ O=4{\cdot}3)$$

do not appear satisfactory.] In the combustion of azotised matters, nitrogen may be obtained (after an estimation of the carbonic acid) by absorbing the oxygen with a stick of phosphorus, but the results are not very correct. As to chlorinated matters, the author effects their combustion with oxide of mercury; the chlorine remains in the condition of a mercurial chloride, in which it may be estimated by decomposing the chloride by potash.

* Zeitschrift fur Analytische Chemie, t. v., p. 239.—Zeitschrift fur Chemie, nouv. ser., t. iii, p. 391.

ON THE ESTIMATION OF LEAD BY PRECIPITATION IN A METALLIC STATE.

By M. F. STOLBA.

To estimate lead by this method, the author treats both soluble and insoluble lead combinations with zinc in the presence of water acidulated from time to time with hydrochloric acid; the reduction is effected at the temperature of the water-bath in a platinum capsule; the lead is deposited partly on the sides of the capsule and partly on the zinc, whence it is easily dislodged. When the reduction is complete, which is easily discerned by a clean surface of the zinc remaining brilliant in the liquid, decant and wash the spongy deposit of lead with water. As pure water might dissolve small quantities of lead, the author recommends an addition of a drop of sulphuric acid. After washing, dry the lead first in a water-bath, then at about 200° C. Even then its exact weight cannot be ascertained because it has undergone a partial oxidation. After weighing it, the oxygen absorbed must be ascertained, which may be done by Mohr's volumetric method—namely, by treating the lead with a weak standard solution of nitric acid. Wash the dissolved oxide of lead, and add a standard alkaline solution until it begins to produce turbidity. The quantity of oxide of lead is given by the difference in the standard of the nitric acid before and after its action on the lead.

Explosive Powder for Blasting Rocks, &c.—Experiments have recently been made in a cutting for a tunnel at Milford between Nobel's nitroglycerine and the explosive powder which is manufactured, wirhout danger of decomposition a spontaneous explosion, by Mr. Horsley. The preference was given to the powder not only for its power when exploded in a particular kind of cylinder, but alas for its economy, and its greater safety in use and storage. It requires a temperature of 475° to ignite. It has been tried and approved by the Admiralty, and is much liked by miners, who say they are afraid of blasting oil, and think gun-cotton useless.

ON

HEAT AND COLD;

A COURSE OF

SIX LECTURES*

(*Adapted to a Juvenile auditory*),

DELIVERED AT

THE ROYAL INSTITUTION OF GREAT BRITAIN,

(*CHRISTMAS*, 1867—8),

BY

JOHN TYNDALL, Esq., LL.D., F.R.S.

Lecture I.

The nature of heat, and the various modes of generating it.—Friction and combustion.—Changes of volume produced by heat.

I wished very much indeed to be able to write out notes of these lectures in order that you might take those notes home with you, and that they might help you to remember what I spake here. But I have been so very, very busy with other matters—so very hard at work—that I have found it perfectly impossible to write out and to get printed those notes which I now refer to. In fact, I wished very much indeed to avoid giving this course of lectures altogether, in consequence of the heavy labours of another kind that I have been engaged in, but, however, some friends of mine said that the boys and girls here present would take it very unkindly of me, and would think that I was neglecting them if I did not come forward and give this course; and inasmuch as this is a thing I did not wish you to think of me, I thought it best to come forward and to do the best that I can under the circumstances. Of course your not having those notes to bring things back to your minds will render it all the more necessary on your part to give me the utmost possible attention, to endeavour to understand all I say,—and indeed at the very starting I shall have to bring some very difficult matters before you that will require a concentration of attention on your part. But I calculate—and I know I can calculate with confidence—upon your attention; and if you give me that attention, as I am sure you will, I have no doubt we shall get on, on the whole, exceedingly well together. (Applause).

Now, I suppose all of us twenty times a day—perhaps more—make use of the word "*I*." Every boy here present says, "I eat," "I drink," "I sleep," "I feel;" but perhaps very few boys or girls either ever ask themselves, "Who is this *I* that does all these things?" and if you went to the biggest man in the world, or the greatest philosopher, you would puzzle him exceedingly if you asked him "Who is this *I* that sleeps, and drinks, and eats, and feels?" In fact, philosophers, great as they may be—and great they are—find that there are things altogether beyond their knowledge and beyond their power to understand, and this wonderful human *I* is one of those things. Hence, I do not want you to be able to answer me if I ask, Who is this *I*—what is this *I*—that sleeps, and drinks, and eats, and feels, and makes use of its senses? In fact, as I have said, the best of us know very little about it; but we know a great deal of that peculiar instrument by which the *I* operates upon the world, and by which it understands the things that are going on in the world, and that instrument is the wonderful human body. When we examine that body, looking into its interior parts, we find bones and blood and muscles and tissues of various kinds; and passing through these muscles we find strings of whitish matter—strings going from the spinal marrow, and going from a mass of matter that rests in this wonderful cavity called the head. I say those strings of white matter go through the body, and they are called the *nerves*; and it is by the intervention of these nerves and this wonderful brain that we human beings are able, so to say, to hold converse with the world round about us. Now, these nerves transmit the impressions from without. If I prick my finger a nerve is effected: it is lacerated by the pricking of the pin or the penknife, and that nerve thus lacerated sends intelligence through itself up along the arm to the brain; and until it arrives at the brain you do not feel anything. It travels up to the brain at the rate of about 180 feet in a second. This is one of these wonderful things that have been measured by able men. You do not feel the exact moment your finger is pricked.

Now, what the nerves in all these cases convey to the brain is something in the nature of motion; and in order to enable you to form an idea of this motion I have arranged a little experiment. And here I must call upon that power which every boy and girl here possesses—that wonderful power which is sometimes called "imagination"—the power of picturing things before the mind. I would ask you to picture one of these nerves going through the body to the brain; and I would ask you to figure that nerve burned, we will say. Now, how are you to conceive of this nerve? The nerve is made up of very minute particles to which we give the name of "molecules" or "atoms." They are sometimes called atoms. In fact a molecule is an aggregate of atoms. But what I want you to clearly realise, and which is perfectly in your power to realise, is that these nerves are composed of little particles—(I do not care about the name, whether "atoms" or "molecules"); and if you disturb the end of any nerve—if you burn it—if you prick it—what you do there is that you impart *motion* to the body. This motion runs along the nerve, and when it reaches the brain it declares itself in some form—of pain, or, it may be, of pleasure. Now, how is this done? You may, in fact, consider those nerves to be like the telegraphic wires that go through the streets. You have seen them passing through the air of London; and these telegraphic wires carry messages to and fro through various parts of London. I say, you may consider the nerves as being represented by those telegraphic wires, and you may consider the brain a great central station, so to say, with which the nerves communicate—to which they communicate their messages, and from which they receive their messages. In order to make this plain I have have here arranged a little experiment—very simple indeed. You can make it yourselves with the glass balls used in the game of solitaire. You see I have there a series of these balls, and I want to enable you by these balls to conceive how motion is propagated through the nerves. There is nothing shot through the nerves: the motion is communicated from particle to particle. Observe, here. If I take hold of this ball and strike it against the first ball of this series, you will observe what occurs. The motion will be transmitted through all the series of balls. Each ball will take up the motion given to it by the preceding one, and pass it on to its neighbour, and thus the motion will go through the entire series so that the last ball of the series will be the only one affected. Observe how the last ball is detached. There it goes away. The moment I hit this first ball the terminal ball flies off. Now, in some such way—in a way somewhat analogous to this—is motion propagated to the brain. Allow this bell to represent the brain. Now, if we take our series of balls thus, and strike, as I have said, the first ball, the blow will be communicated to the terminal ball, and that, liberated, will strike against the bell. The sound of that bell is something like a signal given in the brain. [The bell was sounded in the manner indicated.] Here you have the motion transmitted from the first ball, and finally the bell is thus affected. In the way somewhat rudely and roughly represented by this experiment the motion is transmitted to the brain, and when it reaches the brain it

* Reported verbatim, by permission of the Author, for this journal.

evidences itself, as I have said, as pleasure or pain, as the case may be.

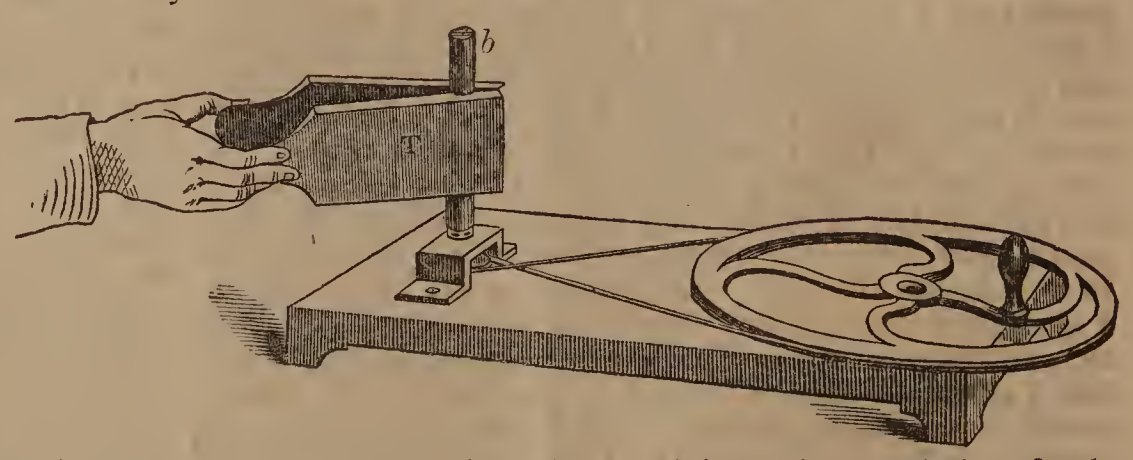
Now, having exercised your imagination upon those particles which I have called atoms or molecules, I think we may go on to consider the character of this power that we have to deal with in this course of lectures; that is, this thing that we call "heat." Long reflection and many experiments on this important subject have caused men of science—learned men who investigate such things—to the notion that this thing that we call heat is a kind of motion. And now I should like every, even my youngest hearer—(and that is a large demand)—to figure, by this power of imagination, what I describe. Take any substance,—for instance, this body which I hold in my hand. This, like our nerves, is composed of little particles or atoms. It is not absolutely cold at the present time. Of course it may feel cold to my hand, but it is really not cold. Those particles that I have been speaking of are in a state of motion. Although they are too small to be seen, and although the motion is entirely too small to be seen even by our best microscopes, still we have every reason to believe—the very strongest reason to believe—that the particles of that body at the present time are vibrating. The little particles, remember—(picture them to your mind)—are vibrating to and fro; and the warmer the body is, the more intense is this motion; and, in point of fact, it is this motion of the smallest particles of the body to which, when communicated to the nerves, and through the nerves to the brain, we give the name of heat. Now, although I am dealing with some of the deepest things in science, still I expect all the boys and girls here to clearly figure to their own minds this substance as an assemblage of small particles, and those particles oscillating—vibrating; and the warmth that I feel when I take this in my hand is due to the multitude of these small motions that are going on within the body.

Well, now, this motion of the particles of a body can be excited in various ways, and one of the most ordinary ways of exciting it is by friction. If you take, say a flat brass button, in your hand, and if you rub this button upon a surface of wood, as I am doing this which I hold in my hand, very soon, by rubbing this body [a short rod of metal] I make it so hot that I don't like to bear it against the skin of my face. In point of fact, the friction exerted against this substance produces the motion wecall heat, and I very nearly burn myself. The rubbing throws the particles into this furious motion. If I place this body, before rubbing it, upon a flat piece of white wax, there it stands; but let me rub the end of the piece of metal for a time, thus, and then place it upon the wax, you observe it runs away; it melts the wax underneath it, and slides down in this way. This body [a similar short rod of metal] which has not been rubbed, will never melt the wax, and there it rests. The sliding of the other piece of metal is due to the heat produced by the friction. And in various other ways heat is produced by friction. For instance, if you take a saw, and pass that saw through wood, if you are careless and do not put grease upon the saw, then there is so much friction that the amount of heat developed in the saw becomes very great indeed. The saw becomes quite hot. And that is the theory and that is the reason why carpenters grease their saws when they use them. They do not want to make heat, for when this friction is overcome you actually *create* heat. Now, the carpenter is not anxious to make heat; he wants to get through the wood, and he wants to get through it with the least possible trouble; and, in consequence, he lessens the heat by putting grease upon the saw; he makes it as smooth as possible.

In this way, then, that is, by means of friction, we can actually generate, produce, create, this thing we call heat—this motion; and that is a very important point. It was thought for a long time impossible that heat could be generated. It was supposed that there was a certain quantity of heat in the universe, and that this was perfectly constant, no change occurring in it; but you see we have simply to produce this motion of the particles, and then that motion we call heat is set up. I have here an experiment that will still further illustrate this. When I was a boy—and I suppose I was like the average of boys—I was very fond of savages, and people of that kind. Now, I should like immensely to be able to transform myself into a New Zealand savage for the next five minutes. If I could do so I should be able to make a very beautiful experiment which it is not now in my power to do, for I am not so clever as those savages. My friend, Sir John Lubbock, who is a very great man on savages, has given me these two sticks. These are the genuine articles, brought from Australia. This stick is made of a particular kind of wood, pithy, and rather soft; and you see there are holes in one of the pieces of wood. This second stick is made of a harder material. Now, one of these native savages takes one stick and places the end of it in one of the holes of the other stick. He then clasps it, thus, and by the friction he uses he causes a little dust, first of all at the end. He works on until that dust takes fire; and then he manages by blowing, and by operating with far more skill than I can bring to bear upon the experiment, to actually produce flame. These are the very articles used by these New Zealand savages when they wish to produce fire by friction.

Well, I can illustrate still farther this mode of producing heat. I have here, you see, a hollow tube, *b*, and I will place in this tube a quantity of a certain liquid which boils a little more readily than water. I might take water, but I will make use of ether for the purpose of making the experiment more rapidly. Now I will try whether I can not boil that liquid by friction. You see after putting

FIG. 1.



the ether into the tube I cork it up thus, and then fix the tube on this instrument which is called a whirling table, and by means of which I can cause the tube of liquid to spin round with great rapidity. The tube is now fixed firmly upon the whirling table, and we will there spin it rapidly round and round. I could boil that ether by simply clasping the tube in my naked hand. I have done so over and over again. The friction of my hand against this tube has been sufficient to boil this ether, but I have found it very hot and very unpleasant; and in order to protect my hand I will take a piece of flannel, and grasp the tube tightly with the flannel round it. Now, I want you to observe that if the experiment succeeds—(and experiments are always liable to fail)—the friction of the flannel against the tube which goes round and round will cause the ether to boil, and when that happens the steam of the ether underneath the cork will project the cork into the air. I want you now to observe the cork while I clasp the tube in the flannel. [In the course of a few seconds the cork flew from the mouth of the tube.] There it is, you see. Look at that!—boiled in half a minute,—boiled by the friction of that piece of flannel against the tube. Well' now, I have here another tube, and I have here a quantity of metal. Look at it,—hard metal. There it is. Now, I break that metal into bits thus; and I purposely avoided putting it into this tube until now so that you might actually see the metal going in, and see that there is no

delusion or mistake about the matter. Now, I will place some of this broken metal in this tube. We can put a little more in afterwards. I have put in as much as will go in now. I expect to be able to melt that metal by friction. I will cork the tube up tightly as in the former case, and when the metal is melted I will pour it out on this plate. [The rotation was commenced.] I am beginning to feel the heat now, and I have no doubt that very soon we shall have the metal in the tube molten. [Examines the contents of the tube.] Yes. I will put in more so as to get a greater quantity melted. I will pour it out presently, but you must first exercise your patience until we get it all melted. I put in as much as the cavity would hold in the first instance. Now, we will work the whirling table once more, and I will clasp it as before. [After a further interval]—Now the tube is so hot that I have no doubt the metal inside is melted. Yes, it is melted. Let us put in a last bit, and thus we shall get back the whole of that cake after it has been liquefied by the friction. I cork up the tube in order to keep the molten metal from splashing about. [The tube was caused to revolve again for a short time, and then detached from the whirling table. The metal was poured out, and found to be completely fused.]

Well, there are various other ways by which this motion that we call heat can be generated. It can be generated by percussion—by hitting with anything hard. For instance, I have here a piece of lead—a lead bullet: if I place this bullet upon an anvil, and strike it in this way, when I take it up afterwards it is too hot to hold, and burns me. I have actually created that heat. I have called that heat into existence. By hitting this bullet I have thrown its particles into this peculiar vibratory motion to which we give the name of heat.

Now, how do we know the precise amount of heat produced by a stroke of this kind? I had intended to make an experiment before you in connection with this point; but you will understand the experiment without my taking up your time to perform it in your presence. Here is a piece of lead, and there I have upon the floor a thick plate of iron. I intended to send one of my assistants to the top of the house, and I intended him to drop this piece of lead down, and let it fall upon this plate of iron. Now, it so happens that the height of this room is such that this piece of lead, having a certain amount of temperature on leaving the hand, would have that warmth augmented by one degree of temperature. I must here make use of the term "degree," although I cannot explain it till the second lecture; but you will remember that by the falling of this piece of lead from the ceiling, upon this plate of metal, we should raise the temperature of the lead one degree Fahrenheit. In like manner, if I sent up this liquid metal, which is called mercury, and had it poured out from the ceiling, and let it come down upon this plate, the mercury in falling from the top of the house to the bottom would have its temperature raised one degree. But if I took water it would be totally different. In this case I should have to go not to a height of 30 feet, but to a height of 770 feet and a little more, in order that the water should have its temperature raised one degree. You will understand this difference between water and mercury and between water and lead, by-and-by. I now wish you to understand that we can tell the exact amount of heat which a shot falling from a certain height can generate or produce; and we should find an increase of heat produced in all such cases if we had instruments of sufficient delicacy. No doubt many of you will see when you grow up that fine waterfall in Switzerland where the river Aar jumps or tumbles down a perpendicular precipice. I suppose it jumps from a vertical height of 400 feet. Well, if you could place a thermometer at the top of that fall, and another at the bottom, the water at the bottom, if the the thermometer were delicate enough, would be found warmer than the water at the top; and knowing the height from which the cataract plunges, we can tell the exact amount of heat generated by its fall downwards, through his power of percussion in developing heat.

When I was a boy instead of using percussion caps, which are now so common for firing guns, they used to employ an instrument of this kind in guns—[exhibiting an old-fashioned gun-lock]. Here is a piece of steel, and this other substance is a piece of ordinary flint which you see moves forward in this way. Now, I can cock that gun-lock, and then by pressing on the trigger I release the hold, and the flint falls against the steel, and you notice the sparks produced. This is a very old lock, and a very bad one; but still you see there are sparks produced when I liberate the flint, and it strikes against this steel. If we put a little powder in the pan beneath the flint we imitate what used to be the method of firing guns in former days. [The lock was then primed.] Now, you see when I let the flint strike the steel the gunpowder is exploded by the sparks produced. In the same way tobacco smokers and others used to get a light by igniting tinder by means of the sparks produced from a flint when struck on a piece of steel.

Now, what is the meaning of this experiment? What is the theory of that gun-lock? It is this. You have seen that when I struck the lead I raised its temperature. A very great man who used to lecture in this room many years ago—Sir Humphry Davy—caused a lock of this kind to go off where there was no air, and when he examined the lock afterwards he found that the flint had struck away little bits of the steel from the part of the lock against which it struck; and when he examined those little bits of steel he found that they had been fused; so that really the percussion of this flint against the steel surface is so strong that it raises those particles of steel which it breaks off almost to a white heat. When steel or iron is thus raised to a high temperature it is affected by a certain substance which is round about it in the air. You must remember the name of that substance, it is so very important. It is called *oxygen*; and when iron or steel is raised to a sufficient temperature, this oxygen instantly attacks it—plunges against it. As before, I must ask you to exercise your imagination with regard to this oxygen. You must figure in your minds this oxygen as very small particles diffused throughout the air. Then, I say, when the iron or steel is raised to a high temperature the oxygen diffused through the air plunges against it, and hits it so hard that there is a kind of percussion. The oxygen hits the iron or the steel so hard as to produce this thing that we call heat, and produce it in such a degree as actually to render the body white hot. Now, I want to show you that this is the case. I have here the means of producing a flame of considerable size; and downstairs we have a pair of bellows. A man has just quitted the room to work those bellows. A current of air will pass through this tube, and we shall obtain here a flame of considerable power. Now, what I want you to understand is this,—that if by means of this flame I heat particles of iron or steel, you will find that those particles of iron or steel will shoot out like stars because of the plunging upon them of the oxygen of the air. Here I have a vessel containing these iron filings, and as I throw them on the flame you see the sparks produced are very brilliant indeed. (Applause). The iron is burned in this way. I have thrown in sufficient of it to illustrate what I have been saying. First of all these particles of iron were heated exactly as in the case of the gun-lock; and when they were heated the oxygen of the atmosphere plunged against them, and plunged against them so violently as to produce these star-like forms which you have seen. Some call this force attraction or chemical affinity; but what I want you to see is this—that these particles of iron when heated to this temperature are showered down upon by the oxygen of the air. This wonderful substance of the air, called oxygen, forms but a small portion of the atmosphere—about one-fifth of it by weight. Hence, if we had the whole atmosphere composed of oxygen those effects of combustion would be very much greater indeed than they are at present. I have here some pure oxygen obtained

by proper methods, and I will just ask you to observe how much more powerfully this atmosphere of pure oxygen acts upon a body than does the oxygen in the ordinary air, where it is diluted, as I have said, to a considerable extent. I have here a piece of wood which I set fire to. I blow the flame out then, leaving the end red. You see the air has no power to make it ignite again. If I bring it into the oxygen see what occurs. [The incandescent end of the stick was introduced into a jar of oxygen gas, and immediately burst into a brilliant flame.] The oxygen when it is not diluted has this wonderful effect. And so I might take paper or other combustible bodies instead of this wood. In fact I might use iron. I will produce here a flame from a mixture of oxygen and another gas called hydrogen, and I will cause the oxygen to burn, not a piece of paper or wood, but actually a piece of steel. I hold a piece of steel here in my hand. It is the spring of a watch. A man has now gone down to start the apparatus. I shall very soon have a jet of gas passing through here. I will ignite that jet of gas, and then you will see the flame of the hydrogen,—not a brilliant flame by any means. [A jet of hydrogen was then ignited.] I will presently mix with the hydrogen flame which you see a quantity of this oxygen, but I want first to raise this steel to a very high temperature, and then to allow the oxygen gas to act upon it. I will now throw into this jet of hydrogen a quantity of this wonderful oxygen. You will see that the flame becomes very much smaller; and now it is enormously hot. Observe what it can do with that piece of steel. Observe how it can burn it away. This substance called oxygen is playing upon that spring. If I take away the hydrogen you see no flame whatever, but we have only the pure cold oxygen; but when once the temperature of the steel has been raised sufficiently, the force with which the oxygen particles, or atoms as I called them, plunge down upon the steel is sufficient to produce this wonderful effect. [The watch-spring continued to burn in the jet of oxygen.]

Well now, we have the generation of heat exemplified in this way. I showed you first of all that it could be generated by friction to such an extent that you were able to melt metal with it. I then showed you that it was generated by ordinary mechanical percussion, as in the striking of two pieces of lead by the hammer. And now I ask your power of imagination to help me here in the case of the oxygen uniting with the iron or the steel, which is, to all intents and purposes, a case of percussion. It is, however, a case of the percussion of atoms, instead of the percussion of a hammer descending upon a weight. Now, I think that if you have followed me I have not uttered a word that you can not perfectly understand. You can picture before your mind these little oxygen atoms showering down with this tremendous force upon the surface of the iron; and the object I have in lecturing to you boys and girls is that you may see with the eyes of your mind those things which are too small to be seen with the eyes of your body, and that is the power I referred to in the first instance—the power of imagination.

I have here a variety of jars of this oxygen gas. I do not want to spend too much time in operating with them, but one experiment I must make because it is of such importance and such historic interest in science. The great Sir Isaac Newton, regarding whom a great deal of nonsense and a great deal of wrong has been uttered lately in the newspapers and elsewhere, operated with a diamond in the course of his experiments on optics; and he concluded from his experiments on the diamond that that beautiful gem, the hardest of all substances, was an unctuous, peculiar substance like wax or grease. Long before the experiment was ever made, this Newton by that very power which exists in every boy and girl here present, and which I called upon in the beginning of the lecture, saw that this beautiful gem was a combustible substance; and now I want to show you that Newton was true in his prediction. I have here a small diamond—(for diamonds are very precious, as you know, and it would be a wasteful expenditure, of course, to use a large one); and I will first of all heat it by means of this very hot flame that we possess here. I have there some oxygen gas, and after heating the diamond I will plunge it into the oxygen gas, and I think you will find it will there glow like a little star. Perhaps the hydrogen can not heat it strongly enough, but we will try it. [The heated diamond was lowered into a jar of oxygen.] Yes, there is the diamond burning before you. And now, how are you to figure that diamond? How are you to imagine the state of things going on there? At the present time it is surrounded by oxygen; and the oxygen atoms, as I have called them, are showering down upon the diamond, and showering down upon it with such percussive force as to render it that bright and brilliant star. Now, I think every boy and girl here present can picture before his and her mind what is going on. Imagine these atoms of oxygen showering down upon the diamond, and the force with which they do so raises the diamond to that temperature.

In all these cases heat is actually generated. There is called into existence heat which did not exist before. It is, as I have said, a kind of motion which can be generated in the way which I have indicated.

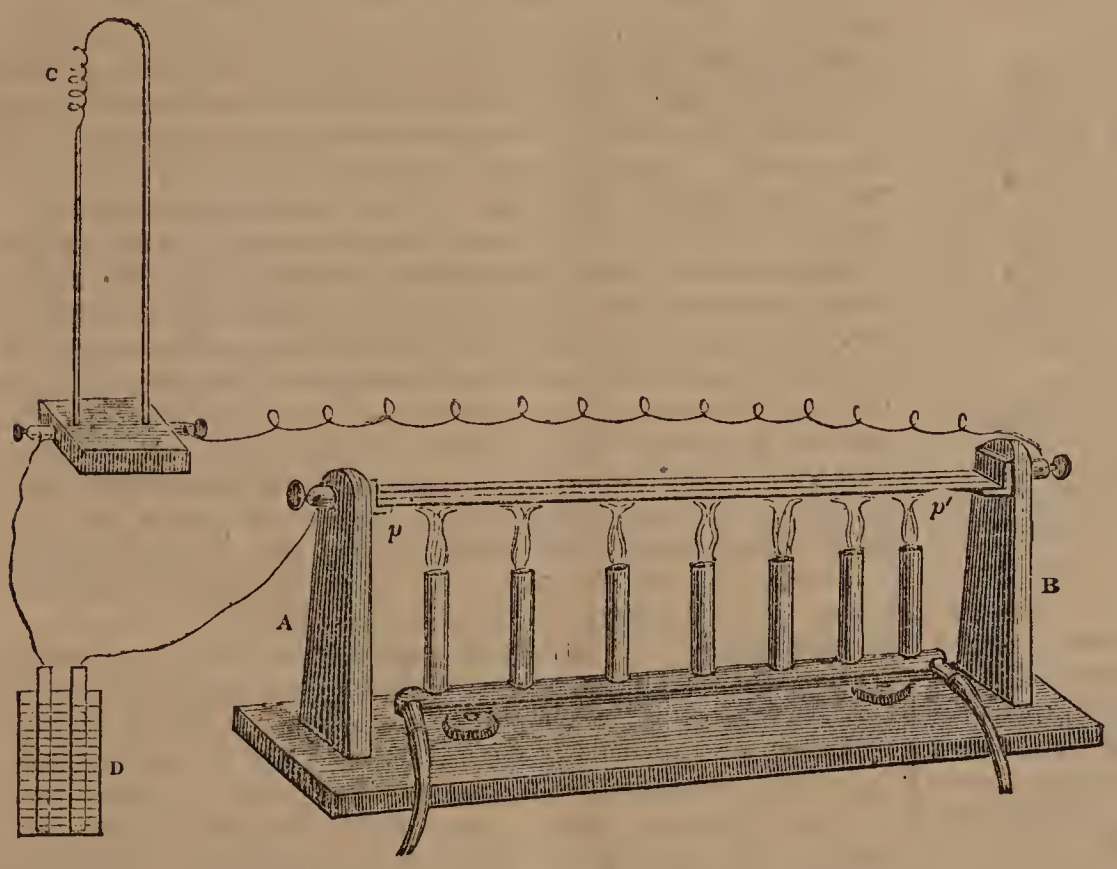
Having now obtained a general notion as to the methods in which heat is generated, we may pass on for a moment or two to investigate what it can do—how bodies are affected by it.

I have arranged an experiment here, in the front of the table, which will enable you to see what heat can do; and here again I would call upon that wonderful power of imagination. Imagine the particles of a body getting gradually warmer, vibrating with greater and greater intensity. What is the natural consequence? That these particles should force themselves asunder, that the body should become bigger by being heated, that the volume of the body should be augmented by the augmentation of its temperature. Here I have a platinum wire stretched from this stand to this. You observe that at the end I have attached a straw with a piece of paper fastened on it. Here you observe a little wheel, and from that wheel you observe a weight descending. Round the axis of the wheel a platinum wire is coiled. Now the platinum wire is pulling in one direction, and the weight is pulling in the other direction, but if you relax the platinum wire the weight will instantly predominate and the index will rise up. Observe that index rises if I relax the wire by simply pressing this rod to which one end of it is fixed; and when I take my hand away the wire remains no longer relaxed, and the index falls back again. (A great portion of what we call "experimental science" consists of devices of this kind. This was devised by my assistant Mr. Cottrell.) But how shall I heat that wire? By a power which is far away from here, which I hope to be able to talk to you about at some future time. Coming up from the yard beneath there is a power which heats the wire; it is called an electric current. When the current comes the platinum wire will be heated and elongated, and the elongation of the wire will manifest itself on the index. You see this piece of paper smoking with the heat of the wire. If I stop the current, the source of heat is detached, and the wire cools. When the wire cools it contracts, and when it contracts the index falls in this peculiar way.

I have another experiment here to show how heat operates in causing bodies to expand. I have here two bars—one of iron and the other of brass; and at the present time you see here in front of the table a little piece of apparatus the meaning of which you will understand immediately. I will show you that this wire which you see here in front is a little coil of platinum wire. But before I show you this wire I should just like to show you what a power we possess for heating the platinum wire, when we augment our current. This current comes from a battery downstairs, which I trust to have the

pleasure of explaining to you, not this year, but perhaps in some future year. Now the assistant will give me a powerful current, and I think you will see that this wire will be raised to redness throughout its entire length. [The electric current was then passed through the wire]. The platinum wire is now red hot, and the index goes up in this prompt way. You will see the glow of the red hot wire now the light is lowered. Now, if I shorten the length of wire less and less resistance is thrown in the way of the current, and a greater amount of electricity passes through, and you have the wire raised to this much greater temperature. There is one thing to be observed here. You must not allow yourselves to suppose that this apparent thickening of the wire on being heated is due to a real thickening. The red hot wire looks as thick as a quill. This appearance, which I have no doubt is visible to you, is not due to a real thickening. It is an effect produced by a bright light on the eye. A bright body is always seen larger than it ought to be, and this particular wire now before you is seen thicker by those in more distant parts of the theatre than it is by those near at hand. This proves that it is a deception of the eye—a kind of illusion called "irradiation." It is not a real thickening. [The platinum wire was still further shortened and then parted asunder]. There, the wire is now fused by this electric current.

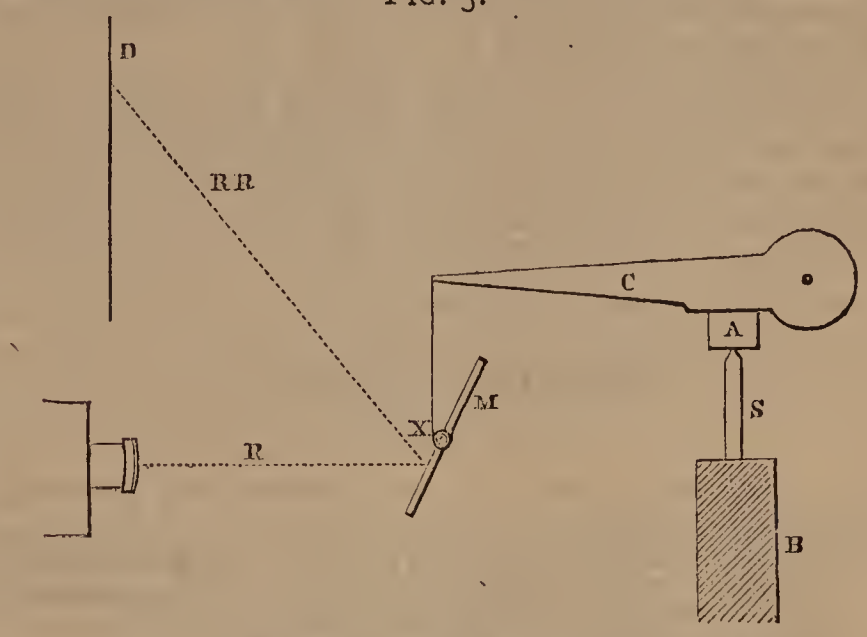
FIG. 2.



Now, I will call back your attention to this spiral C which you see here. Here on one of these supports A B is a piece of brass *p*, and here is another *p'*; and stretching across from support to support are two bars, one of brass and one of iron. At present they are not long enough to span the distance from one support to the other; but I will heat them, and then they will expand, and you will find that when they expand sufficiently to bridge this chasm from one support to another an electric current will pass, and then that spiral C will be like a voice telling us that the bars have expanded from one support to the other. We will now light the jets of gas underneath these bars, which at present are too short to span the distance between the supports. [After an interval]—Observe now that what I predicted a moment ago has occured. The spiral is now ignited. If I remove this brass bar the spiral sinks. What I want to show you by this experiment is that the brass expands more than the iron. It was the expansion of the brass which bridged the chasm across.

I have told you that a great portion of experimental science is taken up by devices of this kind to render these small expansions evident. I think there is before you on the floor in front of the table a piece of apparatus more delicate than any that has ever yet been made. It is an apparatus intended to show, among other things, the expansion of volume by heat. You will understand this apparatus immediately by reference to this small sketch that I have drawn upon the black board. I have taken simply the essential parts of the apparatus, and you will understand them, I am sure, perfectly well.

FIG. 3.



The bottom part B of the sketch represents the upper end of that upright bar of metal which you see between those two brass pillars in the apparatus in the middle of the room. On the top of this bar rests a little brass stem S; and the top of that stem is pointed and presses upon a very hard flat stone—a plate of agate A. Now, conceive the top of this bar to be lifted, and to push this stem up against the plate of agate. What will occur? You see the arm C above the piece of agate. That arm moves upon a pivot which you see marked by a dot; a very little pushing of this arm causes it to move through a greater space than the body which pushes it. Now, attached to this arm is a piece of the hair-spring of a watch, and that is carried round an axis X, attached to which axis is a piece of looking-glass—that is, a mirror M. Upon that mirror a beam of light R is cast. The figure at the left of the sketch I suppose to be the front part of a lamp from which the light will issue. The beam of light will fall upon that mirror, and will be reflected upwards RR, and will mark itself as a spot of light upon the screen D. Now, if you conceive the end of the bar to be lifted, and to push the arm upwards, it will cause the axis of the mirror to turn round, and cause the mirror to take another position; and when the mirror takes another position, this beam of reflected light will travel with the mirror, and will travel with twice the velocity of the mirror. Thus, in this experiment, instead of having a straw for an index, I use a beam of light. You will understand the apparatus when I make the experiment. I think, as I have said, it is the most delicate instrument of the kind that has ever yet been made. Now I will try and get the apparatus in proper order for showing the experiment. I throw a beam of light upon the mirror, and there you see it reflected and quivering on the wall. I will bring it down so as to get it on the screen. You see it is exceedingly sensitive. That constitutes our index. And now I will ask you to observe what I am going to do. I will not touch that heavy bar of lead; I will not heat it with a flame; I will simply breathe against it; and I believe that this apparatus is so exceedingly delicate that the mere breathing against this mass of lead (and it is very large) will cause the lead to expand upwards, and

will bring down that spot of light from the top of the screen to the bottom. [The lecturer then breathed on the bar of lead, and the image of the beam of light gradually travelled down the screen.] The mere warmth of the breath is sufficient to produce this effect. Now I will pour upon the bar a little liquid that will chill it—make it cold; and I think you will find that as the bar cools it will contract, and that the beam of light will go back to the top of the screen. [The spot of light was successfully brought back to the upper part of the screen in the manner described.]

So much for these actions which this wonderful t hing called heat produces. In our next lecture we shall endeavour to understand how this wonderful thing can be measured. We shall deal with the construction of thermometers and things of that kind, and I trust we shal l get to know a great deal about t hem.

FOREIGN SCIENCE.

PARIS, JAN. 1, 1868.

New minerals accompanying cryolite.—Fluosalts of antimony and arsenic.—Preparation of indium.—Electrical jewels.—Estimation of nicotine in tobacco.—ACADEMY OF SCIENCES: Capillary Action—Solar Spots—Synthesis of nevrine—Action of hypochlorous acid on essence of turpentine and camphor—Volumetric estimation of nitrogen—Gallic fermentation—Amalgamation of voltaic piles.

AMONG the many valuable and very interesting researches that have recently been made in organic chemistry in this portion of the scientific world, there are some interesting facts in mineral chemistry for your correspondent to mention. M. Hagemann has discovered two minerals accompanying cryolite, they have been named by him *dimetric pachnolite* and *arksutite*. The first resembles the pachnolite described by M. Knofs; it occurs in prisms or in quadrangular pyramids, cleavable in the direction of the base, of a pinkish white colour and very brilliant. Its density is from 2·74 to 2·76, and its hardness the same as cryolite. Sulphuric acid easily attacks it. The specimen analysed contained 2 per cent of silica, which M. Hagemann considers foreign to the composition of the mineral, to which he assigns the formula,

$$Al_2Fl_3+2(\tfrac{2}{3}Ca+\tfrac{1}{3}Na)Fl+2HO$$

Arksutite is granular, white, and crystalline, and, like the other mineral, very brilliant. Its density is from 3·03 to 3·17; its hardness is equal to cryolite. At a dull red heat it fuses, without loss of water. Analysis gave numbers corresponding to the formula $2(Ca,Na)Fl+Al_2Fl_3$. These two minerals occur at Arksut-Fiord in South Greenland, and are probably the result of the decomposition of cryolite.

M. Marignac has made an elaborate research upon the fluo-salts of antimony and arsenic; some of his results will be mentioned in a future letter.

M. Richter has published a method of extracting indium from zinc: the rare element occurs in blende. The zinc is dissolved in sulphuric or hydrochloric acid, and the residue, which is composed of zinc, indium, and other metals, is treated with nitric acid. The solution is evaporated with sulphuric acid, diluted, and a current of sulphuretted hydrogen gas passed through. The indium is almost completely precipitated with the cadmium and copper. The precipitate is dissolved in hydrochloric acid, and precipitated by ammonia. By repeatlng the process several times the whole of the zinc and cadmium is separated. Finally, the small quantity of iron still mixed with the indium is removed by a partial precipitation with ammonia and carbonate of soda. Indium is obtained by reducing the oxide; this may be effected by heating in a current of hydrogen gas, or by the power of a voltaic battery.

A curious application of electricity has been made by a jeweller in the Rue Therèse, M. Trouvé. He makes scarf-pins, &c., with heads upon them which at the will of the wearer move their eyes. They are delighting fashionable Paris. The electro-motor is usually carried in the waistcoat pocket. It is formed of one couple, either zinc and carbon or zinc and platinum. The carbon is fixed in the vessel which holds the exciting liquid—a saturated solution of sulphate of mercury—there being an outer case in which this vessel is placed. The zinc is fixed to the lid of the case, and does not plunge into the liquid, which only fills the lower half of the vessel. So long therefore as the apparatus is in an erect position, there is no action, but when placed horizontally the current is formed. The whole apparatus makes a little case of the most trifling size. A scarf-pin with electro-motor and connections, costs from 60 francs upwards.

A process for the estimation of the nicotine contained in tobacco has been devised by M. Liecke. He exhausts the dry tobacco leaves with water acidulated with sulphuric acid, renewing the water three times, and evaporates the solution just to the consistence of an extract. This extract is treated with an equal volume of alcohol, the alcoholic solution filtered, and the residue washed with alcohol. The alcoholic solution contains all the nicotine as sulphate. The solution is evaporated, and the residue obtained from it decomposed by caustic potash in a retort heated by oil to 260° C., the nicotine being received in dilute sulphuric acid.

At the meeting of Academy of Sciences held on the 16th December, besides the memoirs, of which abstracts are given in this letter, there was a note relating to a particular effect of capillary action, from M. Definis; and from M. Kirchhoff a communication on solar spots. M. Wurtz presented a memoir entitled *Synthesis of Nevrine*,* this will doubtless possess great interest for scientific chemists. M. Adolph Wurtz has in fact succeeded in the synthesis of one of the proximate principles of the brain. M. Liebrich in 1865 obtained from the brain a crystallisable definite compound containing phosphorus and nitrogen, to which he gave the name of protagon. By acting upon this body with strong baryta water, phosphoglyceric acid, and a powerful base which he named nevrine, were obtained. M. Bayer has recently demonstrated that nevrine is an oxethylenic base, being in fact hydrate of oxethyl-ammonium in which three atoms of hydrogen are replaced by three groups of methyl; it is therefore hydrate of oxethyl-trimethyl-ammonium.

This fact led M. Wurtz to suppose that the synthesis of nevrine might be effected by treating hydrate of oxethyl-ammonium (formed by acting upon ammonia with oxide of ethylen) with iodide of methyl. This reaction was only partially successful, since it yielded only small quantities of the base in a state of purity. M. Wurtz succeeded, however, in performing a beautiful synthesis by another process which he has indicated for the preparation of oxethylenic bases—the treatment of monochlorhydride of glycol by ammonia. The chloride of the base nervine, *i.e.*, the chloride of oxethyl-trimethyl ammonium, is formed by the direct addition of the elements of mono-chlorhydride of glycol and trimethylamine.

$$C_2H_4\begin{Bmatrix}OH\\Cl\end{Bmatrix}+\begin{matrix}CH_3\\CH_3\\CH_3\end{matrix}\Big\}N=\begin{matrix}CH_3\\CH_3\\CH_3\\C_2H_4(OH)\end{matrix}\Big\}NCl$$

5 grammes of trimethylamine are heated in a sealed tube, by a water bath, with 10 grammes of chlorhydride of glycol. At the end of 24 hours the tube is allowed to cool, when beautiful prismatic crystals, perfectly colourless, make their appearance. The crystals dissolve easily in boiling absolute alcohol, and they are partially deposited on cooling from concentrated solutions. Ether precipitates this solution, but if the liquid contains a trace of water, a heavy thick liquid precipitates instead. The crystals are chloride of oxethyl-trimethyl-ammonium, which is a very

* M. Wurtz proposes the word nevrine as the correct translation o the German neurine.

deliquescent compound. When to a solution of this chloride a moderately concentrated solution of chloride of gold is added, a precipitate of a pure yellow colour is formed,—the double chloride. This precipitate has been shown by M. Bayer to be characteristic of nevrine. It is soluble in boiling water, the solution depositing little yellow needles. M. Wurtz has compared his aurochloride prepared from the artificial nevrine with the compound obtained from the brain substance. The crystals of the two salts under the microscope exhibit rhomboidal plates; they are identical save in the size of the crystals.

A solution of chloride of platinum added to a concentrated solution of chloride of oxethyl-trimethyl-ammonium, causes no precipitate, and no crystals are deposited upon concentration to a syrupy consistence, but addition of alcohol causes a precipitate which by analysis gives 31·8 per cent of platinum. The formula,

$$(CH_3)_3,(C_2H_4OH)N,Cl+PtCl_2$$

contains 31·8 per cent Pt.

Chloride of oxethyl-trimethyl-ammonium by the action of moist oxide of silver, is decomposed, hydrate of oxethyl-trimethyl-ammonium being set free. By evaporating the solution, a syrupy liquid is obtained, which upon heating evolves a strong odour of ammonia.

M. Wurtz thinks that the mode of formation, and the analyses he has made, leave no doubt as to the compound being really nevrine.

The eminent author of the preceding paper presented a note, by M. C. G. Wheeler, on the action of aqueous hypochlorous acid on the essence of turpentine and on camphor.

Essence of turpentine, by the action of aqueous hypochlorous acid, is converted into a yellow viscous liquid, probably a mixture of the bi- and trichlorinated compounds; at the same time another product is formed which is retained by the water. This can be completely separated by agitating the aqueous solution with ether, in which it is very soluble, and separating the etherial solution and distilling. The residue is a yellow syrupy substance, neutral, very soluble in ether and in alcohol, and slightly soluble in water. Analysis shows this compound to be $C_{10}H_{18}Cl_2O_2$. This chlorhydride cannot be distilled without decomposition, hydrochloric acid is lost in the operation. Nitric acid oxidises it, producing a resinous substance. The whole of the chlorine is removed with great difficulty, the author failed when acting upon it with sodium during several hours. He obtained in this way an acid which appeared to have the composition $C_{10}H_{16}O_3$, but the quantity was too small to admit of a decisive examination.

Camphor added little by little to hypochlorous acid is liquefied and falls to the bottom of the vessel. After a time, especially if agitated, it forms a solid mass presenting the appearance of camphor itself. By two or three crystallisations from alcohol, this product may be obtained pure. It is mono-chlorinated camphor $C_{10}H_{15}ClO$, and is formed according to the following equation :—

$$C_{10}H_{16}O+ClHO=C_{10}H_{15}ClO+H_2O.$$

Mono-chlorinated camphor is a white body, indistinctly crystalline, soluble in ether and in alcohol, and nearly insoluble in water; it crystallises much better from alcohol containing a little water, than from absolute alcohol.

It melts at 95°, and is decomposed at a temperature approaching 200°, emitting vapours of hydrochloric acid.

By treating mono-chlorinated camphor with a solution of alcoholic potash at a temperature approaching 80° during six or eight hours, the author obtained products containing no chlorine; one of them he has been able to separate with certainty, its composition is $C_{10}H_{16}O_2$. It is isomorphous with the camphinic acid of M. Berthelot. The author gives to this compound the name Oxycamphor; it crystallises in white needles, soluble in alcohol and insoluble in water; it fuses at 137°, and sublimes without decomposition, yielding fine crystals. The odour resembles that of camphor.

M. Pratt, at the meeting of the Academy on the 25th, addressed a memoir on a general method for the volumetric estimation of nitrogen in its various combinations, and on a new process for the preparation of this gas in a state of purity in laboratories. M. Van Tieghem sent a communication upon the Gallic fermentation. Your correspondent promises some account of these in a future letter. There was also a note on the amalgamation of voltaic piles by M. Demance. If M. Demance says his process is new, one might justly add, query. The process is simply the placing of metallic mercury in the cell, when, by the galvanic current, the zinc becomes amalgamated. The explanation of the manner in which this is effected, as given by him, is certainly not without interest. He has found that there is no previous conversion of the mercury into a salt, that in fact the action is nothing else than a transference of metallic mercury. Furthermore, the amalgamation only takes place under the influence of the current.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Colouring Matter of Saffron.—B. Weiss has re-examined the colouring matter of saffron (polychroite) and finds that the discrepancies in the statements of former investigators are due to the ease with which the dye decomposes during the processes of purification. The dye seems to be a glucoside; on being treated with sulphuric acid it splits up into a secondary colouring matter, named by the author crocin, sugar, and an essential oil. The composition of crocin is $C_{32}H_{18}O_{12}$, that of the oil $C_{20}H_{14}O_2$.—(*Journal pr. Chem.* ci. 65).

Cement.—Sorel describes a new cement which he prepares by mixing magnesic oxide with a more or less concentrated solution of magnesic chloride. The hardness of the cement increases with the strength of the solution; 20 to 30° Baumé is found most suitable. Its binding power is greater than that of any other cement, it being capable of producing hard blocks with more than twenty times its weight of sand or other inactive material.—(*Comptes R.* lxv. 102).

Manganese, New Compounds of.—T. Niklès Fluormanganous acid is formed by adding fluorhydric acid to an ethereal solution of manganic perchloride, or by dissolving in the concentrated acid manganic dioxide. The reactions of fluormanganous acid are similar to those of perchlorides. Alkalic fluorides produce rose-coloured precipitates of double fluorides. It likewise combines with organic bases. All these compounds are of comparatively unstable nature, most of them being gradually decomposed when in contact with much water. On adding manganic perchloride to a boiling solution of potassic or ammonic fluoride, precipitates are formed which may be considered as salts of oxyfluormanganous acid, Mn(OFl).—*Comptes R.* lxv. 107).

Mimetesit, Artificial Preparation of.—By following the general method adopted by Deville and Caron for the preparation of crystallised chlorophosphates, G. Lechartier has succeeded in obtaining corresponding chlorarseniates. The arseniate and chloride of the same base are fused together, and after cooling the excess of chloride is dissolved out by water, which leaves the crystallised chlorarseniate behind. Mimetesit, $3(AsO_5, 3PbCl)PbCl$ amongst other compounds, has thus been obtained).—(*Comptes R.* lxv., 172).

Phenolsulphuric Acid, Salts of.—E. Menzner. Phenolsulphuric acid was prepared by heating equal equivalents of phenyl and sulphuric acid on the water-bath, diluting, 24 hours later, with water, neutralising with plumbic carbonate, decomposing the lead-salt with sulphuretted hydrogen, and concentrating the dilute solution first by applying heat and finally in the dessicator over sulphuric acid. The salts are obtained by neutralising the free acid with oxides or carbonates. They are all soluble in water, mostly well crystallised, contain, with the exception of the ammonic salt, water of crystallisation, which they lose at 140° C. The plumbic and cupric salts decompose when heated to that temperature. The stability of the acid, which in aqueous solution bears continued boiling, and that of its salts, support the supposition that its constitution is different from that of the normal sulphovinic acids.—(*Ann. Chem. Pharm.* cxliii. 175).

Chlorsalylic Acid, Preparation of.—Glutz. Gaultheria-oil and phosphoric perchloride in the equivalent proportion of 1 to 2 are mixed together in a large well-cooled flask. The reaction, at first very violent, is completed by heating on the water bath for several hours. The flask is then connected with a reversed Liebig's condenser, and the boiling continued for a day. After this treatment the (now liquid) products of the reaction are distilled, and the fraction going over above 220° C., poured into a large quantity of boiling water. This dissolves, under evolution of chlorhydric acid, all, except chlorsalylic trichloride, which remains as a heavy brown oil (a solid cake when cold) at the bottom of the vessel. On cooling the chlorsalylic acid crystallises out from the aqueous solution in white needles.—(*Ann. Chem. Pharm.* cxlii. 194).

Cyanacetic Acid.—Th. Meves. Pure cyanacetic acid was prepared in the following manner:—250 grm. of monochloracetic ethide, 300 grm. of potassic cyanide, and 1,200 grm. of water were heated on the oil bath in a retort connected with a reversed Liebig's condenser until the smell of prussic acid had disappeared; an excess of the ether was then distilled off. The dark brown liquid thus obtained was then neutralised, evaporated to half its original volume and filtered, the filtrate further concentrated, and after addition of an excess of sulphuric acid, extracted with ether. The etherial solution on evaporation leaves the crude acid, which is purified by being converted into the lead salt, and the latter decomposed by sulphuretted hydrogen. The salts of this acid, of which the potassic, baric, zincic, cupric, argentic, mercuric, and plumbic, are described, are very soluble in water, with the exception of the two last named. They are obtained either by saturating the free acid with the oxides or by mutual decomposition of ammonic cyanacetate with the the neutral metallic salt of the desired radical. (*Ann. Chem. Pharm.* cxviii. 201).

Toluol, Substitution Compounds of.—F. Beilstein and A. Kuhlberg. Of the four possible isomeric trichlortoluols, $\overset{\ominus}{C}H_2Cl_3(\overset{\ominus}{C}H_3)$, $\overset{\ominus}{C}_6H_5(\overset{\ominus}{C}Cl_3)$, $\overset{\ominus}{C}_6H_3Cl_2(\overset{\ominus}{C}H_2Cl)$, and $\overset{\ominus}{C}_6H_4Cl(\overset{\ominus}{C}HCl_2)$, the first (trichlortoluol) and second (benzoterchloride) are already known. The third, dichlorbenzylic chloride, is formed by passing a current of chlorine through benzylic chloride containing iodine, or through boiling dichlortoluol. It boils at 241°C., loses *one* atom of chlorine on being treated with alcoholic potassic hydrate. The fourth, chlorbenzylalic chloride, is obtained by passing chlorine through benzylalic chloride (chloride of oil of bitter almonds) charged with iodine, or through boiling chlortoluol. It boils at about 221°, and contains only *one* atom of chlorine firmly attached. The boiling points of the 4 isomers are respectively—235°, 241°, 221°, 218°. Benzylalic chloride dissolves in concentrated nitric acid with formation of a nitro compound, which on being treated with chromic acid is converted into *nitro-benzoic* acid, while the analogous chlor-benzylalic chloride under similar conditions forms *para* chlorbenzoic acid).—*Zeitschr. Ch.* N. F. iii. 513).

Phenylic Acid, and Derivatives of.—Glutz. Phenylic chloride is obtained by heating together in equivalent proportions carbolic acid and phosphoric pentachloride. The phenylic chloride is distilled off and freed from phenol by shaking the mixture with a solution of sodic hydrate. The residue, after distillation, consists chiefly of neutral phenylic phosphate. The boiling-point of the chloride after purification is 130° C. When heated with an excess of sulphuric acid to 100° for several hours, chlorphenyl sulphuric acid is formed, which is purified by being converted into lead salt, and separated again from the metal by means of sulphuretted hydrogen. It is a syrupy liquid which slowly crystallises under the dessicator. It is moderately soluble in alcohol, insoluble in ether. The composition of the lead salt, which crystallises well, as do most salts of this acid, is

$$\left(C_{12}\begin{Bmatrix}H_4\\Cl\end{Bmatrix}\right)[S_2O_4]O.PbO$$

Chlorphenylsulphuric acid is converted into phenylsulphuric acid by the action of sodium-amalgam. The lead salt, treated with strong nitric acid, is partly converted into nitrochlorphenylsulphate, partly into nitrochlorphenyl,

$$C_{12}\begin{Bmatrix}H_4\\Cl\\NO_4\end{Bmatrix}$$

The latter may also be obtained from phenylic chloride.

Phenylic phosphate $(C_{12}H_5O)_3\,PO_5$ is insoluble in water, readily soluble in alcohol, ether, and hot sulphuric acid; it is converted into diphenolphosphoric acid when acted upon by strong bases. When heated with bromine in sealed tubes to 180° the neutral ether gives rise to the formation of a white crystalline body of the composition $(C_{12}(H_4Br)O)_3.PO_5$, the reaction being represented by the following equation,

$$(C_{12}H_5O)_3.PO_5+6Br=\left(C_{12}\begin{Bmatrix}H_4\\Br\end{Bmatrix}O\right)_3.PO_5+3HBr$$

Ann. Chem. Pharm. cxliii. 181.)

CORRESPONDENCE.

CRYSTALLISATIONS PRODUCED BY THE BLOWPIPE.

To the Editor of the Chemical News.

Sir,—With reference to the miscellaneous memorandum under the above head in your journal of the 27th Dec., 1867, in which it is stated that the sudden opacity of beads, of borax, P. salt, or soda, is found by M. G. Rose to be due to crystallisation of contained substances, allow me to state that the fact has been long known, and is to be found in the works of Berzelius, Plattner, and other reliable writers on the blowpipe. Berzelius says (page 64) :—"Titanic acid combines with soda with effervescence, and forms a clear dark green glass. This glass has the property of crystallising exactly at the moment that it ceases to be ignited. . . . This property is common to all bodies which crystallise at a very high temperature, as, for instance, phosphate of lead." Again, of apatite, Berzelius says (page 214):—"It is dissolved in large quantity by the salt of phosphorus to a transparent glass, which, when nearly saturated, becomes opaque on cooling, and acquires a crystalline appearance, less distinct, however, than that of phosphate of lead."

Plattner alludes to the same fact in more than one place.—I am, &c.,

W. A. Ross.

Woolwich, Dec. 28, 1867.

SUPPLEMENT
TO
THE CHEMICAL NEWS.
VOL. XVII. No. 422.

NITROGLYCERINE OR GLONOINE.

THE awful accident which lately took place at Newcastle-on-Tyne, caused by the sudden explosion of nitroglycerine, or Nobel's patent blasting oil, has induced me to collect together, from various sources, chiefly published abroad, the following particulars in respect of this substance, and as many of the leading daily London papers have in various ways given accounts about nitroglycerine which are incorrect, I venture to hope this paper will not be found by yourself, Mr. Editor, and your many readers quite uncalled for.

Nitroglycerine was discovered by the well-known Dr. Sobrero, now Professor of the Technical Institute at Turin, somewhere about 20 years ago. The substance was studied simply in a scientific interest by Dr. J. E. de Vrij, the chemist of the Netherlands Indian Government, well-known for the analysis of this and testing of the Cinchona bark, and also by Dr. Gladstone, and of late by Dr. Kopp. Up to the end of 1864 nitroglycerine was not only not familiarly known, nor to be had in quantity in commerce, but continued to belong entirely to the domain of science. This may easily be accounted for by the fact that glycerine itself is only in use and to be had on the large scale since the last 8 or 10 years. When pure, nitroglycerine is a liquid of from 1·525 to 1·6 specific gravity; it has no odour, is often colourless, or yellowish, has a sweet, pungent, aromatic taste, and is powerfully poisonous. It is only very slightly soluble in water, readily so in ether, alcohol, and methylated spirits; it does not inflame when touched with a light, nor does it explode by being so touched, but concussion, touching with a red-hot iron, or the concussion due to the explosion of gunpowder, and, better yet, detonating mixtures, and fulminates, sets off the nitroglycerine. According to Dr. Johann Rudolf Wagner, the well-known technologist to the Bavarian Government, nitroglycerine may be cooled down to 4° Fahr. without becoming solid; but it appears after all that the nitroglycerine of commerce, if exposed for a continued period to 46·4° Fahr., becomes solid, crystallising in long needles, which are most dangerous to handle, since they explode, even on being gently broken, with a frightful violence. At 320° Fahr. the nitroglycerine begins to decompose, giving off red vapours, and if the heat be suddenly applied, or slightly raised above this point, the substance explodes instantaneously, and with great violence, shattering even open vessels to atoms. Nitroglycerine may be assumed to consist of anhydrous glycerine, in which 3 atoms of hydrogen have been replaced by 3 atoms of NO_4. The products of the complete combustion of 100 parts of pure nitroglycerine are the following:—

Water	20
Carbonic acid	58
Oxygen	3·5
Nitrogen	18·5
	100·0

Since the specific gravity of nitroglycerine is 1·6, one volume, say 1 cubic inch of the material, yields on combustion or explosion—

Aqueous vapour	554	volumes, or bulk.
Carbonic acid	469	,,
Oxygen	39	,,
Nitrogen	236	,,
	1298	,,

According to Nobel, these gases expand on explosion to 8 times their bulk. 1 cubic measure volume of nitroglycerine will, therefore, give 10,384 cubic measures of gases; while 1 cubic measure of gunpowder will only yield 800 cubic measures of gases. Hence it follows that for equal bulks nitroglycerine is 13 times stronger than gunpowder, while by equal weights the former is 8 times stronger than the latter.

The danger of the use of nitroglycerine is greatly enhanced by the instability of this compound; even when pure it is affected by increase of temperature, and at from 68° to 75° Fahr. it is prone to incipient spontaneous decomposition, accompanied by a slow but sufficiently strong escape of gaseous compounds, which, while exerting a slight pressure on the vessels the liquid is contained in, also can cause the fluid to explode on the slightest concussion. During the slow and spontaneous decomposition of the glonoine there are formed divers products, among these glyceric, oxalic and hydrocyanic acids, and ammonia, and others unknown. Nobel's patent nitroglycerine, or blasting oil, is made in the following manner:—To 13·5 parts by weight of strong sulphuric acid is added 1 part by weight of nitrate of potash of best quality, and this mixture cooled down to 32° Fahr., the result of which is the crystallising out of a salt which contains 1 equivalent of potash, 4 equivalents of sulphuric acid, and 6 equivalents of water; the strongly acid liquid is decanted from the crystals, and to the liquid, commercial glycerine is added, taking care to keep the liquid cold; the ensuing nitroglycerine is separated from the acid by water, once washed with fresh water, and is fit and ready for use.

I may here observe that the manufacture of the substance which has already given rise to so much mischief, is carried on in the free City of Hamburgh, which is not subject to any of the laws which in other closely adjacent countries would render the manufacture of the nitroglycerine, if not entirely illegal, at least subject to very stringent but equally justifiable police supervision. In France, Switzerland, Belgium, and the Netherlands, where the French law of 1810, *régulant les métiers insalubres et dangereux* is yet in force, the manufacture of this article can be prohibited. The best mode of manufacturing nitroglycerine where it is desirable to use it, and that is the case in open quarries where one has to deal with tough hard rock, is to make it extempore on the spot where it is to be applied. Take a sufficient quantity of strong nitric acid, density from 1·4758 to 1·4902, mix therewith the double of its weight of strong sulphuric acid, weigh off 3300 grammes of the acid mixture when quite cool; take 500 grammes of glycerine, which must be free from either lime or lead salts, and mix the same cautiously with the acid while keeping the mixture very cool by constantly stirring. Let the mixture stand quietly for about 10 minutes, and then pour it out in from 5 to 6 times its bulk of cold water, taking care well to stir the same all the while. The nitroglycerine will sink to the bottom; the dilute acid is removed by decantation, the nitroglycerine once more washed with water, when it would be fit for use after removal of the latter. The glycerine to be used should have a specific gravity of from 1·2459 to 1·2562 *i.e.*, contains from 94 to 96 per cent real glycerine. Dr. Gladstone has found while engaged with his researches on nitroglycerine, that the perfectly anhydrous glycerine did *not* yield, when treated with the mixture of nitric and sulphuric acids, an explosive compound, but, on the contrary, one which when touched with a flame, or red hot metal, burns off pretty quietly. Impure nitroglycerine is dangerously self-explosive, even while standing quietly.

From the evidence brought forward at the coroner's inquest at Newcastle-upon-Tyne, it appears that the nitroglycerine which had been brought to that town certainly contained some methyl-alcohol, or methylated spirits of wine, not in sufficient quantity to render the nitroglycerine safe; in fact it is not in the interest of the makers of the article to render it so, as this would evidently occasion the trouble of having to separate the nitroglycerine from its

solution by the addition of water, previous to being fit for use, and this trouble would be undoubtedly disliked at the mines and quarries, and the use of the article therefore discarded.

From the evidence, given at the inquest just alluded to by I. L. Bell, Esq., the well-known proprietor of large chemical works, collieries, and iron mines, in the county of Durham, it appears that the printed instructions issued by M. Nobel, of Hamburgh, the manufacturer of the nitro-glycerine, contain statements which are very contrary to fact, and apt to mislead, and hence give rise to serious mischief and grave danger, while the instructions omit to mention how to deal safely with the article when congealed. The quantity of nitroglycerine, 30 canisters, originally brought to Newcastle in May last, and stored in a public-house under the semblance of dirty grease, was equal to 4·5 tons of powder, and would, according to the pamphlet of M. Nobel, have sufficed to blow down 115,000 tons of solid rock. Mr. Bell stated that he had caused the use of nitroglycerine to be discarded in his mines on account of the injury to the health of the workmen. The inducement to the use of this substance is that, as compared with gunpowder, its effect in blasting is far greater, with less previous labour, and that its use loosens large blocks of rock or stone without crumbling, thus enabling miners and quarrymen to extract large masses of stone easily and without damage. The Messrs. Webb and Co., of Carnarvon, Wales, are the sole agents and consignees of Nobel's patent blasting oil in this country. They also have on hand an article made by Nobel, and sold under the name of dynamite, or patent safety blasting powder, 7 times stronger than ordinary gunpowder. It is stated that dynamite will not explode either from a spark or concussion alone, but requires the combined effect of both. In a compressed state it may be fired under water equally well as nitroglycerine. What that dynamite is made or composed of I have not been able to find out; I never saw a sample of it, nor find it mentioned otherwise than in advertisements.

A. Adriani.

Mr. Nobel has written to defend his blasting oil against many of the accidents which have been laid to its charge, he also denies that it possesses some of the properties that have been ascribed to it.

The following almost ludicrous examples, tending to show that the accidents have chiefly arisen from ignorance, are detailed with several others in a list of minor disasters which have resulted from its use:—

"In five cases congealed nitroglycerine has been melted purposely over fire.

"In three cases a red-hot poker has been inserted into the oil in order to melt it.

"In one case a man took to greasing the wheels of his waggon with nitroglycerine, knowing what it was, and it went all right until it struck hard against something, and the wheels went to picces.

"In one case it was burnt in a lamp as an improvement on petroleum.

"In these days every mischief is charged to nitro-glycerine. Thus, we read in the *Northern Evening Express* that recently a box with nitro-glycerine exploded at a railway station in the city of Berlin, 'and that the simple act of placing it in the van caused it to explode.' It is a proved and confirmed fact that it was fulminate of mercury that exploded."

Nitro-glycerene, Mr. Nobel says, has been accused of spontaneous combustion, but the truth is that unless properly purified, it emits a nitrous odour and will gradually decompose during some years. The nitro-glycerine, however, now made by him is always pure, he writes "chemically pure"; it is obtained by crystallisation from wood naphtha.

"Nitro-glycerine is also charged, and all the world believes it, with being extremely dangerous, even from the scratch of a needle, when congealed. It is a mere fable. It is the nature of every explosive to be more sensible to concussion in its liquid than solid state, since bodies, as a rule, are possessed of greater stability at a lower temperature. As regards nitro-glycerine, the congealed crystals, to be exploded, require a far more potent blow than the liquid oil, and it was probably owing to adhering drops of the latter that the Newcastle explosion took place. A crystal thrown with great violence against a stone or iron plate is crushed without exploding, and a strong percussion cap, when inserted into it, produces the same effect. In the mines of Kœnigsgrube (Silesia) a large lump of congealed oil was hurled by an explosion against the rock, and dropped harmlessly to the ground."

Another accusation cited, and refuted, is that of nitro-glycerine exploding by contact with oil of turpentine. Mr. Nobel maintains that nitro-glycerine is a substance of uniform composition and very reliable.

There is unquestionably much truth in many of the remarks made in this letter and we thoroughly acquiesce in the following statement.

"Whenever an article can be regularly manufactured it may be regularly used, and accidents are only the result of inexperience—the want or neglect of instruction."

NOTICES OF BOOKS.

First Principles of Modern Chemistry. A Manual of Inorganic Chemistry for Students and for use in Science Classes. By U. J. Kay Shuttleworth. London: John Churchill and Sons. 1867. (Pp. v. and 214).

This little book is mainly founded on Dr. Williamson's lectures at University College in the session 1864-5, and on those delivered by Dr. Frankland at the Royal College of Chemistry in the following winter. It was originally intended to supply the want of a strictly elementary manual for the use of science classes—a want, however, which, in our opinion, has had no real existence since the appearance of Professor Roscoe's excellent "Lessons in Elementary Chemistry." The great and rapidly-increasing popularity attained by Dr. Roscoe's book is no less an indication of the reality of the want of a manual of this character, than a measure of the success with whicht hat want has been met. Already the book has been translated and avourably received in Germany, and we understand that Professor Beilstein is about to prepare a Russian edition for the use of his students at St. Petersburg. Without laying claim to any great degree of originality, the author of the book before us has attempted to indicate how the study of the non-metallic elements may be facilitated by the aid of modern theories, and thus the student's early steps rendered less tedious and more suggestive than they commonly are. Dr. Frankland's system of notation is employed throughout the work, together with Dr. Crum Brown's method of graphic formulæ; the author considering that the advantages of the former ought to insure its universal adoption, whilst what is sometimes urged as a fundamental objection against the use of the latter, namely, that students are prone to regard graphic formulæ as physical arrangements of the atoms, he believes not to be warranted by the experience of those who have given the method an impartial trial.

No detailed directions for manipulation are given, the author justly considering that such directions are seldom very intelligible, except when given orally in presence of the objects used. Practical study in a laboratory should invariably accompany a course of reading in chemistry although the manifest advantages of such method of

study have hitherto, in the system of cram so much in vogue, been too frequently lost sight of. In the few instances in which they have been attempted the detailed descriptions of apparatus are fairly given. The author, however, has erred with other compilers of chemical manuals, when describing the method of determining the composition of water by volume (p. 83) in ascribing the invention of the pear-shaped vessel with its elaborate system of screws, glass stoppers, brass and glass stopcocks, &c., to Cavendish. True the apparatus here referred to is the one selected by the Cavendish Society as their emblem, and appears on the title pages of its publications, but withal Cavendish never employed such an instrument.

The apparatus, as described by him in his memoir in the *Philosophical Transactions* consisted simply of a glass globe provided with a stopcock, wires for the passage of the spark, and an arrangement for suspending it to the beam of the balance. The eudiometer figured in tho pages of its publications (and in Mr. Shuttleworth's book) represents the instrument as constructed at the period of the formation of the Society, and not as it was actually employed by Cavendish.

Before entering on the more special part of the subject two chapters are devoted to a consideration of such of the principles of physics as may be deemed indispensably necessary to the student. The author believes these chapters to contain nearly all the knowledge of heat required by the University of London for its matriculation examination, and moreover the subject is treated very much in the order laid down in the University Calendar. In the description of the different thermometric scales in use, the commonly-received opinion is that Fahrenheit fixed his zero-point at the temperature of a mixture of snow and salt or sal-ammoniac, on the supposition that in such a mixture no heat remained. It is, however, the opinion of at least one well-known Professor of Natural Philosophy (moreover a London University Examiner) that Fahrenheit had other and far more philosophical reasons for thus defining his zero, but what those were it is impossible at this distance of time to determine, since the Dutch physicist left scarcely any papers at his death.

The study of the laws regulating chemical affinity is deferred until the student has gained a-preliminary knowledge of the principal facts concerning hydrogen, chlorine, and their compound hydrochloric acid. This method of procedure has unquestionably the merit of simplicity over the more usual plan, and materially facilitates the subsequent consideration of the laws of combining proportions, atomic volumes, &c.

Considerable space is justly afforded to a consideration of the properties of water, and their influence in the economy of nature; of the peculiarities of the several kinds of natural waters, together with the methods for their purification from natural impurities and artificial contaminations. The author very properly insists upon the injurious effect of allowing the gases, which enter through the waste-pipes descending into drains and sewers, to pollute the water in our cisterns. "The arrangement to which this abominable nuisance is due is briefly this; cisterns are filled daily by means of a tap, to which is fitted a ball-cock to arrest the supply of water so soon as the cistern is nearly full; just above the level where the rise of water in the cistern is thus arrested is the opening of a pipe which leads straight into a drain communicating with the sewer; lest the atmosphere of the house should, through this pipe, be placed in direct continuity with that of the drain and sewer, it is usual to make the pipe that enters the drain curved at its lower end, in the shape of the letter J; it is imagined that this bent extremity of the tube will always be kept filled by water flowing down from the waste-pipes, and will act as a tolerably effectual valve so as to exclude the sewer gases. But what is really the fact? On account of the very ingenious regulation of the ball-cock, no water can ever pass into the waste-pipe at all, nor even rise to the level of its orifice; the consequence is that if there ever was any water in the bend of the waste-pipe it cannot have remained there long, nor can it ever be renewed; hence no valve is interposed between the atmosphere of the house and that of the drain, and fires, and chimneys—creating the draught which enters the house at every chink and opening—suck into it the foul gases of the drain and sewers to pollute the air we breathe, and not only so, but also—seeing that it is across the surface of our cisterns that all these gases (many of them highly soluble in water) are dragged—to render the water which we drink, and which we associate with the idea of purity and cleanliness, unclean and deleterious. The only cure practically applicable and fairly reliable for water thus clumsily contaminated, after it has, perhaps, for the sake of a pure supply, been brought from a great distance and filtered with extraordinary care, is filtration through animal charcoal, renewed regularly at short intervals. But since prevention is better than cure, and (so far as I know) the only *sure* way of avoiding these dangers of contamination in towns is to allow water to be drawn straight from the mains, without the intervention of any apparatus of cisterns and waste-pipes, and to have a constant instead of an intermittent system of delivery, this plan, already followed in Manchester and some other towns, and at the public drinking fountains in London, should be universally adopted." (pp. 98—99).

A large portion of the chapter on the atmosphere is avowedly compiled from Dr. Roscoe's article in "Watts's Dictionary of Chemistry," but the author errs in ascribing the gravimetric process for the determination of the principal constituents of the air to Dumas and Peligot. Dumas's collaborateur in the famous research was, as every tyro could tell, not Peligot but Boussingault. On p. 123 we see repeated the statement of Liebig that the absolute quantity of carbon contained in the atmosphere as carbonic acid amounts to more than the weight of all the plants, and of all the strata of mineral and brown coal existing on the earth. A moment's calculation will suffice to show the incorrectness of this statement. The relative volume of carbonic acid contained in the atmosphere is usually stated at four volumes in 10,000 of air, equivalent to ·0612 per cent by weight. It certainly cannot exceed this amount, indeed the recent researches of Angus Smith on the air of mountains, and of Thorpe on sea-air, render it highly probable that this number as an average is somewhat too high. From the barometric pressure of 30 inches of mercury, and the known area of the globe, the weight of the atmosphere is found approximately to be 5,260,000,000,000,000 tons, and hence the weight of carbonic acid would be 3,220,000,000,000 tons. Now Mr. Hull, in his "Coalfields of Great Britain," estimates the amount of available coal in the English and Welsh coal fields at 60,000,000 tons for 1000 years, and states the American coal fields to be 72 times greater than our own. Assuming it to contain 80 per cent of carbon, this coal by its combustion would produce 12,845,000,000,000 tons of carbonic acid—a quantity nearly four times as much as that actually existing in the atmosphere at present. Bischof has also demonstrated the falsity of this statement of Liebig, by calculating the probable amount of carbon contained simply in the thick strata, occurring both in the clay slate and in the more recent schistose formations. Assuming the average quantity of carbon contained in these strata to be 0·1 per cent—a quantity without doubt far short of the actual amount—and assuming also the thickness of the entire sedimentary formation to be eight miles, it may easily be shown from these data that the carbon contained in these strata would amount to nearly seven times as much as the absolute weight of carbon in the atmosphere.

But enough has been written to show the general character of this book. On the whole, it fairly represents such of the leading features of the science as may be gleaned from the study of the non-metallic elements. We cannot, however, anticipate for it a permanent place among estab-

lished manuals. The book is doubtless adapted to the requirements of students preparing for the matriculation examination of the University of London, and intending to proceed to an Arts degree, although we are slow to believe that this constitutes the main idea of the author, since any book, however ably compiled and arranged, professing merely to facilitate the process of cramming, is unworthy of much respect or toleration.

CONTEMPORARY SCIENTIFIC PRESS.

(Under this heading it is intended to give the titles of all the chemical papers which are published in the principal scientific periodicals of the Continent. Articles which are merely reprints or abstracts of papers already noticed will be omitted. Abstracts of the more important papers here announced will appear in future numbers of the "Chemical News.")

Bulletin de la Société Chimique de Paris.
August.

BRISSE, "A new Method of Manufacturing Soda." SCHIFFMAN, "A Method of Manufacturing Hyposulphite of Soda from the Waste Products of Soda Making." CLEMANDOT, "A new Silica Glaze for Pottery Ware." J. FUCHS, "On Preparing Metals in Fine Powder by Amalgamation." EVRARD, "On the Manufacture of Aluminium Bronze." W. FULLER, "A Method of Separating Gold and Silver from their Ores by means of Lead." ESQUIRON and GONIN, "On a Method of Revivifying Peroxide of Manganese." TILGHMAN, "A Process for Manufacturing Paper Pulp from Wood." J. ROUSSEAU, "An Improvement in the Manufacture of Beetroot Sugar." JUNEMANN, DU RIEUX, and BOTTGER, "On the Use of Strontia in the Manufacture of Sugar." DU RIEUX and BOTTGER, "A new Method of Treating Beetroot Pulp and Juice with Lime." BOURNE, "A Method of Deodorising Vulcanised India Rubber." FORTIER, "On a Process for rendering Fabrics Waterproof."

Annales de Chimie et de Physique.
August, 1867.

A. CORNU, "Researches on Crystalline Reflection." P. VIGIER "On the Mteallic Phosphides." W. D. KHANIKOF and V. LOUGUININE, "Some experimental Researches on Henry and Dalton's Theory of the Absorption of Gases by Liquids at a Constant Temperature and under variable Pressures." J. BOUSSINGAULT, "On the Fermentation of Stone Fruits." V. LONGUININE, "On the Density and Dilatation of Benzene, Toluene, Xylene, and Cymene." PERRET, "Note on the Use of Pyrites instead of Sulphur in the Manufacture of Sulphuric Acid."

Bulletin de la Société Industrielle de Mulhouse.
July, 1867.

M. ZIEGLER, "On the Presence of Aniline in the Coloured Fluid ejected by the Sea Hare *Aplysia depilans*." G. SCHAEFFER, "Report on Pernod's New Extract of Garancine. JUNDT, "Report on Broun's Photographic Reproductions of Sketches and Cartoons." SCHEURER-KESTNER, "Report on A. Rivière's Memoir on the Preparation of Caustic Baryta." I. SCHLUMBEGER, "On a Peculiar Product of the Calcination of Paper." SCHEURER-KESTNER, "On the Presence of Chloride of Calcium in certain Fossils found in the *Lehm* near Colmar." A. BRAUN, "On some Carbon Prints." E. HOFER-GROSJEAN, "On the Substitution of Cadmium for Bismuth in Stereotype Metal." A. BRAUN, "On an Improvement in the Carbon Process."

NOTES AND QUERIES.

Deodorising Petroleum—I have been informed that a solution of "plumbate of soda" will deodorise petroleum. Can any of your correspondents courteously give me instructions in the mode of preparing and using this agent.—O. P. A.

Cleansing Fire-arms.—Can any of your readers kindly inform me as to the composition of the "spirit" sold for cleansing fire-arms. I imagine turpentine is one of the principal ingredients.—THOMAS BLAIR.

Water for Steam Boilers.—E. S. T., in "Notes and Queries," will find all he requires in "Engineering," (published at 37, Bedford Street, Covent Garden), for March 22nd, 1867, page 280; for March 29th, 1867, page 304; and May 10th, 1867, page 484.—H. K. B.

Extraction of Oil—Dyeing Turkey Red.—In answer to correspondent E., I can state that bisulphide of carbon is inapplicable to the extraction of olive oil from an aqueous solution of carbonate of potash, as bisulphide of carbon is decomposed in alkaline solutions forming sulpho-carbonate of the alkali, which still keeps the oil in solution. The process carried out in Turkey-red works for the recovery of oil is to neutralise the emulsion with sulphuric acid, when the oil separates. Hyposulphite of alumina has been tried as a mordant in Turkey-red dyeing, but does not produce colours so brilliant as the mordants in use. To describe the Turkey-red process, as carried on in Lancashire, would occupy more than the space set apart for "Notes and Queries;" but I may state here that the Turkey-red dyers of England are three in number. Two of them adopt a process similar to that invented and carried on by F. Steiner, Esq., of Accrington, who is the third. This method cannot be applied to the dyeing of yarns. The process for yarn dyeing is a modification of the Scotch process for cloth.—F. F. F.

MEETINGS FOR THE WEEK

SATURDAY.—Royal Institution, 3 o'clock, Professor Tyndall on "Heat and Cold." (Juvenile lectures,)
MONDAY—Medical, 8 p.m.
TUESDAY—Royal Institution, 3 o'clock, Professor Tyndall on "Heat and Cold." (Juvenile lectures.)
WEDNESDAY—Geological, 8 p.m. On the "Lower Lias of Bristol," by W. W. Stoddart, Esq., F.G.S. On the "Lower Lias at Cotham, Bedminster, and Bristol," by C. O. Groom Napier, Esq., F.G.S.
—— Microscopical, 8 p.m.
THURSDAY.—Royal, 8½ p.m.
—— Zoological, 8½ p.m.
—— Royal Society Club, 6 p.m.
FRIDAY.—Astronomical, 8 p.m.
—— Geologists' Association. Anniversary meeting, 7½ p.m.

TO CORRESPONDENTS.

W.—1. The oil-nut has been submitted to a high botanical authority for report upon. 2. We believe no one bought the business you speak of. You can get everything you require in the way of apparatus at Griffin and Sons, Garrick Street.

W. R.—Will our correspondent favour us with the full description of the dyeing process which he thinks would be too long for our columns?

X.—The explanation is very simple. Each acid will separate a portion of the other from its combinations. Our Publisher says that he can supply the periodicals if you will send particulars.

W. H. Vodrey.—We are obliged for our correspondent's calling attention to the article, and will endeavour to meet his wishes. It is very gratifying to find that the American reprint of the CHEMICAL NEWS is so highly appreciated in all parts of the United States.

J. W. Young.—Received with thanks.

D. Waldie.—The remarks referred to in the Quarterly Journal were not written by the Editor, and were in fact inserted without his knowledge. We will draw attention to your answer.

C. P. Bahin.—Either Bowditch's or Sugg's treatise on coal gas will answer your purpose. They have each been reviewed in these columns.

Spectrum will find some excellent articles on the application of spectrum analysis to the microscope, by Mr. Sorby, in recent volumes of the CHEMICAL NEWS.

Enquirer.—The French metre is the forty-millionth part of the length of the earth's meridian, or the ten-millionth part of the distance from the equator to the pole.

A Mystery.—A correspondent forwards the following cutting from a contemporary, and asks for an explanation: "Luecinic Acid.—M. J. Hooper, in a communication to the CHEMICAL NEWS, points out that the presence of active acid or a soluble acitate, partially or entirely suspends the ordinary reaction of succinic acid in solutions of ferric salts."

H. Bamber—Coat the polished steel with a mixture of lime and sweet oil before putting it away. This will prevent it getting rusty.

Analyst.—A mixture of finely granulated zinc and iron filings, put into an alkaline solution of a nitrate, will cause all the nitrogen to be evolved in the form of ammonia. Harcourt's process for estimating nitrates is based on this principle.

Xpect.—You can filter the strong nitric acid solution through a tuft of gun cotton loosely pushed into the neck of a funnel.

E. K. B.—We strongly advise you not to attempt io prepare chloride of nitrogen. Nitroglycerine is innocence itself in comparison to it.

Communications have been received from Dr. H. Bence Jones, F.R.S.; M. Peterson; J. Hallett (with enclosure); Constantine, Count Zabielo; L. Lloyd; F. O. Ward; Dr. Quarlach; F. H. Hill (with enclosure); Dr. Röhrig (with enclosure); J. Russell; W. Holyoak; F. Roberts; Dr. Letheby (with enclosure); A. Lavenant; Dr. R. Angus Smith, F.R.S.; Captain W. A. Ross (with enclosure); T. Reader (with enclosure); F. C. Samuels; G. F. Symons; Dr. Wilhelms; C. P. Bahin; E. J. Quarles; W. Sutton (with enclosure); T. Blair; D. Waldie (with enclosure); J. Wallace Young (with enclosure); S. Smith; Vodrey and Bro.; J. Bray; M. A. Whichelo; W. Rodger (with enclosure); W. H. Dear; W. Salter (with enclosure); J. E. Thorpe; W. E. Walker; P. Jessop; G. Farrer; L. Honer; W. Wilson (with enclosure); W. Kelso; W. H. Hoadley (with enclosure); J. H. Johnson (with enclosure); Longmans and Co.; Spottiswoode and Co.; E. W. Eccles; J. Robarts Penrose; Dr. Adriani; A. C. Bowdler; Dr. Day; Mawson and Swan; H. Rastrick; J. Chalmers, A. Glendinning; J. A. Parkes; L. Wundt; C. E. Gorman; P. Darcey; Rev. A. Rigg; Liebig Extract Meat Company.

Books Received.—"Philosophical Magazine" for Jan., 1868; "Popular Science Review;" Hardwicke's Science Gossip;" "First Principles of Modern Chemistry," by U. J. Kay Shuttleworth, London, Churchill; "Braithwaite's Retrospect of Medicine," London, Simpkin and Co.; "American Journal of Mining;" "Scientific American;" "American Gaslight Journal;" "Pharmaceutical Journal;" "A Dictionary of Chemistry," by Henry Watts, B.A., F.R.S., part xliii., London, Longmans and Co.; "The Journal of the Quekett Microscopical Club," London, Robert Hardwicke.

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THE CHEMICAL NEWS.

Vol. XVII. No. 423.

ON

HEAT AND COLD;

A COURSE OF

SIX LECTURES*

(*Adapted to a Juvenile auditory*),

DELIVERED AT

THE ROYAL INSTITUTION OF GREAT BRITAIN,

(*CHRISTMAS*, 1867—8),

BY

JOHN TYNDALL, Esq., LL.D., F.R.S.

Lecture II.

Change of volume (continued)—The force of heat—How to measure heat—Boiling water.

I want you in the first place to pay attention to what Mr. Cottrell will do here in front of the table. There is a very thick bombshell, for which I am indebted to the great kindness of my friend Professor Abel of Woolwich. It is now filled with water, and the hole of the bomb is plugged. Mr. Cottrell will now place the bomb in this bucket, which contains a mixture of pounded ice and salt; and I want, if I can, to explode that bomb. Do not feel in the least alarmed about it. The explosion will not be such as to injure any one. I will ask him now to cover the bomb carefully with this freezing mixture of pounded ice and salt, and we will leave it there for half or three-quarters of an hour, first putting a blanket over it in order to keep the warm air of this room from acting upon it. And now on the top of this I will put these iron bottles and this leaden bottle, which also all contain water. Having placed them in the freezing mixture we will examine what occurs when the water within these bottles and this bombshell freezes. It will require, no doubt, half an hour or more to produce any action upon the bomb, because it contains a very considerable amount of water. We may possibly obtain an action more rapidly upon the iron bottles, though they are exceedingly thick. We made a similar experiment with a bombshell in the yard of the Institution, and there it occupied only half an hour to freeze the water and burst the bomb. The result is here in these fragments which are on the table. Look at the thickness of these pieces, I hope the bombshell now in the bucket will be pleasant and courteous and agreeable enough to burst before the lecture is ended, but in case it does not burst, these fragments must represent the effect I intended to produce. [At a subsequent stage of the lecture the success of the experiment was indicated by the bursting of the bomb. At the conclusion of the lecture the bottles were also found to have been burst by the freezing of the water.]

And now let me recur for a moment to our last lecture. I then attempted something very daring indeed. I dare say many of my elder hearers will have imagined that, in fact, I aimed too high,—that I endeavoured perhaps to make you understand too much; but I do not think that that was the case. I think it is possible for your minds to see the operations of this thing that we call heat almost —not quite, I think, but *almost*—as clearly as I see these operations myself, and for this reason I wish, as far as in me lies, to make you see what I see, when I think and talk of this thing that we call heat. It was for that reason that I endeavoured to cause you to picture to your minds first of all the motion of the particles produced by striking a piece of lead. You remember I put a piece of lead upon the anvil and struck it forcibly with the hammer, and in that way I produced heat. I then went on from that to what we call combustion; and I asked you to consider this combustion as something almost identical with the action of the hammer upon the lead,—that the combustion of bodies is due to the fact that our atmosphere contains what is called oxygen gas—the vital gas,—and that when certain bodies are raised in temperature this oxygen hits them with such force as to produce the effects that we call combustion. This, in point of fact, is the theory of combustion. If we remove the oxygen from a place where a body is burning, you will find at once that it can no longer burn. In order to make that evident to you, I have here a candle which I intend to place under what is called the "receiver" of an air-pump. Now you have the candle burning within the receiver of the air-pump. If I allowed it to continue burning, the oxygen enclosed in that receiver would by and by be exhausted by the burning of the candle, and the flame of the candle would die out as soon as the exhaustion of the oxygen took place. I will hasten that exhaustion by working the pump, and rendering the atmosphere around the candle rare; and you will find that presently the flame will become rather feeble. [The air-pump was then set in action.] You see the flame is already beginning to become dim. Now it is very dim. As I work on it becomes still dimmer, but if I let a little oxygen into that receiver I at once restore the brightness of the flame. [Some oxygen was caused to enter the receiver.] Now the flame is brighter than it was before. If I exhaust again you will find that as we take the oxygen away we remove the atoms that are now, as it were, showering down against the combustible matter of that candle. If we take those atoms away you see the flame becomes more and more feeble; and finally if I proceed farther I should be able, of course, to entirely extinguish the flame, for when these little oxygen atoms are no longer able to rain down upon that flame, then the flame inevitably goes out. I will readmit the air before the flame is quite extinguished. [At this moment the candle ceased to burn.] Ah! I am too late, and the flame has gone out. Now, you saw that just before that flame went out it was exceedingly feeble. It was exactly similar to the flame that you obtain at very high elevations upon the earth's surface. Many years ago Dr. Frankland and myself spent a whole night upon the top of Mont Blanc. We slept upon the top, and we there burned a number of composite candles such as we have here, and we also burned a number of them at Chamounix. The air upon the top of the mountain was very rare and very thin, and it was most wonderful to see the effect of this rarefied air upon the flames of the candles. They were exactly like the flame you saw here immediately before it went out. Strange to say, however, the quantity of stearine (the stuff of which these candles were made) consumed above in one hour was exactly equal to that consumed below. There was no sensible difference, in fact, between them, notwithstanding the enormous difference in the characters of the flames. So much for these flames.

We must now say one or two words with regard to the structure of this wonderful and beautiful thing—flame. If you look at the flame of a candle you will observe a particular portion of it to be much more luminous than the rest. At that particular part the flame gives out its greatest light; and if you light two candles, such as I have here, and look at the flame of one of these candles through the flame of the other, you will find that you can, with the greatest ease, see one through the other for a considerable distance upwards; but then you come to a very bright portion of the candle flame, and that bright

* Reported verbatim, by permission of the Author, for this journal.

portion almost wholly cuts off the vision of the other candle. Thus, through the part of the most intense brightness the light of the other candle cannot pass. There is something going on which intercepts the light of the other candle. Now, what is this something? This will lead us to a knowledge of the structure of this beautiful flame. The flame here is produced in this way. We have a wick in the centre of this column of greasy combustible matter. We ignite the wick. The heat first of all liquefies the greasy matter, and not only liquefies it, but reduces it to a state of vapour, or gas. The candle actually makes its own gas. This vapour comes from the candle straight upwards; and being heated and surrounded by the oxygen of the air, this heated vapour is immediately attacked by the oxygen; the atoms of oxygen plunge against the vapour, and what we see as light and heat is the result of this collision. But, let me say a word or two more with regard to flame. I have spoken of the vapour of the greasy matter of the candle. That vapour is composed mainly of two distinct substances. It is called a "hydrocarbon." We have there hydrogen, which is a gas, and we have carbon, which is also, under certain circumstances, a gas. These bodies are united together in the grease of this candle. Now follow me, please; and you will understand the structure of this candle-flame immediately. The vapour is attacked by oxygen; but the oxygen loves the hydrogen better than the carbon. It takes the hydrogen first, and liberates little solid particles of carbon in the flame. These carbon particles are the soot which you see sometimes in a smoky flame. You see the smoke here, in point of fact. If the combustion were perfect all that smoke would be burned, and it would be raised to a white heat in the flame. In that particular portion of the flame which gives out the maximum amount of light you have a crowd of these solid carbon particles raised to a white heat by the intense temperature of the flame. And then, finally, these carbon particles also become burned, and the products of combustion pass away into the air as gas. This is the structure of all flames—first of all, an inner core of unburned gas or vapour; and then round about that the oxygen of the air plunging, as it were, against the heated vapour, and forming a kind of luminous shell round about the interior ball.

If, when the carbon particles were heated and liberated from the hydrogen in the manner I have described, oxygen were at once to seize upon them, you could not have this intense luminosity that you find in the candle-flame. Here is a lamp, constructed by a particular friend of mine —Professor Bunsen, of Heidelburg—and you see it burns with a very small amount of light. The reason of that is that, by means of these apertures which he has made round about the central tube he mixes the oxygen of the air with the gas before the gas is ignited, and the presence of this oxygen entirely destroys the existence of these carbon particles, to which the light of the flame is mainly due. If I cut off the air the gas alone will come out, and you see then at once that the light greatly increases. In the former case you have the carbon particles halting for a moment in the flame, and raised to a white heat before the oxygen seizes them; and thus you have a far greater amount of light than when you allow the oxygen to get in amongst them and seize them the moment they are liberated.

The combustion which I have just shown you is of a very vivid kind. There are also slow kinds of combustion going on. For instance, when the oxygen of the air attacks iron, it produces that red iron rust with which you are all very well acquainted. This is just as much a case of combustion as the combustion exhibited in the candle-flame. It is a case of slow combustion. When the earlier of the Atlantic cables was made it was surrounded by iron sheathing to protect it; and it was found in one case that the temperature of a great coil of this cable became very high indeed—so high as to imperil the gutta-percha and other substances that were employed to insulate the wire. This was found to be due entirely to the slow combustion—to the rusting, or "oxidation," as it is called, because oxygen is concerned in it—of the iron. The iron was slowly burned, and the heat could not get away because the coil was so large, and the consequence was that its temperature became dangerously high. Mr. Siemens has invented an exceedingly beautiful instrument for the purpose of testing cables for this heat. And so in the case of our own bodies there is going on as true a combustion as in the case of the burning candle. We take in food, it is conveyed into the blood, we breathe the oxygen of the air, that oxygen comes into contact with the food in the blood, and the food is there slowly burned, and consequently we are rendered warm. The heat of our bodies is derived entirely from this slow combustion.

Towards the close of the last lecture I passed on to a consideration of what heat does. The usual result, as I told you, is that bodies are made to expand with heat. I made several experiments in proof of this,—one with a very beautiful piece of apparatus made for me by Mr. Becker, by which we multiplied the action more than a thousandfold in order to enable you to see the expansion which occurred when I breathed against a pillar of lead. I now want to make clear to you the wonderful strength of this force with which bodies expand, and the wonderful strength of the force with which they contract. The forces which pull the atoms or molecules of a body together on its cooling are perfectly enormous. I will illustrate this by an experiment which you will understand by reference to this model. I place in a hole at the end of this iron bar a little piece of wood; you see the two ends of this piece of wood rest against these two edges; and if I pull the bar I break the piece of wood. You will observe that it first of all bends and then breaks. Now, what I am going to do is this: for this piece of wood I am going to substitute a piece of steel, and then I shall put a red-hot bar of iron across, and screw it on between these two points. It will cool, and the contraction will, I think, be so great as to break the bar of steel in the way in which I have broken this bar of wood. You see the construction of this iron apparatus is much the same as that of the model. [A red-hot bar of iron was screwed to the apparatus as described by the lecturer.] I will hasten the cooling by pouring a little water on the iron bar. [In the course of a few seconds the steel bar snapped.] There it is. The bar of steel is, in point of fact, smashed by the force with which the particles of the iron bar pull each other together when the motion of heat is taken away from them by cooling. That force is, as I have said, perfectly enormous.

Before we pass on to consider the expansion by heat of other bodies besides solid bodies, I should like to explain for the sake of the elder boys (not for the sake of the younger ones, because they will, perhaps, find it a little too difficult for them) the use of one term that is in common use in books that are written on the subject of heat. Suppose you have a piece of lead 3,510 inches in length, and suppose you augment the temperature of that lead one degree, you would find that its length would extend from 3,510 inches to 3,511 inches. That is it would extend $\frac{1}{3511}$ths of its length. This is the fraction of its own length which the lead expands on having its temperature augmented one degree. Now, that fraction is what is called the *co-efficient of expansion* of the lead. This co-efficient of expansion is much less in many bodies than it is in the case of lead. For iron this co-efficient of expansion is not half what it is for lead. This difference renders it needful for engineers to be very careful not to unite different metals which have different co-efficients of expansion in such a way that on their expansion they would produce distortion and disruption, and, perhaps, fracture. Here, for instance, is a ruler which has one side of brass and one of iron; and when it is heated, in consequence of the brass expanding more than the iron the ruler becomes curved or buckled up. Now, in an architectural structure different metals might be associated

in such a way that on a change of temperature the edifice would be endangered in consequence of the metals expanding or contracting in different proportions. That fact is a very important one for architects to remember.

We will now proceed to a consideration of the expansion of liquids by heat. Here is a bottle containing water, another containing alcohol, and a third containing the liquid metal mercury. Here also is a bulb containing mercury. If I lay hold of this bulb the mercury within it expands, and this little column above the bulb is forced upwards. Now I want to show you, if I can, the motion of the mercury when the bulb is heated, and for that purpose I will throw an image of the column upon the screen. Now you have on the screen an inverted image *t'* of the mercury column *i i*, turned upside down by the lens L which you see in front of the lamp E, and I think you will see that when I heat the bulb, the column *i i* will go towards the lower part of the screen, owing to the expansion of the metal. It really goes upwards, but it appears to go downwards, owing to the image being inverted. I will now place the bulb in hot water,—observe the motion which I indicated. I will now take a bulb containing the liquid alcohol, which is much more expansible than mercury. Mr. Cottrell has coloured it blue, that you may see it better than you would if it were not coloured. The colour indicates the column of liquid. At the first moment of the bulb being heated the column of liquid will appear as if it contracted instead of expanded. This apparent contraction is due to the fact that when we first plunge the glass vessel containing the alcohol into warm water that vessel itself expands, and becomes, in fact, of larger capacity, and thus the column of liquid sinks in it. This sinking, however, will immediately disappear, and then the blue liquid will go up in the tube far more rapidly than the mercury rose. I might take other liquids and show you the same effects, but we must now pass on to the question of the expansion of gases.

You will understand in a moment that gases are capable of expansion by heat. For instance, I have here (Fig. 2) an empty bottle F, to which is attached a tube; and Mr. Cottrell is now placing the end of that tube underneath this column of liquid *t t*. The column of liquid is supported by what the elder boys know as the pressure of the atmosphere upon the liquid outside. Now, if I heat this bottle I cause the air in it to expand: it will ascend with force into the tube *t t*, the water will descend, and in that way I think I shall be able to transfer the air from the bottle into the tube now containing the column of water.

Fig. 1.

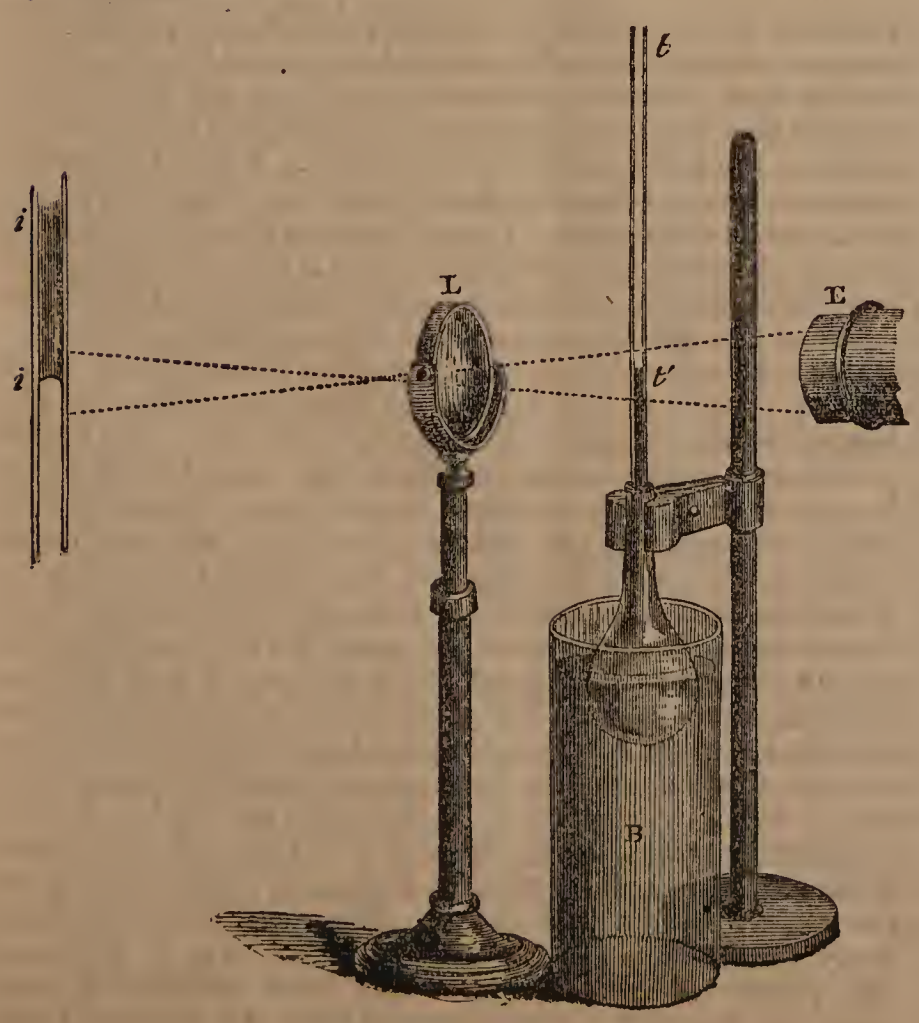
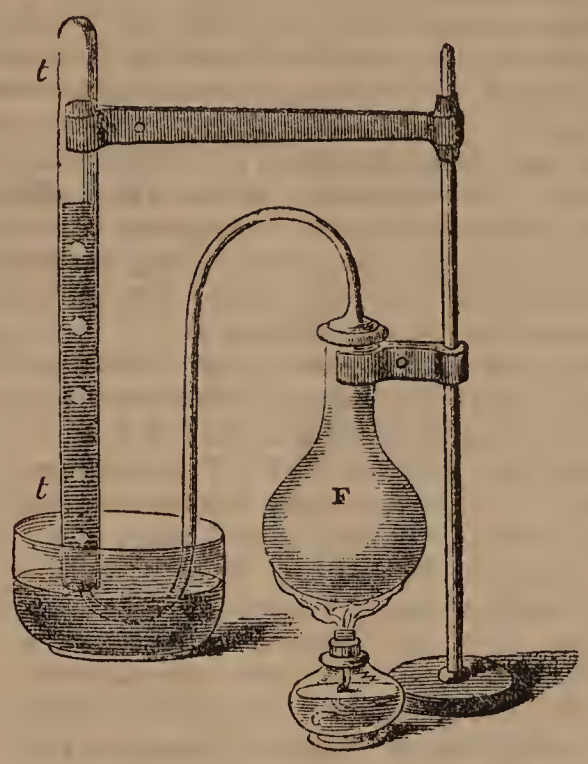


Fig. 2.



Observe now the bubbles of air going up, and pressing down the liquid column. This pressure is due to the expansion by heat of the air in the flask. I might continue this process until nearly the whole of the air of the flask was transferred to the other vessel.

In reference to this subject I might refer to this instrument, which is a thermometer made for the purpose of measuring heat by means of the expansion of air. Here at the top is a bulb filled with air. The liquid column now stands at a certain point. If I put my hand upon it the column descends. The warmth of my hand is causing the air to expand, and in doing that it drives down the liquid column.

Before proceeding farther, I must say one or two words with regard to a term I have just employed. I have used the term "thermometer." That is, *a heat measurer*. I have made use of this bulb of mercury, and the tube attached to it, purely for the purpose of enabling you to understand the common thermometer. If you take this bulb of mercury and plunge it into melting ice, or into water just frozen, at any part of the earth's surface, you will always find that the column of mercury stands at precisely the same height, so that this temperature of frozen water or melting ice is the same thing all the world over. Here, then, we have, so to say, a standard of temperature. First, suppose that our bulb of mercury is plunged into melting ice: that will give the freezing point of water. Then plunge it into boiling water under the same barometric pressure, and the height to which the column will rise under such conditions will be the same all the world over; and that point will indicate the boiling point of water.

We have three different kinds of thermometers. First of all there is the thermometer of Fahrenheit. In constructing his thermometer Fahrenheit made use of a mixture of ice and salt, and he found that this mixture gave him a far greater cold than that of ice itself. He thought this was the greatest cold possible, and he therefore marked that temperature as the zero of his scale, and began to number his degrees from this zero which represented the temperature of pounded ice and salt. He then went upwards to the freezing point of water, which was 32 degrees above his zero. He then obtained the boiling point of water, and divided the distance between the freezing point and the boiling point of water into 180 equal parts or degrees. The 180 added to the 32 makes Fahrenheit's boiling point 212 degrees above his zero. The second thermometer is one which is in general use amongst

scientific men, and I wish it was employed in all parts of the community. It is known as the Centigrade thermometer. This was invented by Celsius, and is sometimes called Celsius's thermometer. Here we have the distance between the freezing point of water and the boiling point divided into 100 equal parts or degrees. We have a third sort of thermometer which is known as Réaumur's. It is a very awkward one, but it is nevertheless used a great deal in Russia. In this instrument the distance between the boiling and freezing points is divided into only 80 different parts. The degrees in these three different thermometer's—Réaumur's, the Centigrade, and Fahrenheit's —are in the respective proportions of 4, 5, and 9. So much then for the terms "degree" and "thermometer" which have been used in these lectures.

Now, if possible, I should like to show you heated air. You cannot detect it by looking at it directly in the atmosphere, but it can be made evident by a device which I intend now to employ. I can show you this heated air rising up in streams from a heated body. Here is a hot spatula. If you look directly at this hot body you can see no emanation whatever from it; but now my assistant will throw a beam of electric light upon this spatula, and we will observe the shadow of it upon the white screen. You now see above the image of the spatula a stream of heated air rising from the hot surface. This effect is quite invisible when you look at the spatula in the ordinary way.

I want now to show you another stream of air. I have here the means of giving you a still greater stream of heated air; and I want to make you acquainted with the celebrated invention of that eminent man, Mongolfier. He conceived the idea of catching these streams of heated air in a bag, and in this way the bag was carried up. From the chimney of this stove we get a stream of heated air. You observe by the effect on the screen how powerfully that stream is rising. I have here a paper balloon, and in this balloon I will catch the column of heated air. If I am successful we shall by-and-by get the balloon filled with the hot air, and then we shall make Mongolfier's celebrated experiment. You see the sides are swelling by this heated air being accumulated inside the balloon. I will now let it go out of our hands, and I venture to say it will sail upwards. There it goes. It has not gone so high as it ought to have gone, but still it will answer for philosophers as an illustration of the balloon of Mongolfier.

It is found that in the case of solids and liquids the expansion is exceedingly irregular, The co-efficient of expansion varies very much. But strange to say—(and I wish I could go into the reason and tell you why)—in the case of really perfect gases it is essentially the same for all. If you take 490 cubic inches of air and heat it one degree it becomes 491 cubic inches, so that the fraction $\frac{1}{490}$th is the co-efficient of expansion of air; and this co-efficient, as I have said, is almost exactly the same for all gaseous bodies whatever.

Now I have to direct your attention to some experiments with regard to the action of heat upon liquids; and with this view I have provided an apparatus (Fig. 3) which I will now ask Mr. Cottrell, the assistant, to place upon the end of the table. I will now cause the water in this flask F to boil, and I want to show you now what is meant by the vapour of water. We will apply heat to the flask, in which is a quantity of water, and after a little time the water will boil and bubble up. I want you to understand accurately the meaning of this bubbling up. What is going on at the present time in that flask of water is this. The water is heated. As the heat becomes more and more intense this shivering, quivering, vibratory motion becomes more and more intense, and then particles of water are jerked away from the upper surface, and carried away into the space here above. After a time the water begins to bubble. Here you have the bubbles of steam rising to the top. Now, the surface of the liquid is in communication with the air. Every square inch of the surface of that flask of water bears a pressure of about 15 lbs. and every little bubble there bears a pressure of several pounds. Why is it that the bubbles are not crushed? Simply because the pressure of the vapour within them is exactly equal to the pressure of the atmosphere without, so that the film of liquid is squeezed between the air on the upper side and the vapour on the lower side. If you lessen the pressure of the vapour within, you will have the bubble crushed by the pressure of the atmosphere. The boiling point of a liquid is precisely that temperature at which the

Fig. 3.

pressure of the vapour of a liquid equals the pressure of the atmosphere. Now, by turning this tap *y*, I have enclosed in the flask some heated water; and you see that at the present time it is quite quiescent. The vapour in the flask is pressing upon the surface of the liquid. But if I take that pressure away, I have no doubt that the water will again boil. How can I do this? I have in connection with this flask of water another globular glass vessel G from which the air has been drawn by means of an air-pump. Hence the inside of this globe is a vacuum. Now, if I turn the cock *c*, which is between the flask and the other vessel, I open a way for the vapour in the flask to go from the surface of the liquid into the vacuum. Observe what occurs. The liquid is relieved of the pressure which was upon it, the water begins to boil, and the flask immediately becomes filled with the vapour of the water. The sides are now quite clouded. We can actually boil that water by cooling it. If the water in the flask were near its boiling-point, and we plunge cold water upon the upper part of the flask, we should condense the vapour above the liquid, and by thus relieving the water of the pressure on it we should cause it to boil. Here I have a tin vessel containing steam, and the air from which has been chased away by the steam. Mr. Cottrell will place it in front of the table. I will withdraw it from the flame, and I will in fact cause the water in it to boil by placing a piece of ice on the top of the vessel. [This was done.] The water is now boiling away, as the boys near at hand can see. Why? Because the vapour above the water has been condensed, and when the pressure is then removed from the surface of the

liquid, ebullition takes place. If more ice is placed on the top the water will boil still more, but the atmospheric pressure will, perhaps, crush the vessel entirely in. This effect will be due to the reduction of the pressure of the vapour on the inner sides of the tin vessel. [The effect anticipated was not produced, but the experiment was repeated at the beginning of the next lecture, and the sides of the tin can were then successfully crushed. The lecturer informed the audience that it had been found that the failure in the present instance was due to an accidental air hole in the side of the tin vessel.]

I have now to pass on to a consideration of the vapour of water. I have here the two gases or substances of which water is composed. I will show you first of all that one of these is a certain gas which is inflammable, and this gas we call hydrogen. Mr. Cottrell is now getting me some hydrogen which has been actually produced by the decomposition (to use a learned term) of water. He will now give me this gas. We hold downwards the mouth of the vessel containing it, as it is excessively light, and would escape if the vessel were held upwards. I will ignite this hydrogen, and you see what occurs. It is an inflammable gas. There it is burning with a flame at the top of the tube. Now the assistant will give me some of the other gas which is a constituent of water, and here we shall find our familiar friend oxygen—that gas which causes bodies to burn so brightly when they are placed in it. I will introduce into the oxygen a small bit of wood with an ember at the end; and what is the consequence? [The glowing wood immediately burst into a bright flame.] This gas is the other of the substances of which water is composed.

Now I will take the two gases mixed together, instead of having them in separate tubes. I have here a wonderful instrument [a galvanic battery] which enables me to tear asunder the particles of water. Mr. Cottrell will now connect the vessel of water with the battery, and we will let the decomposing gases escape into soap-suds. [The mixed gases from the decomposed water were caused to form bubbles with the soap lather. The lecturer then placed a cluster of the bubbles on the palm of his open hand, and exploded them by the application of a light.] How must you figure this act of the combination of hydrogen and oxygen? I suppose you must figure it in this way. You must figure them rushing together with a great clash, and then quivering and recoiling in virtue of their resilience—their elasticity. As far as I can follow the thing in my mind the flash is due to the collision between the particles of the oxygen and hydrogen. It is due mainly to the enormous heat produced by the collision; and the heat produced by this collision is so great that for a time the molecules of water produced are so hot that they are preserved in a state of invisible gas. Water is composed of oxygen and hydrogen in the proportion of two atoms of hydrogen to one of oxygen; and two atoms of hydrogen and one of oxygen constitute what is called a "molecule of water." *Molecule* is the term employed to express that combination, and you must remember the term.

I want to show you the difference between vapour and invisible gas. This room is filled with invisible vapour; but here, early in the lecture, I placed this vessel containing something very cold—a freezing mixture; and this frost which you see upon the outside of the vessel is due to the condensation of the aqueous vapour which has come from the gas-lights and from the lungs of the persons here present. That vapour has been condensed on the cold surface of the vessel containing the freezing mixture, and then frozen into hoar frost. The fog through which you were kind enough to come on Thursday last to this place was not a true vapour. It consisted of particles of water. Here you see the same thing. The steam which you see rushing from this vessel is not a true vapour. It is due to the vapour cooling and being precipitated. If I allow the steam to pass through this flame it is converted into a true vapour. The steam is now water—now vapour. [Passing the steam-jet through the flame, and thus rendering the steam invisible.]

After a time we shall have that vapour cooling and falling into the state of water, and then if we cooled that water still more the particles would bring other forces and powers into play; and those are the forces and powers that I now want to illustrate before you. I want to exhibit to you the marvellous force of crystallisation. When we cool water sufficiently it becomes, as every boy knows, reduced to ice. That ice is one of the most wonderful things on the face of the earth, and in another lecture I shall dissect a piece of ice and show you how wonderful it is. I want to show you something similar to what occurs on your chamber windows when they become frosted during the cold nights and covered with forms as beautiful as vegetable forms. I show you that in this way. If I took this piece of glass and poured a solution of common table salt upon it, and allowed it to remain, the water only would evaporate. The salt would be left behind incrusted on the surface of the glass. You can make the experiment at home with the greatest ease if you drop a little solution of sugar upon glass and allow it to stand. You get the water evaporated and the sugar remains behind. Now I want to do the same with a solution of another substance. First of all I must clean the glass plate perfectly, and this I do with potash; and then I shall put on it a film of a solution of something—not sugar, not salt, but something which will give me crystals more beautiful than either of them. We will take a liquid containing a certain kind of salt in solution, and I will pour this liquid upon the glass plate. I want to evaporate this film of liquid before you, and show you the crystallisation of the substance. [An image of the moistened glass plate was projected on the screen. Crystals began to appear in the course of a few seconds, and gradually spread over the surface of the plate.] See how splendidly these crystals form. See them building themselves together in this wonderful way as if they were forming vegetable growths before your eyes. This salt is ferrocyanide of potassium. We will take another plate, and cover it in the same way with a solution of chloride of ammonium. I will warm the plate in order to hasten matters. [This plate also was represented on the screen, and a similar result was obtained as in the last case.] How beautifully these crystals run together. There they are, darting out like spears. This is an experiment which one makes hundreds of times, but still it is sufficient to strike one with wonder. How beautifully the crystals assume their determinate forms.

One minute more. I want to tell you that in passing from the liquid to the solid state—in falling together so as to form those beautiful crystals—certain bodies, comparatively few in number, become larger. Water is one of these bodies, and that is the reason why ice floats upon the water. When water freezes it expands with powerful force. The bombshell which I placed in the bucket before you was, as you see, burst by the expansion of the water in the act of freezing.

Liebig's Extract of Meat.—The Government have contracted with Liebig's Extract of Meat Company (Limited) for the supply of the Company's extract to the troops of the Abyssinian expedition. The extract is packed in small jars which a soldier can easily carry with him, being enabled thereby to dispense with fresh meat for a number of days, and to cook a palatable soup in fifteen or twenty minutes at any halting place where hot water can be procured. The Government were no doubt guided in this decision by the experience gained in the last German war, it having been acknowledged by many officers and men that they owed to the use of this extract of meat the preservation of excellent health. In many cases, fresh meat distributed to the troops in the morning was spoiled by the effect of heat at the time it was wanted; the extract in all such cases proved an efficient substitute for meat.

ON THE

CHEMICAL CONSTITUTION OF FLUORINE COMPOUNDS,

AND ON THE ISOLATION OF FLUORINE.

By M. PRAT.

The following is a full abstract of M. Prat's memoir on this subject, recently communicated to the French Academy. The complete paper will not be published until the chemical referees of the Academy have reported on it.

M. Prat considers that chemists have hitherto been mistaken as to the composition of fluorides and the theory of fluorine. He regards the fluorides as in reality oxy-fluorides, and the equivalent of fluorine as consequently much higher than is usually supposed. He represents fluoride of calcium by—

2 equivalents of calcium	.. .. ..	40·0
1 ,, oxygen	.. .. ..	8·0
1 ,, the new fluorine	..	29·6
		77·6

This accords with the known analyses of fluor spar, since it contains 51·5 per cent of calcium.

By doubling the old equivalent of fluorine (19), we get 38, that is to say nearly the sum of the equivalents of oxygen (8), and of the new fluorine (29·6) = 37·6.

According to M. Prat, in order to obtain true fluorine it suffices to heat fluoride of calcium with chlorate, or rather with perchlorate of potash, since it is only after the formation of this latter salt that the reaction takes place. Oxygen is disengaged and also a product which silver absorbs. The compound so formed is fluoride of silver, insoluble in water, soluble in ammonia, from which it is precipitated by nitric acid, and more rapidly altered in the light than chloride of silver. Neither chlorine nor oxygen attack it even at the fusing point of the fluoride. It is, however, decomposed by potash at a dull red heat, and this reaction permits its analysis: it contains—

Silver	0·785	..	108·0 = 1 equivalent
Fluorine	0·215	..	29·6 ,, ,,
Fluoride of silver	1·000		137·6

This fluoride of silver, insoluble and very stable, and having great analogy with the chloride and the other compounds of this family, differs essentially from the soluble fluoride of silver of chemists, which according to M. Prat is a compound of—

AgFl, AgO, HO, in the hydrated state;
AgFl, AgO, in the anhydrous state.

Fluorine combines with chlorine. To obtain this compound it is sufficient to pour a weak solution of the hydrofluoric acid of the chemists into a solution of hypochlorous acid: there form

$$FlH, HO + ClO = 2HO + FlCl.$$

Fluoride of chlorine is gaseous, of a more intense colour than chlorine. It converts silver into a mixture of chloride and fluoride.

Fluorine may be isolated, according to M. Prat, by heating fluoride of lead of chemists (1 part) either with nitre (5 parts) or with binoxide of manganese (2 parts); oxygen and fluorine are evolved. A platinum alembic must be used. The oxygen is removed from the mixture by passing over fragments of heated baryta.

Fluorine is gaseous, almost colourless, of a chlorous odour, visibly fuming in the air, incombustible, and heavier than air. It bleaches indigo, and reddens and bleaches litmus. Ammonia produces fumes with fluorine, and will thus detect traces of it. It immediately decomposes water at the ordinary temperature. It combines with hydrogen in diffused light. Fluorine decomposes hydrochloric acid gas, and eliminates bromine and iodine from their compounds. It unites with boron and silicium, and with all metals of the first five groups.

ON THE

MANUFACTURE OF STEEL FROM CAST IRON,

BY THE USE OF NITRATES AND OTHER OXIDISING SALTS.*

By J. HARGREAVES.

The object of this invention is to effect the acieration of cast iron by a direct process, and thus dispense with the many permutations which it is at present made to undergo before the condition of steel is attained. I effect this by the agency of oxidising salts and oxides of iron and manganese. The oxidising salts which are most suitable for the purpose are the nitrates, and especially the nitrate of soda, on account of its lower cost, higher percentage of oxygen, and the highly electro-positive character of its base, which renders it a most effective agent in removing the metalloids, silicium, sulphur, and phosphorus, and the semi-metal arsenic from iron, by forming with them compounds of sodium, thus enabling inferior qualities of cast iron to be used in the manufacture of steel, and also to improve the qualities of malleable iron by depriving it of those objectionable substances. This is effected by placing the converting materials below the fused cast iron, and allowing the products of their decomposition to rise through the fluid metal, exerting while passing through it their chemical energies in separating from the cast iron the excess of carbon above that which is required to constitute steel, and in removing the metalloids which, even in the smallest proportions, deteriorate the value of the product.

The necessary use of fossil coal in Great Britain, in consequence of the scarcity of wood from which charcoal can be obtained, tends, while lowering the cost of production, also to deteoriate the value of the iron produced, by communicating to it some of its own impurities, which, added to those existing in the ore, render the iron very impure.

A great proportion of silicium and sulphur are separated by the lime used as a flux in the form of slag. But the last traces of these elements are very difficult to remove under the circumstances; while, with the exception of a slight trace, the whole of the phosphorus originally present in the ore remains in combination with the metal.

In 1861 my attention was first seriously attracted to the subject of the acieration of iron by the communications of the researches of MM. Caron and Fremy to the French Academy of Sciences, and which were published in the *Comptes Rendus*, an English translation of which first appeared in the Chemical News. These communications show that, to form steel with such qualities as will compete with that produced from Swedish and Russian iron, by cementation, it is necessary that the iron should be pure or approximately so; and that to effect cementation, nitrogen must be present in combination with carbon, or in contact with some gaseous compound of carbon. M. Caron disputes the hypothesis that nitrogen is an essential element in steel; but he admits that it acts as an inter-porter or carrier of carbon between the charcoal in the cementing pots and the iron, acting in fact as a kind of chemical shuttle, carrying carbon to the iron, and depositing it within the substance of the metal; and after delivering the carbon to the iron, it returns, and again picks up another charge or load of carbon, which it again delivers to the iron. A very small quantity of nitrogen is by this means made the agent for carrying a comparatively large quantity of carbon; and he proposes cyanide of ammonium as a gaseous cementing agent. M. Fremy, on the contrary, asserts that nitrogen is absolutely essential to acieration, and that in the absence of this element cast iron and not steel is the result of operating upon iron with pure carbon, unless the iron itself contains nitrogen. That iron can be made to combine with nitrogen has been placed beyond doubt by

* Abstract of a paper read before the Liverpool Polytechnic Society.

M. Despretz, whom M. Fremy quotes and whose experiments he repeats, a ferammoninm (NFe_4) having in fact been prodnced; *i.e.*, a compound analogous to the quasi-metal ammonium in which the hydrogen is replaced by iron. M. Fremy proposes to effect the cementation of iron by the use of ammoniacal and hydro-carbon gases, to supply nitrogen and carbon. I was then, and am still, disposed to take the view of M. Fremy as the correct one. But the thought suggested to me by the discussion between these eminent savants, was—"Why not obtain and use some material which shall effect the removal of the excess of carbon, with all other objectionable elements, and at the same time add nitrogen, which being chemically very inert, must be in the nascent state to effect its combination with iron, instead of removing the whole of the carbon, and then by a very expensive, laborious, and inconstant process restore a portion of the carbon extracted and a very small portion of nitrogen?"

The great difficulty was to find an agent capable of fulfilling the requisite conditions, which are:—

1. That it shall remove any desired quantity of carbon, this being varied with the varying proportion of carbon contained in the cast iron, and leave in it just sufficient carbon to effect its acieration.

2. That it shall remove all the silicium, sulphur, and phosphorus, or at least leave only traces of these elements.

3. That it shall supply nitrogen in the nascent state.

I occasionally took up and studied the subject, but with no satisfactory result—as I could not see clearly how to have a practical process, which could compete with those then in operation—till in 1864, when considering the theory of the action of the alkaline nitrates upon the elements composing cast iron, I found that these salts possessed all the properties necessary to accomplish the object I then had in view.

1. The quantity of carbon to be removed could be regulated at will by the quantity of nitrate used.

2. The alkali would in all cases be in excess of the quantity required to form chemical compounds with silicium, sulphur, and phosphorus, and give rise to the formation of silicate of soda, sulphide of sodium, and phosphide of sodium.

3. Nascent nitrogen would be formed in the presence of iron, in consequence of the formation of cyanides, by the reaction of the sodium or potassium and nitrogen of the nitrate upon the carbon in the cast iron, and would also be liberated upon the decomposition of the oxides of nitrogen.

4. The reaction of the nitrate upon fused iron could be easily effected by placing the nitrate at the bottom of a suitable vessel, and allowing the products of its decomposition to rise through the metal.

Before describing the details involved in treating iron by this process, I beg to point out the general principles upon which it is based. You will have before observed, that to effect the removal of a given quantity of carbon from cast iron, a given quantity of oxygen must be supplied, to convert this carbon into carbonic acid, or carbonic oxide. To convert six parts of carbon into carbonic acid, sixteen parts of oxygen are required, and to form carbonic oxide, eight parts of oxygen combine with six parts of carbon, and in one or both of these forms the carbon is eliminated from the iron. The weight of oxygen contained in the acid of the nitrate of soda, and which will be eliminated from it, is equal to 47 per cent; binoxide of manganese yields about 36½ per cent, and sesquioxide of iron 30 per cent. It thus becomes tolerably easy to ascertain, especially after a few practical trials, the proportion of oxidising materials necessary to remove a given quantity of carbon. This is, however, complicated by some conditions, each of which requires a trial or two to obtain exact results. For instance, when the operation is carried on in a deep vessel, the oxidising materials act more effectively than when a shallow vessel is used, because the products of their decomposition have more time to become saturated with the impurities which it is desirable to remove from the iron. The rate of evolution of the gases from the oxidising material is regulated by the proportion of oxide of iron, of oxide of manganese, mixed with the nitrates. These oxides, though themselves evolving oxygen, especially in the presence of carbon, do so tardily: and they restrain the otherwise uncontrollably rapid action of the nitrates alone. By this means the action of the nitrate can be so retarded as to cause only a comparatively slight ebullition. The nitrate of soda is mixed with a proportion of oxide of iron, the most suitable form of which is hæmatite, and when in a slightly moist condition, passed or tamped into the bottom of a vessel lined with fire-brick; the whole is then dried into a solid block. If the vessel has been used immediately before, its heat will be sufficient to dry the mass, but if not heated by a previous operation, it is heated by other suitable means. The nitrate of soda of commerce is generally sufficiently moist, without the addition of more water. When the mass is dry, the fused iron is poured upon it; successive layers being then removed, the materials by their levity are carried through the body of the metal, and the reactions occur, to which I have before referred; and as each layer is removed, the part immediately below is exposed to the heat of the melted metal. A boiling appearance accompanies this action, and a frothy slag containing some oxide of iron, and compounds of soda, with the impurities extracted from the iron, rises to the top. After the action has ceased, in consequence of the converting materials being expended, the steel is tapped out, and made use of in any suitable way.

Refined iron, for the manufacture of malleable iron in the puddling furnace, may be made by the use of about 3 per cent of nitrate and 6 per cent of peroxide of iron. Steel, by 8 to 10 per cent of nitrate and equal weight of binoxide of manganese. Malleable iron by 8 per cent of nitrate and 20 per cent of peroxide of iron. In each case iron with 5 per cent of carbon being used.

But it was often suggested to me that the use of separate and special apparatus is objectionable, on account of its expense, as manufacturers are generally averse to any large outlay upon new processes; and that some mode of applying it to the ordinary puddling furnace would be very useful. But there was this difficulty to contend with, the puddling furnace is too hot for the introduction of the converting materials, and fixing them at the bottom, and could this be done they would be decomposed before the fusion of the iron could be commenced, to say nothing of their remaining till it could be melted, so as to allow the gases evolved to rise and act through the fluid metal. I get over this difficulty as follows:—I make the converting materials into blocks or balls, and fix them on the ends of iron rods. These balls being made hard by drying, are ready for use. When the iron is fused in the puddling furnace, and the boil has commenced, one of these balls is pushed to the bottom of the metal in the furnace—the products of its decomposition rise through the metal, causing rapid agitation, which is much more effectual than that produced by the puddler with his tools. After the ebullition has ceased, the rod is withdrawn and another put in its place. The time occupied in puddling is thus very much shortened, the labour very much reduced, fuel saved, and a better yield of metal obtained, in consequence of the soda forming a base which readily combines with the silicic and phosphoric acids eliminated from the iron. In the ordinary puddling operations the silicium and phosphorus are extracted by the previous formation of oxide of iron, with which those acids, which are also products of oxidation, combine. But when silicium and phosphorus are reduced to a somewhat small proportion of the whole, the last traces of them are removed with difficulty, still the powerfully basic character of the soda intensifies the disposition of these substances to separate from the iron, and to enter into combination with itself.

The malleable iron produced from cast iron which has been treated with nitrates is of a very superior quality, having great range of temper. The same metal which by gradual cooling is fit for purposes requiring great toughness and powers of endurance of bending and torsion, may by rapid cooling be made sufficiently hard for wood-cutting tools; and its freedom from impurities is shown by the remarkably thin scale formed when the iron is worked by the smith, and the consequently small amount of loss in working. In this respect it very much resembles the best charcoal iron, and contrasts very remarkably with the iron made from the same "pig," but which has not been previously treated with nitrates. The presence of silicium causes a large amount of waste when malleable iron is exposed to the atmosphere at high temperatures, causing a thick, heavy scale, which must contain at least 70 per cent of iron.

PROCEEDINGS OF SOCIETIES.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, December 24th, 1867,

E. W. BINNEY, F.R.S., F.G.S., Vice-President, in the Chair.

"*On the Examination of Water for Organic Matters*," by Dr. R. ANGUS SMITH, F.R.S.

The author repeated his opinion that the mere expression organic matter had no such meaning as would allow chemists to measure the impurity of water by its amount. He went more fully into the division of the organic matter into various portions, some acting as unwholesome agents, others being entirely innocent. He said he was glad to find that other chemists were also attending to the quality as well as the quantity of the organic matter, and he insisted also on the condition of the matter being observed. He discussed the methods of Professors Frankland and Wanklyn, considered, however, that they did not supersede his own methods, which made a greater number of subdivisions. He explained the mode in which the organic matter is entirely removed from water, leaving frequently none of its elements behind, unless we include amongst them the inorganic bodies with which they were combined. The body which remains is chiefly common salt, which cannot be removed, and by which more than any other substance animal matter is to be detected in water under certain precautions. He also showed the importance of finding the amount of atmospheric oxygen in water, and its meaning; but as the paper was not concluded the notice is here left incomplete.

FOREIGN SCIENCE.

PARIS, JAN. 8, 1868.

Fluosalts of antimony and arsenic—Detection of salicine in quinine—New reaction for alkalies and alkaline earths—Method of estimating sulphur in iron.

So many contradictory conclusions have been arrived at by different investigators with regard to the fluorine compounds, that English chemists will have noticed with pleasure the mention made in my last letter of M. Marignac's research. The former researches of this chemist on the fluorides of niobium and tantalum led to the conclusion that they contained five atoms of fluorine. It appeared to him interesting to study the analogous combinations which antimony and arsenic seemed capable of forming. The hope of meeting in these compounds relations isomorphous with the fluoniobates and fluotantalates has not been realised: the question, however, still remains somewhat uncertain, by reason of the very restricted number of fluantimoniates and fluarseniates which it is possible to obtain well crystallised.

The antimonic fluoride M. Marignac has not been able to obtain crystallised; its solution, evaporated quickly in the cold, becomes syrupy. If heated, it decomposes, forming a white insoluble deposit, which is probably an oxyfluoride. By adding potash, soda, or ammonia to the acid solution of this fluoride and concentrating, crystals may be obtained. These fluantimoniates are deliquescent. Neither acids nor alkalies precipitate their solutions. The alkaline carbonates, after a considerable time, cause a precipitate in the cold—speedily upon boiling. The crystallised salts dissolved in water exhale the odour of hydrofluoric acid; by dissolving and evaporating repeatedly, several of these salts pass into the state of fluoxyantimoniates. M. Marignac has only studied the alkaline fluantimoniates, not having been able to obtain the others crystallised. The following is the analytical process adopted:—The water is determined by calcination with pure anhydrous protoxide of lead. For the estimation of the antimony and alkaline metal sulphuric acid is added in excess, and heat applied until the whole of the hydrofluoric acid is expelled. Fluoride of antimony is not disengaged under these circumstances. The residue is suspended in water, and a current of sulphuretted hydrogen passed through the milky fluid. It is necessary to digest the fluid with the reagent for a long time before filtering. The antimony is determined in the sulphide collected, and the filtered solution is evaporated, ignited, and the alkaline sulphate obtained, weighed.

The fluorine in these compounds must be estimated, at least approximately, to distinguish the fluantimoniates from the fluoxantimoniates. The following is the method which M. Marignac employs to effect this; he is aware the results it gives are not quite satisfactory:—A solution of pure sulphydrate of sulphide of calcium is prepared by passing sulphuretted hydrogen gas into pure milk of lime. For the analysis of 1 gramme of fluosalt the lime required is obtained by the ignition of 2 grammes of pure carbonate of lime. The filtered solution of the calcium sulphide is mixed with the solution of the fluantimoniate, and 1 gramme of pure carbonate of potash is added. A precipitate of fluoride of calcium and carbonate of lime results, the alkaline sulphantimoniate remaining in solution. The precipitate is treated by the method of H. Rose for the determination of the weight of the fluoride of calcium. The solution can be precipitated by dilute acid, and the antimony determined again.

Monopotassic fluantimoniate is obtained by dissolving antimoniate of potash in hydrofluoric acid, and concentrating the solution. It is anhydrous, and possesses the composition $SbFl_5$, KFl.

Bipotassic fluantimoniate is produced when a solution of the preceding salt is added to an excess of fluoride of potassium. It forms shining crystals; heated to 90°, they fuse in their water of crystallisation; becoming dry they lose water and hydrofluoric acid. The residue is not entirely soluble in water, a gummy substance remaining which retains fluorine. Analysis leads to the formula

$$SbFl_5, 2KFl + 2H_2O$$

Monosodic fluoxyantimoniate is obtained on adding carbonate of soda to a solution of antimonic fluoride containing excess of hydrofluoric acid. By concentration, the solution yields little crystals which are regular hexhedral prisms, terminated sometimes by a very acute rhombohedron, sometimes by a six-sided pyramid. The salt is very deliquescent. The determination of the antimony, sodium, fluorine, and water yielded numbers closely agreeing with the formula

$$SbOFl_3 + NaFl + H_2O.$$

Monosodic fluantimoniate results from the solution of the preceding salt in hydrofluoric acid. Crystals are de-

posited upon concentrating, which at first sight would appear to be cubes. They possess the property of double refraction. The composition of the salt is expressed by the formula

$$SbFl_5, NaFl.$$

Monammonic fluantimoniate forms slightly deliquescent acicular crystals—hexagonal prisms terminated by rhombohedrons. Analysis showed this salt to contain no water of crystallisation; the numbers obtained agreed with the formula $SbFl_5, NH_4Fl$. By adding to a solution of this compound ammonic fluoride in excess, and evaporating, rectangular plates are obtained which are the biammonic fluantimoniate. Analysis leads to the formula

$$2(SbFl_5, 2NH_4Fl)+H_2O.$$

M. Marignac finds the fluarseniates to be even more soluble than the fluantimoniates, and more difficult to obtain in the crystalline state. The ammoniacal salts are only obtainable as gum-like masses. Sulphuretted hydrogen decomposes the fluarseniates, but only slowly. At the end of two days the precipitation is not complete. They may be analysed by a method similar to that indicated for the fluantimoniates. No loss of arsenic is sustained in heating with sulphuric acid under redness. These salts are capable of preservation in the dry state, but their solutions evolve hydrofluoric acid, and then furnish by concentration fluoxyarseniates. Monopotassic fluarseniate is obtained by dissolving arseniate of potash in hydrofluoric acid. It crystallises out upon concentrating the solution. Analytical results correspond with the formula

$$2(AsFl_5, KFl) + H_2O.$$

Upon heating water and hydrofluoric acid are disengaged.

When arseniate of potash is dissolved in an insufficient quantity of hydrofluoric acid, the corresponding fluoxyarseniate is formed; it may also be obtained by acting repeatedly upon the preceding compound with water. The composition is expressed by the formula $AsOFl_3, KFl + H_2O$. Heated in a tube it melts easily, evolving hydrofluoric acid vapours abundantly. Bipotassic fluarseniate results when excess of fluoride of potassium and hydrofluoric acid are added to a solution of the monopotassic fluoxyarseniate. Analysis yielded numbers agreeing with the formula $AsFl_5,2KFl + H_2O$. Bipotassic fluoxyarseniate is produced when the preceding salt is submitted to repeated solution and evaporation; it is also formed when neutral fluoride of potassium is added to a solution of monopotassic fluoxyarseniate. Analysis leads to the formula $As_2OFl_8,4KFl + 3H_2O$.

Mr. Bœttger has discovered a reaction of great sensitiveness for alkalies and alkaline earths. He finds an alcoholic extract of the leaves of the ornamental plant known as *Coleus Verschaffelti*, possesses the property of becoming green under the influence of alkalies. To prepare this reagent, the fresh leaves are agitated with absolute alcohol mixed with a few drops of sulphuric acid, and left digesting for 24 hours; paper soaked in the tincture becomes red, and strips of this paper are the media of applying the test. This reagent is not influenced by carbonic acid, so that the earthy carbonates contained in water may be detected with it. The sensitiveness of the reagent is so great that a strip of the test-paper presented to a jet of coal gas speedily becomes green from the presence of ammonia.

M. Parrot has indicated a method of detecting the presence of salicine in the sulphate of quinine. In effecting this he takes advantage of the action of chromic acid on salicine; by his process a quantity as small as ½ per cent is discovered. To make the examination, the quinine salt is introduced with a little water into a flask, 2 c.c. of sulphuric acid, diluted with 4 parts of water are added, and 4 c.c. of a concentrated solution of bichromate of potash. To the flask is fitted a curved tube which dips into a few grammes of distilled water contained in the little flask serving as receiver. Heat is applied; at the end of three of four minutes, hydride of salicyle is produced which distils. By adding to the water in the flask a few drops of solution of perchloride of iron, a more or less deep violet colour is developed.

M. Eggertz has published a paper on a method of estimating sulphur in iron and its ores. This paper is one of great practical value, and your correspondent is engaged in making a full translation which you will receive shortly; it is, therefore, unnecessary to outline the process in this place. M. Kopp prefaced it by a few sentences eulogising M. Eggertz's services in the improvement of the quantitative methods of analysing iron.

CORRESPONDENCE.

NEW VOLUMETRIC ASSAY OF IRON.

To the Editor of the Chemical News.

Sir,—The two methods at present known, viz., that by the bichromate of potash, known as Dr. Penny's process, and that by the permanganate of potash, which are both based upon the same principle of the oxidation of a ferrous solution and its consequent conversion into a ferric one—involve the necessity (to the travelling chemist) of carrying about a large quantity of expensive and unstable standard solutions, or the trouble and inconvenience of dissolving fresh portions of the crystallised reagent, whenever required, upon the spot.

A little circumstance which occurred at Cawnpore, in 1862, suggested to me another and apparently more simple method, which I beg to recommend to chemists and assayers, especially those travelling in out-of-the-way countries. You are aware that rooms in India are floored with a substance called "chunam," which is a kind of hydrate of lime. In a room of this kind, without a carpet, I was amusing my children by showing them the beautiful deep red colour which a drop of the solution of sulphocyanide of potassium bestows upon one of peroxide of iron, and which I told them (in fun) was "the blood of the theatres." A few drops of the red sulphocyanide of iron happening to fall upon the lime floor, I observed that they were immediately decolourised, and this naturally led me to make an experiment similar to that upon which Parkes' volumetric assay of copper is based.

I dissolved some sulphate of iron, "green vitriol," in distilled water, and added a few drops of nitric acid to peroxidise the solution, to which a single drop of the sulphocyanide solution was then sufficient to impart a deep red colour. This colour I removed effectually by the addition of about half the quantity of common lime water, leaving a perfectly clear solution. I have not had time or opportunity since to carry out the experiment to a practical result by standardising a solution of lime with one of sulphocyanide of pure iron (piano wire); but I hope shortly to do so, and, in the meantime, would feel much obliged by the opinion of better chemists than myself if there is any difficulty or serious objection in the way of such a process? If not, there can be little doubt that it would form the most simple and economical method of assaying iron ores, as lime water is procurable in almost any part of the world, and the quantity of sulphocyanide of potassium required is extremely small.—I am, &c.,

W. A. Ross, Captain, R.A.

Woolwich, 28th December, 1867.

FRICTION IN VACUO.

To the Editor of the Chemical News.

Sir,—In the very interesting lectures by Dr. Tyndall, now appearing in your columns, an experiment is attributed to Sir H. Davy which was made long before his time. I allude to the friction of flint and steel *in vacuo*. We owe this remarkable experiment, not to Sir H. Davy, as stated in the lecture, but to Hauksbee, who communicated it to the Royal Society in 1705, as I have shown in my work

on "Phosphorescence," p. 204. In Hauksbee's experiments, as described in the *Philosophical Transactions*, when the receiver was well exhausted of air, then, although a more violent motion was given to the steel than before, yet not the least spark appeared to be struck from it, "but a small continued light was visible on the edge of the flint that was rubbed by the steel." On admitting the air the sparks re-appeared.—I am, &c.,

T. L. Phipson, Ph.D.

The Cedars, Putney, S.W., Jan. 4, 1868.

IS HEALTHINESS DEPENDENT ON STRATA?

To the Editor of the Chemical News.

Sir,—In the report of the meeting of the Local Board of Health of this town the following paragraph appears:—"The surveyor said that Dr. Buchanan, from the office of the Privy Council, waited upon him to make inquiries respecting the nature of the soil at Sheerness. By permission of Messrs. Ward and Brightman he had shown that gentleman the different strata forming the soil of Sheerness. Dr. Buchanan has now stated that after a careful examination he is convinced that in Sheerness there are fewer cases of consumption than in any town in England, and as a whole that Sheerness is one of the most healthy places in the kingdom." If I read correctly it seems that the healthiness of a place is dependent somewhat on the strata of the locality. Can you, or any of your readers, give me any information or the name or any work in which the subject is treated on? Ague is very prevalent here, and two medical gentlemen inform me that they always endeavour to remove, as soon as possible, all consumptive persons from Sheerness, which perhaps to a certain extent may account for the few cases of consumption mentioned by the authority in question as found in Sheerness.—I am, &c.,

John Bray.

Mile Town, Sheerness, Dec. 21, 1867.

MISCELLANEOUS.

Average Composition and Quality of the Metropolitan Waters during the Year 1867.—The following are the Returns of the Metropolitan Association of Medical Officers of Health:—

Names of Water Companies.	Total solid matter per gallon.	Loss by Ignition.*	Oxidisable organic matter.†	Hardness.		Organic and other ammonia.
				Before boiling.	After boiling.	
Thames Water Companies.	Grains.	Grns.	Grains.	Degs.	Degs.	Grns.
Grand Junction .	20·29	1·07	0·700	13·0	4·2	0·003
West Middlesex .	19·34	1·05	0·747	12·5	4·1	0·003
Southwark and Vauxhall .	19·47	0·99	0·816	12·9	4·1	0·003
Chelsea	20·20	1·19	0·794	12·7	4·1	0·003
Lambeth	19·92	1·18	0·817	12·9	4·0	0·003
Other Companies.						
Kent	27·32	0·76	0·184	16·6	7·7	0·002
New River . . .	18·45	0·86	0·403	12·5	4·0	0·002
East London . .	20·15	1·06	0·624	12·7	4·4	0·003

The fluctuation in the amounts of the several constituents have not been considerable, but the proportions are always a little above the average during the early months of the year when there is much rain, and they are below the average in the dry summer months. The Kent water is always remarkable for its beautiful blue colour when seen in large volume, on account of the nearly total absence of organic matter, it being derived from deep chalk wells.

* The loss by ignition represents a variety of volatile matters, as well as organic matter, as ammoniacal salts, moisture, and the volatile constituents of nitrates and nitrites.

† The oxidisable organic matter is determined by a standard solution of permanganate of potash—the available oxygen of which is to the organic matter as 1 is to 8; and the results are controlled by the examination of the colour of the water when seen through a glass tube two feet in length and two inches in diameter.

Quality of the Gas supplied to the City of London.—Dr. Letheby reports that in the course of the quarter which expired on the 30th November last, 669 examinations were made of the illuminating power of the gas supplied to the City; each of the examinations was the mean of ten observations, and they were made in accordance with the instructions of the Act of Parliament. The following are the results:—

Illuminating Power in Standard Sperm Candles.

	Great Central Gas.	Chartered Gas.	City Company's Gas.
Maximum	16·52	16·36	15·69
Minimum	12·02	12·57	12·00
Average	13·96	14·31	13·75

It thus appears that the illuminating power has been constantly above the requirements of the Act of Parliament, and to the extent of about two candles. In the corresponding quarter of last year, the average illuminating power of the Great Central Gas was 13·89 candles; of the Chartered Gas, 14·19 candles; and of the City Company's Gas, 14·17 candles, all of which numbers are close to the averages of the present quarter. The chemical quality of the gas, as regards the amount of sulphur contained in it, has not been so satisfactory, for, excepting the Chartered Gas, the quantity of sulphur has generally been excessive. This will be seen from the following table:—

Grains of Sulphur per 100 *cubic feet of Gas.*

	Great Central Gas.	Chartered Gas.	City Company's Gas.
Maximum	34·61	26·30	30·58
Minimum	12·74	11·35	14·19
Average	21·87	11·73	22·97

In fact, of the 61 observations made during the quarter of the City Company's Gas, 42 were found to be in excess of the quantity sanctioned by Parliament. Of the 60 observations of the Great Central Gas, 41 were in excess; and of the 61 of the Chartered Gas, only 21 were in excess. The gas of each of the Companies has been at all times free from sulphuretted hydrogen, and, with the exception of the Chartered Gas, it has also been free from ammonia. The pressure at which the gas has been delivered to the laboratory has been rarely under an inch of water.

The Value of Milk as an Article of Food.—Mr. Horsley, analyst to the county of Gloucester, in a paper on this subject, says that a milk may be of high density and yet give but comparatively little animal matter, such as cream and casein, whilst the amount of lactine retained in solution in the whey may be greater than usual; on the other hand, a sample of milk may be of lower density and yet yield far more animal matter than ordinary, though each may be perfectly genuine; the difference in the relative value of the constituents depending much on the time of year, the mode of keeping and feeding the cow, &c. He found only one degree of difference between a sample purchased at Cheltenham and a sample supplied to the workhouse, but an analysis of the two specimens shows not only a vast difference in the amount of solid matter, but also that very little reliance can be placed in any of the instruments usually employed in determining the value of milk; for the fatty matter of the milk, unlike any other aqueous solution, helps to keep up the instrument, and gives no idea of the actual density of the sample nor of its value.

MEETINGS FOR THE WEEK.

Monday.—Geographical, 8½ p.m.
Tuesday.—Photographic, 8 p.m.
Wednesday.—Meteorological, 7 p.m.
—— Society of Arts, 8 p.m.
—— London Institution, 6½ p.m.
Thursday.—Royal, 8½ p.m.
—— Chemical, 8 p.m. "On Water-Analysis," by Dr. Frankland, F.R.S. "On Isomeric Modifications of Valeric Acid," by Mr. A. Pedler.
Friday.—Royal Institution, 8 p.m. "On Faraday as a Discoverer," by Dr. Tyndall, F.R.S.

CONTEMPORARY SCIENTIFIC PRESS.

Under th heading it is intended to give the titles of all the chemical papers which are published in the principal scientific periodicals of the Continent. Articles which are merely reprints or abstracts of papers already noticed will be omitted. Abstracts of the more important papers here announced will appear in future numbers of the "Chemical News.")

Dingler's *Polytechnisches Journal.*

August, 1867.

H. GRUNEBERG, "On the Occurrence of Phosphorite in Nassau." A. WAGNER, "On the Chemistry of the Revivification of Oxide of Iron as used by R. Laming for the Purification of Coal Gas." BOTTGER, "On the Colouring Matter of the Leaves of *Coleus Verschaffelti* as a delicate Test for Alkalies and Alkaline Earths." A. SIERSCH, "On the Action of Common Salt on Zinc and Oxides of Zinc." MULDER, "On a Method of Decolourising drying Oils." "On the Use of Creosoted Lime for keeping Fruit fresh." KUHR, "A new Marking Ink." G. LUNGE, "On the Depth of the Sea."

September.

C. AUBEL, "A new Method of Estimating Dioxide of Copper in Refined Copper." A. E. NORDENSKJOLD, "On Selenium and Thallium Ores (Crookesite, Eucairite, and Berzelianite) from the Skrikerum Mine." "On the Use of Creosote for protecting Timber against the Teredo."

Comptes Rendus.

September 16, 1867.

A. W. HOFMANN, "On a new Series of Homologues of Hydrocyanic Acid." NIEPCE DE SAINT-VICTOR, "On some newly discovered Chemical Effects of Light." BERTHELOT, "On the Hydrocarbons of Coal Tar; Acenaphthene and Anthracene." PRAT, "On the Chemical Constitution of Fluorine Compounds."

September 30.

A. W. HOFMANN, "On the Preparation of Methylic Aldehyd, by passing Atmospheric Air charged with the Vapour of Methylic Alcohol over Incandescent Platinum." SECCHI, "A Resumé of the Author's Researches on Stellar Spectra." MELSENS, "On the Passage of projectiles through resisting Media." MORIN, "Remarks on the preceding Memoir." CHEVREUL, "Remarks. *àpropos* of Melsens' Paper, on Mariotte's Experiments showing that Rain Drops and other falling Bodies carry with them a certain Quantity of Air, and on his Explanation of the Mode of Action of the Trompe." A. RICHE, "Researches on the Hypochlorites, and on the Chlorides used for Bleaching."

October 7.

A. DONNÉ, "On the Production of Organized Bodies during the Putrefaction of Eggs." POZNANSKI, "On the Use of Hydrocyanic Acid as a Remedy for Cholera and other Diseases." FAA DE BRUNO, "On a Portable Mercurial Barometer." BALSAMO, "A Method of Depositing Designs in Relief by Voltaic Electricity without the Use of Stopping-out Varnish."

Bulletin de l'Académie Royale de Belgique (Classe des Sciences.)

August 3, 1867.

SCHWANN, "Report on E. Husson's Chemical and Physiological Researches respecting the Action of Alkaline Silicates on the Animal Economy." GLUGE, "Report on the same Memoir." MELSENS, "Report on the same Memoir." KEKULE, "Report on W. Körner's Paper on the Synthesis of Anisic Acid, of Methyloxybenzoic Acid, of a new Cresylic Acid, and on Paraiodobenzoic Acid." "Report on Glaser and Radziszewsky's Memoir on some Transformations of Formobenzoic Acid." STAS, "Report on the same Memoir." KEKULE, "Report on Körner's Contributions to the Determination of Chemical Position in the Aromatic Series." STAS, "Report on the above Memoir." KEKULE, "Report on H. Ronday's Memoir on Homotartaric Acid." STAS, "Report on H. Ronday's Memoir on Itamalic Acid." "A. KEKULE, "On the Sulphophenic Acids." E. HUSSON, "Researches on the Action of Alkaline Silicates on the Animal Economy." W. KORNER, "Note on the Synthesis of Anisic Acid, of Methyloxybenzoic Acid, of a new Cresylic Acid, and on Paraiodobenzoic Acid." GLASER and RADZISZEWSKY, "On some Transformations of Formobenzoic Acid." W. KORNER, "Contributions to the Determination of Chemical Position in the Aromatic Series." H. RONDAY, "Note on some Salts of Itamalic Acid." "Preliminary Note on Homotartaric Acid."

Journal für Praktische Chemie.

September 24, 1867.

F. STOLBA, "On Silicofluoride of Rubidium." On Crystallised Silicofluoride of Copper." F. ROCHLEDER, "On Aescigenine, and on two allied Substances—Caincine and Chinovine." A. CLAUS and C. REESE, "On Neurine and Sincaline." "R. OTTO and H. OSTROP, "On the Action of Chlorine on Sulphobenzide." H. VOHL, "On some hitherto unknown Properties of pure Naphthalin." "On the Detection of Naphthalin." "On the Preparation of Sulphide of Copper and Ammonium." C. WINKLER, "A Method of Preparing Hydriodic Acid." K. HAUSHOFER, "On some Malacolite from Gefrees, Bavaria." "On some Glauconite from the *Cénomanien* at Havre." F. REINDEL, "On Prussian Blue." "On some Compounds of Ferrocyanides and of Ferricyanides." A. FROEHDE, "On the Part which Nitrite of Ammonia plays in Nature."

Bulletin de la Société d'Encouragement.

August, 1867.

G. DE CLAUBRY, "Report on P. Havrez's Improved Arrangement of Lixiviating Apparatus in which a Single Distributing Cock is employed." SALVETAT, "Report on Brianchon's Pearl Glaze for Glass and Porcelain." ASSMUS, "On the Manufacture of Caramel." J. STINDE, "On the Manufacture of Formic Ether. J. FEUQUIERES, "On the Electrolytic Deposition of Tin on Lead which will bear Rolling." "On the Electrolytic Deposition of Iron of Great Hardness." TROOST, "On Obtaining Steel from Cast Iron by the Action of a Current of Oxygen.

Journal des Fabricants de Papier.

September 1, 1867.

E. BOURDILLIAT; "On Testing the Chemical Products used in Paper Making. (Continuation.) Bichromate of Potash. Acetate of Lead. Litharge. Chlorides of Tin." "On the Use of Sulphite of Lime as an Antichlore.

No. 18. September 15.

E. BOURDILLIAT, "On Testing the Chemical and other Products used in Paper Making. (Continuation.) Animal and Vegetable Fibres."

Bulletin de la Société Industrielle de Mulhouse.

August, 1867.

J. THOMAS, "On a new Colouring Matter derived from *Sericographis mohitli.*

NOTES AND QUERIES.

Mordant for Green on Cotton.—Could you kindly inform me through your CHEMICAL NEWS what is the best mordant for dyeing the new greens on cotton yarns?—W. T. C.

Bleaching Calico.—Can any one inform me, how far in bleaching calico, the bleaching powder solutions are exhausted? Can they be refreshed with new powder *ad infinitum*, or must they be thrown away when a certain quantity of fabrics has passed through them? If so, is there any practical rule for guiding the manufacturer, such as the observation of the specific gravity of the solution? It appears some bleachers exhaust their solutions much more than others.—G. L.

Deodorising Petroleum.—Your correspondent O. P. A., desires to receive information on the preparation of "plumbate of soda," its use and application in deodorising petroleum; when oxide of lead or litharge, is added to a pretty concentrated solution of caustic soda, the oxide of lead is dissolved therein, and may be considered to play towards the soda the part of an acid; the clear solution may be used with advantage to deprive petroleum of some foul smelling compounds it may happen to contain, especially as sometimes occur organic sulphur compounds, by thoroughly shaking and mixing the petroleum with the plumbate of soda, and afterwards giving sufficient time for the liquids to separate in two layers; the upper layer being the petroleum, the latter will have to be washed with water to remove the adhering soda, and should then be deprived of moisture by applying lumps of caustic lime.—Dr. A. A.

TO CORRESPONDENTS.

S. Scott (Calcutta).—Your letter has arrived. There will be great difficulty in getting what you require, but we will try and send it in the course of a mail or two.

T. Sterry Hunt (Montreal).—Arrived too late for insertion in this number. It shall appear next week.

S. Maldon.—You must use the best charcoal iron. "Pig" is too impure.

Physicist.—The exact equivalent of heat is 772 foot-pounds, according to Joule's most recent researches. The probable error is considerably less than 1 lb.

W. Murray.—Dr. Phipson has detected the presence of xanthic oxide in guanos which contain no uric acid.

Querist.—The colour of green pickles may be greatly improved by previously boiling the vegetables in water containing a quarter of an ounce of liquid ammonia to the quart.

Carbo.—Permanganic acid is volatile but very unstable. You will not be able to make use of this property in separating manganese quantitatively from iron.

Exhibit.—The list of jury awards in class 2, section A, was given in the CHEMICAL NEWS, vol. vi., p. 62, *et seq.*

Dr. Wilhelmi.—The price of the German edition of "Gerschichte der Chemie," by Dr. T. Gerding, is 9s.

W. Davies.—The best practical information which we know of on the subject of grinding lenses is given in a little work published by Secretan, of Paris, entitled "De la distance Focale des Systémes Optiques Convergents." The price is about 2s. 6d., or 3s.

Communications have been received from J. Heywood; A. Stark; L. Power; F. C. Calvert & Co. (with enclosure); Townsend & Adams; E. A. Parnell; Prior of the Monastery of St. Joseph; G. Lunge; Kingsbury & Co.; Dr. Adriani; C. J. Woodward (with enclosure); P. J. Worsley; Runcorn Soap and Alkali Co. Lim.; S. Rowland (with enclosure); M. A. Gage (with enclosure); Capt. W. A. Ross, R.A. (with enclosure); A. W. Wilson; A. M. Scott (with enclosure); W. Briggs (with enclosure); Dr. Quesneville; S. Scott; Molson, Bros. (with enclosure); Montgomerie & Greenhorne; T. Hill (with enclosure); H. R. Marsden (with enclosure); R. C. C. Lippincott; F. C. Clayton (with enclosure); T. Fisher (with enclosure); Professor Frankland, F.R.S.; Professor W. A. Miller, V.P.R.S. (with enclosure); J. Stubbins; Asher and Co.; T. Sterry Hunt, F.R.S. (with enclosure); J. Walker; W. H. Exall; C. R. A. Wright; W. Exmore; C. Richter; Professor Weltzein (with enclosure); Davenport & Co.; E. C. Hogg (with enclosure); E. P. H. Vaughan; Mottershead & Co.; J. C. Bell (with enclosure); Rudolf Schomberg (with enclosure).

Books Received.—"Boiler Deposits," by Dr. T. L. Phipson, F.C.S.; "Bulletin de la Société d'Encouragement;" "American Artisan;" "The Oriental Mail and War Office Gazette;" "American Journal of Mining; "Action of Sunlight on Glass," by Thomas Gaffield; "Scientific American."

Recent Improvements.—People do not as a rule take notice of the improvements and changes that take place in the small details of daily life—the changes from stage coaches to railways, the telegraph, the penny post, and many other great and startling novelties are all noticed and commented upon, and they each and all, more or less, impress us; but if we extend our observations, and recall the domestic appliances of a few years ago, and compare them with the present, we shall find plenty of subjects at any rate worthy of passing reflection. Take, by way of illustration, a common and every-day requirement—Candles—and refer to our advertisement columns, we shall be able to mark—within the experience of those not past the prime of life—great and startling changes. Thirty or forty years ago, wax and spermaceti candles lighted the upper ten thousand, and the masses accepted snuffer-requiring tallow candles as the only means of lighting open to them; now we advertise an almost bewildering variety of lights. Price's Patent Candle Company (which, under the name of Edward Price & Company, is the father of the candle trade). some thirty or forty years back. began by introducing cocoa-nut candles as a substitute for strong smelling tallow; then they invented composite candles, and to-day they advertise in our columns Belmontine, Price's Paraffine, Battersea Paraffine, Gold Medal Palmitine, Sherwood Palmitine, Belmont Wax and Belmont Sperm Candles, Best Composites, &c., &c. A good housekeeper is now bound to discriminate the advantages and prices among such an *embarras de richesses*, and our middle classes can now, at a reasonable price, be lighted as well as, or better than, princes were forty years ago.

An Introduction to Chemical Philosophy, according to Modern Theories, by ADOLPHE WURTZ, F.R.S.

Translated and Edited by WILLIAM CROOKES, F.R.S.

"A little work of singular merit, and appearing at a most opportune period; it gives a remarkably clear *exposé* of the changes taking place in chemical nomenclature, with the reasons for their adoption."—Medical Times and Gazette.

London: J. H. Dutton, Chemical News Office, Boy Court Ludgate Hill. E.C.

Scholl's Patent Platinum Gas Light Perfecter.—Extract from Report by Dr. Letheby:—

"The results have been very remarkable, for they show an average increase of 63 per cent on the illuminating power of the gas. I am of opinion, therefore, that the invention is of great practical value.' Price One Shilling each for Fish-tail Burners. To be had retail of Gas-fitters and Ironmongers, and (wholesale only) of JOHN SCHOLL, 41 and 42, Berwick Street, Oxford Street, London, W. Terms on application. N.B.—A specimen sent free on reccipt of 12 stamps.

PATENTS.

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Instruction in Practical Chemistry and Evening Classes for the Study of Chemistry, Botany, Materia Medica, &c.

TO PHARMACEUTICAL AND OTHER STUDENTS.

Mr. J. C. BRAITHWAITE, for thirteen years Principal Instructor in the Laboratories of the Pharmaceutical Society of Great Britain, and Demonstrator of Practical Pharmacy, Pharmaceutical Latin, &c., wishes to inform his old Pupils and others that he continues to devote his whole attention to Education. The Session 1867—1868 will commence on the 1st of October, when

Mr. BRAITHWAITE'S Laboratory, which has been enlarged during the recess, will be re-opened at 10 a.m. for Instruction in Practical Chemistry as applied to Pharmacy, Medicine, Analysis, &c. Pupils can enter at any period. Terms moderate.

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MEDAL PARIS EXHIBITION, 1867.

THE CHEMICAL NEWS.

Vol. XVII. No. 424.

ON THE

CHEMICAL GEOLOGY OF MR. DAVID FORBES.

By T. STERRY HUNT, F.R.S.

In the Chemical News of October 4th, 1867, there appears a paper purporting to be a criticism of some views on the chemistry of the primeval earth, put forward by me in a lecture delivered before the Royal Institution of Great Britain, on the 31st of May last, and published in the Proceedings of that Institution, as well as in the Chemical News of June 21st, and the *Revue des Cours Scientifiques*, of October 19th, and also in *Les Mondes* and *Cosmos*. The object of my present communication will be to notice briefly some of the criticisms of Mr. Forbes. The readers of my lecture are aware that I assumed as my starting point the hypothesis, now generally accepted, of the origin of our earth, and of all planetary and stellar worlds, by a process of condensation and cooling from a nebulous or a gaseous matter, so intensely heated as to be luminous, and to contain, at the same time, in a free or dissociated condition, the various chemical elements. The first objection of Mr. Forbes, is that I do not explain the origin of this intensely heated condition : a consideration entirely beyond the scope of my lecture, but established by the spectroscope, and to be accepted as an ultimate fact, the secret of which, like that of the origin of matter itself, rests with the Great First Cause.

In discussing the laws which presided over the cooling of our own globe, I gave several reasons which have led modern investigators to reject the old theory of a liquid centre covered by a thin crust of congealed rock. I alluded briefly to the mathematical deductions of the late Wm. Hopkins from the phenomena of precession and nutation,—those of Archdeacon Pratt on the feeble resistance which would be offered by a crust of the thickness generally admitted by the old school, to the crushing weight of masses like the Himmalayah Mountains,—and the conclusions of Thompson as to the rigidity of the earth, deduced from the theory of the tides, as so many concurrent arguments in favour of a crust at least many hundred miles in thickness, if not of a globe entirely solid. Proceeding, thence, to consider the conditions of cooling presented by the fused and oxidised mass of the globe, I asserted that the analogies offered by most of the bodies forming the earth's crust, which yield compounds considerably denser when solidified than when in their fused condition, lead us to conclude that the solidification of the globe must have begun from the centre. In fact, the numerous and detailed experiments of Charles Deville (*Comptes Rendus*, xx., 1453), and those of Delesse (*Bull. Soc. Geol. de Fr.* [2] iv., 1380), not to mention the earlier ones of Bischof, unite to show that the density of fused rocks is much less than that of the crystalline products resulting from their slow cooling, so that, as Saemann has justly observed, we are forced to conclude that the crystalline stony masses formed at the surface of a liquid globe must sink towards the centre (*Ibid*, Feb. 4, 1861). To this conclusion Mr. Forbes objects that, in the cooling of sulphur or metals from fusion, a crust forms at the surface before the interior is solidified; he should consider that the conditions in a small crucible, placed in a cold atmosphere, where cooling is rapid, and the crust is supported by adhesion at the edges, are vastly different from what would obtain in a world-wide bath, cooling with great slowness beneath an intensely heated atmosphere. In such a case, as the crystalline silicates known to us, are, according to numerous experiments, from one-seventh to one-sixteenth denser than the same materials in a fused condition, it would require a suspension of the laws of gravity to counteract the inevitable tendency of the heavier solids formed at the surface to sink in the fused mass, in which they would subside as naturally as the crystals which form at the surface of an evaporating basin of brine. The analogy holds good since the crystals formed at the surface, whether by evaporation or by cooling, obey the laws of gravity. The freezing over of the surface of such a mass would be as unnatural as the freezing of a lake of water from the bottom.

Mr. Forbes next comments upon my allusion to the experiments of Hopkins on the effect of pressure in elevating the melting points of such bodies as contract in cooling, and says that I appeal to these as conclusive proof that the melting points of bodies do become (*ad infinitum*) elevated in proportion to the pressure. In fact I said nothing of the sort, but insisted that the researches of Hopkins "are to be considered in this connection." If Mr. Forbes had taken some pains to inquire into the question, he would learn that these experiments of Hopkins, and others (by W. Thompson on the effect of pressure in reducing the melting point of ice) were suggested by a remarkable essay by James Thompson (Trans. Roy. Soc. Edin. xvi, part 5). In this it was shown that the fusing point of ice, which contracts in melting, must necessarily be reduced by pressure; while, as Sir Wm. Thompson showed, the reverse effect was to be expected for all solids which expand in melting (L. E. and D. Phil. Mag. [3] xxxvii. 125). The results of Hopkins thus come under a general physical law. Mr. Forbes will find a simple and intelligible statement of the principle laid down by Thompson, and Hopkins's argument therefrom, for the solidity of the interior of the globe, in the fourth of Dr. Tyndall's admirable lectures on Heat, delivered before the Royal Institution. See also Mr. Sorby's Bakerian Lecture for 1863. As to Mr. Forbes's suggestion of denser matters towards the earth's centre, I have said the same thing in my lecture.

Mr. Forbes next proceeds, in his own words, to submit my views of the chemical changes which took place at the surface of the globe, to "careful scrutiny," in order to determine whether "they are sound and likely to meet with acceptance in the chemical world." Of the critic's fitness for his self-imposed task the reader shall judge. The first thing to be determined in the cooling of an intensely heated vaporous mass is the nature of the chemical compounds which would be formed among the dissociated elements. As I have stated in my lecture, the combinations stable at the elevated temperature then prevailing, would be first formed. The affinities of oxygen are such, that under such conditions, an excess of this element being present, instead of sulphides of the heavy metals, as imagined by Mr. Forbes, oxides and sulphurous acid would be produced in virtue of affinities know to every chemist and metallurgist. So with regard to chlorine, the production of alkaline chlorides in such conditions is inconceivable, since in the conjoined presence of oxygen, hydrogen, and silica, an alkaline silicate and hydrochloric acid would necessarily result. Even if, as Mr. Forbes supposes, chloride of sodium were to be formed in the heated atmosphere, it would be precipitated into an intensely heated bath of fused silicates, covered by an atmosphere charged with aqueous vapour, or with mingled hydrogen and oxygen, and would immediately undergo the same decomposition that takes place when the vapours of common salt are diffused through the heated atmosphere of a potter's kiln, or, as in Mr. Gossage's new soda-process, are passed with steam over red-hot flints. In both cases silicates of soda are formed, with separation of hydrochloric acid. These considerations lead to the conclusion that, after all the more fixed elements were precipitated, the whole of the chlorine would remain as hydrochloric acid, and the whole of the sulphur as sulphurous acid, together with a large pro-

portion of oxygen, since we find sulphates and not sulphites in the sea-waters. To this constitution of the still intensely heated atmosphere, Mr. Forbes objects, and inquires whether it is "at all probable, even if possible, that an excess of oxygen could exist along with the vast amount of sulphurous acid." He farther adds that "the improbability of such an atmosphere containing a mixture of hydrochloric and sulphurous acids, may be inferred from Dumas's researches; that chemist having long ago shown that these two gases, when mixed together, react and mutually decompose each other, with the formation of water, chlorine, and sulphur." Mr. Forbes thinks he has hit upon two objections to the existence of a heated atmosphere holding, as I have endeavoured to show, besides nitrogen, oxygen and watery vapour, sulphurous and hydrochloric acids. He has evidently a vague notion that sulphurous acid and oxygen have an affinity for each other, and ought to form together sulphuric acid. So they do unite slowly at proper temperatures, in the presence of water, being converted into oil of vitriol, and it was doubtless in this way, as I have elsewhere shown, that the sulphur was eventually brought down from the atmosphere, and formed the sulphates of the sea. But every chemist is aware that at higher temperatures oil of vitriol is resolved into water, sulphurous acid and oxygen gases, and that this reaction is made use of as an economical process for the preparation of oxygen on a large scale, the sulphurous acid being removed by absorption from the cooled gases. As regards his second point, Mr. Forbes, who cites Dumas (Traité i. 146) has been misled by quoting at second-hand, apparently from the English edition of Gmelin (ii. 321). Dumas states that in solution sulphurous acid and hydrochloric acid undergo no change; but, "in a dry state, on the contrary, they are rapidly decomposed, at least in operating over mercury." It may be true that as Gmelin states, water, chlorine, and sulphur result, but such is not the assertion of Dumas. The point, however, is immaterial, since as Dumas and Gmelin state, and as every chemist knows, the two gases remain unaltered in the presence of water, even if in the form of vapour. Indeed, it happens, unfortunately for both of Mr. Forbes's objections, that large quantities of precisely such an atmosphere as he supposes to be impossible, are disengaged from numerous volcanic vents, as he will find by referring to the researches of Charles Deville and Leblanc. (Ann. de Ch. et Phys. [3] lii. pp. 5—63). Among other examples described by these chemists, a *fumerolle* of Vesuvius yielded in June, 1856, a mixture of highly heated steam, hydrochloric acid and air, the latter containing in 100 parts, oxygen 18·7, sulphurous acid 2·6, the remainder being nitrogen; while the acids of the steam and air together yielded, for one part of sulphurous acid, about five parts of hydrochloric acid. Traces of sulphuric acid, due to the slow union of the sulphurous acid and oxygen, were found in the water condensed from this fumerolle. Volcanos, as I have elsewhere stated, reproduce, on a limited scale, the conditions of the primeval earth, not only in their solid but in their gaseous products.

Mr. Forbes proceeds to comment upon my illustration of the condition of the primitive globe from a supposed reaction of the present air, sea and land under the influence of intense heat. He suggests that the carbonaceous matters would convert into sulphides the mineral sulphates. Here, as before, he ignores the intervention of water and siliceous matters, which would cause the sulphur to escape in the form of sulphuretted hydrogen (which is doubtless evolved from modern volcanos by a similar reaction), and this at an elevated temperature, would at once be burned to sulphurous acid and water. He descends to trifling when he objects that by the effect of heat upon the present surface of the globe, the water of the sea would be first evaporated, and then the chloride of sodium sublimed in its turn. It was made clear to every reader, that I never intended by this illustration to represent the process of nature; moreover, I said, "*if the elements were made to react upon each other*," which would not be the case if they were successively removed by evaporation beyond the sphere of reactions.

Here I cannot resist the temptation of giving my readers a choice specimen of Mr. Forbes's chemistry, which he has embodied, with many other surprising things, in a further criticism of my lecture, which appears in the *Geological Magazine* for October, but has, for some unknown reason, withheld from the readers of the Chemical News. Proceeding to give his own notions of the chemistry of the primitive globe, Mr. F. supposes that immediately above the "solidified crust," there existed a zone composed chiefly of chloride of sodium; "above this, a stratum of carbonic acid, and then of water in the form of steam, whilst the oxygen and nitrogen would be elevated still higher;" and, probably, also, in Mr. F.'s imagination, separated according to their densities. In explanation of this order, he tells us, in a note, that the zone of carbonic acid gas would be heavier than that of steam, and should therefore come below it; he even gives their respective weights, but he forgets that oxygen and nitrogen are also both heavier than steam, and should be found below, and not above, this zone of watery vapour. In fact, as is well known, the specific gravity of oxygen being 1·109, and nitrogen, 0·970, that of atmospheric air is 1·000; while carbonic acid gas is 1·525, and that of watery vapour, 0·624. But, apart from this absurd mistake, what shall we say of the man who displays an utter ignorance of the laws which govern the diffusion of gases and vapours? Will Mr. Forbes explain why it happens that in our present atmosphere, these same elements, namely, oxygen, nitrogen, carbonic acid gas, and watery vapour, are commingled, instead of being, as he would have them, arranged in separate zones?

Mr. F.'s mode of explaining the saltness of the sea must fall to the ground, unless he succeeds in showing how, despite well-known chemical affinities, the requisite amount of chloride of sodium could be formed and preserved under the conditions which I have discussed above, so that, as he supposes, it was ready to be dissolved by the first waters precipitated on the surface. When he has satisfactorily established this part of his theory, he will, perhaps, tell us how sulphates found their way into the sea, if, as he asserts, all the sulphur was at first separated in the form of dense metallic sulphides, which sank at once, "and remained in the interior of the earth, protected from oxidising action?" Mr. F. may have data unknown to the world, for estimating the total amount of sulphur in the globe; but when he tells us that it would be sufficient to convert all the soda of the sea to sulphate, he reasons as if the amount of bases in nature were limited, forgetting that the earth's crust contained more than enough of alkalies, lime, and magnesia, to saturate the acids of the primeval atmosphere, and, moreover, that the whole of the sulphur, sulphates, and sulphides of the earth's crust, have, to judge from all analogy, been derived from the soluble sulphates of the ocean.

Mr. F. next proceeds to enquire why the sea contains so much sodium, and so little potassium? If he will study the question, as he may do in my *Contributions to the Chemistry of Natural Waters* (Amer. Jour. Sci. [2] xxxix. 176; xl. 43, 193.), he will learn that at an early period the salts of calcium and magnesium greatly predominated over those of the alkalies in the ocean waters, precisely as they must have done in the crust of the primitive earth. It is by subsequent subærial decomposition that have been liberated the alkalies, which, in the form of carbonates, have decomposed the salts of the primitive sea, and substituted sodium for calcium, for it is well known that natural alkaline waters convey to the sea chiefly soda, and comparatively little potash, which is retained by argillaceous sediments. Moreover, the potash which does find its way to the sea, is constantly withdrawn in the form of glauconite, and also by the agency of fucoids,

which, as Forchammer has shown, fix great amounts of potash, and, subsequently, by their decay in the ooze, restore it to the earth.

Mr. Forbes next expresses surprise that I find the origin of all carbonate of lime (except that from the subærial decomposition of primitive calcareous silicates) in the reaction of carbonate of soda on the lime-salts of sea-water, since, according to him, the results of the careful study of limestone rocks by geologists, palæontologists and microscopists have shown these rocks to be "*the result of organic action.*" And, moreover, that neither chemists nor zoologists will accept my assertion that *animals can only appropriate the carbonate of lime which they find ready formed*, but "will consider these animals capable of utilising the other lime-salts in the sea." If we admit the power of the lower animals to decompose chloride of calcium or sulphate of lime, as would appear from the acid liquid said to be found in some of them, will Mr. Forbes tell us what becomes of this at the death of these animals, and how the acid is to be disposed of? If the thousands of feet of limestone strata, consisting in large part of organic remains, have been derived from the decomposition of the sulphate or chloride of calcium of the sea by any other process than by that which I have indicated, namely, the intervention of alkaline carbonates, will Mr. Forbes kindly inform us what has become of the vast amount of hydrochloric acid equivalent to all this carbonate of lime?

As to the origin of dolomites Mr. Forbes will do well to read my paper in the *Amer. Jour. Science*, for July, 1866 ([2] xlii, 49). In this, at § 112, he will see that apart from the formation of stratified sedimentary dolomites, I insist upon the frequent occurrence of dolomite as a mineral of secondary deposition, lining drusy cavities, filling veins, and even the moulds of fossil shells. To such cases, the observations of Sorby and of Harkness may probably be referred; the microscopical investigations of the former, as given by him in the British Association Report for 1856, are like all the other works of that excellent observer, doubtless entitled to the highest credit. No one, however, who has carefully studied, as I have done, the distribution and association of the great beds of dolomite which occur in the Lower Silurian rocks of Canada and New England, can, for a moment, admit that they are the products of subsequent alteration. Repeated alternations of pure blue lime-stones with reddish ferruginous dolomites, interrupted beds and patches of these enclosed in the former, the line of demarcation sharply drawn, and finally conglomerates in which pure limestone pebbles are enclosed in beds of dolomite, all of which may be studied near Quebec, are evidences incontrovertible against the theory of dolomitization of pure lime-stones, and in favour of the deposition of dolomites as magnesian sediments.

Mr. Forbes insinuates that I am unaware of the various speculations and theories which have been put forward to explain the supposed origin of dolomites by alteration. Although the stratigraphical relations of dolomites, as described above, set aside entirely this hypothesis of its formation, at least in the great majority of cases, Mr. Forbes will find that the observations and speculations of Haidinger, Von Morlot, Marignac. and others on this subject, have been discussed and made the subject of multiplied experiments by me in a memoir published in 1859 (*Amer. Jour. Sci.* [2] xxviii. 170, 365), and farther in the paper quoted above; and that I have shown that the reaction of the sulphate of magnesia on carbonate of lime, to which he refers, does not give rise to dolomite, but to an admixture of the carbonates of lime and magnesia.

Some of the results of my prolonged study of certain salts of lime and magnesia, which are for the most part set forth in the papers just referred to, were, says Mr. Forbes, by me considered worthy of being presented to the French Academy of Sciences (*Comptes Rendus*, April 22, 1867), although he declares the reactions there described to have been for twenty-five years in general application on a large scale in Great Britain, for the manufacture of magnesian salts. Here it becomes difficult to admit the plea of ignorance which suggests itself for most of Mr. Forbes's previous statements. I have in the note to the French Academy above referred to, pointed out the following as facts discovered by my investigations of the salts of lime and magnesia: 1st. That bi-carbonate of lime, at ordinary temperatures, decomposes solutions of sulphate of soda and sulphate of magnesia, with formation of sulphate of lime and bi-carbonates. 2nd. That from mingled solutions of sulphate of magnesia and bi-carbonate of lime, there separates, by evaporation, crystalline gypsum, and subsequently a hydrous carbonate of magnesia; the bi-carbonate of this base being, as is well known, very much more soluble than the sulphate or the bi-carbonate of lime. 3rd. That this separation of gypsum is favoured and rendered more complete by an atmosphere impregnated with carbonic acid gas; and 4th. That mixtures, in due proportions, of precipitated carbonate of lime and hydrous carbonate of magnesia, when gently heated under pressure, and in the presence of water, unite to form the anhydrous double carbonate, dolomite. These are the reactions which I described to the French Academy as *new*, and I demand Mr. Forbes to make good his assertion to the contrary, or to show that any one of them has been employed for the last twenty-five years in the manufacture of magnesian salts.

Montreal, December, 1867.

ON

HEAT AND COLD;

A COURSE OF

SIX LECTURES*

(*Adapted to a Juvenile auditory*),

DELIVERED AT

THE ROYAL INSTITUTION OF GREAT BRITAIN,

(*CHRISTMAS*, 1867—8),

BY

JOHN TYNDALL, Esq., LL.D., F.R.S.

Lecture III.

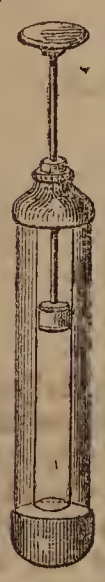
Winds and breezes—Ice, snow, and glaciers.

In the last lecture I showed you the change which takes place in water when it is gradually cooled; and I showed you in a very striking manner that water when it freezes and becomes ice, expands, and that the force of the expansion is so great as to burst the bombshell which was placed before you in the last lecture. Now follow me for a moment, please. Conceive water at the ordinary temperature; conceive it growing gradually colder and colder. Like almost all other bodies it becomes smaller and smaller; it shrinks as it becomes colder; but at a certain point, and some time before it turns into ice, it leaves off contracting. Suppose the water to go down from a temperature of 60°: it continues contracting until it reaches the temperature of 39° Fahr., or 4° Centigrade; and then the water instantly ceases to contract, and 7° F. before it becomes solid it begins to expand as it becomes colder. What is the consequence of this expansion? The water from 39° Fahr. downwards, becomes lighter, and it swims like oil over the surface of the water underneath, and there it is frozen; and when it freezes—when it passes from the liqdid state to the solid state—a sudden and very great expansion occurs, so that eight volumes of water weigh about as much as nine volumes of ice, the ice being the lighter of the two, and therefore swimming upon the water.

* Reported verbatim, by permission of the Author, for this journal.

I must ask you now to accompany me for a moment to some of the things that occur in nature in connection with this subject of heat. You know that at certain parts of the earth's surface the heat is very much more powerful than it is here in England; and you know that the reason of this is that at certain parts of the earth's surface the sun is overhead, and its rays come vertically downwards, and thus heat very much the surface of the earth directly underneath the sun. In the region of what is called the Equator we know that the sun is directly above the heads of the people living there and at a certain distance on each side of it. Now, imagine this sun pouring down its heat through the atmosphere upon the sea. The surface of the sea is thereby warmed, a quantity of vapour is produced, and that vapour ascends with the air into the higher regions. When the surface of the earth at the equator is heated, the air also at that point becomes heated, and rises, as the air of this room rose from the surface of that heated spatula, in the last lecture. When the air at the Equator is heated by the sun, part of it goes towards the North Pole and part towards the South Pole, while underneath air rushes in from the other directions to supply the place of the air which goes to the north and south. If you could see the air you would see it going one way and coming back another. A continuous circulation is thus going on, and the winds that are produced in this way have a particular name given them. They are called the "trade winds." The current above is called the "upper trade wind," and the current beneath is called the "lower trade wind." Now, as I have said, when the sun's rays act upon the ocean they convert its water into vapour, and this vapour is carried up into the air. What is the consequence? I want to show you one or two facts that will enable you to understand what must occur.

The first fact that I wish to show you is that if we compress air suddenly we develope heat; and I do this by means of the syringe that I have here. This is a small glass tube bored very carefully, and furnished with a piston that fits air-tight into that glass tube; so that if I squeeze this piston down I compress the air underneath it. Now, here I have a piece of German tinder which I place in a little cavity made at the bottom of the piston; and I think I shall be able to ignite that German tinder by forcing down the piston and thus compressing the air. [The tinder was ignited as described.] Now, what we have done here is, indeed, nothing more than simply throwing the atoms (as we have agreed to call them) of the air into this intense state of vibration to which we give the name of heat. On the other hand, if we take a body having a certain amount of heat, and, instead of compressing the air, allow it to expand, then the expansion of the air produces cold. I will show you one effect of this expansion of air. I have here condensed in this vessel—forced in by a kind of syringe—a great deal more air than the vessel would contain naturally; and if I were simply to turn this cock and allow the air to issue from the vessel against an air thermometer, I should produce an effect which would, perhaps, be visible to my young friends immediately before me. If cold is produced in this way the column will rise a little. I will now turn this air out against the thermometer. The column has risen a little, which proves that the air which has come out of this vessel, and become expanded, has become chilled. A great man who used to lecture in this room many years ago—Sir Humphry Davy—described a machine which he saw at Schemnitz in Hungary, formed so as to allow a very strong current of compressed air to issue from it, and the amount of cold produced by the expansion of the air was such as to cause the vapour of the atmosphere to condense and congeal and form icicles. Now, I want you to remember that when air is condensed in the way I have described heat is developed, and that when an expansion of the air takes place an opposite effect is produced. Mr. Cottrell has here arranged a little experiment, but as I do not know whether it will be visible or not to you all, I will tell you what it is. This glass receiver contains air, and within is a small elastic balloon which also contains air. The air which the balloon has within it has a certain amount of heat, and in virtue of that heat it has a certain power of squeezing out the sides of the balloon. If we now pump the air out of the outer vessel, and so remove the air from the outside of the balloon, we take away the force which counteracts the force inside this balloon. It will then expand and almost fill the entire vessel. [The air was then exhausted by means of an air pump.] You see the balloon becomes larger and larger. You see it growing visibly before you, and the air within this balloon at the present time is being chilled because of its expansion. The assistant will go on pumping out the air from the glass receiver, and after a time the balloon will almost fill the receiver. It thus goes on swelling and swelling, the air within it expanding, and this air, by the act of expansion, becomes chilled. We will now allow the air to enter by turning this cock, and then the balloon will shrink to its first dimensions. See how small it becomes, because we get a pressure on the outside of the balloon squeezing it inwards, until now it is finally reduced to the same size that it had at the commencement. Mr. Cottrell will now remove that balloon altogether, as I want to show you what takes place within that receiver when the air is thus taken out of it. I want to show you the effect of the chilling produced by the rarefaction or expansion of the air in nature. But first I will tell you the effect produced on a body of air rising, we will say, from the surface of the sea to a certain height above it. We will take a definite height, such as we often find in the Alps—11,000 feet, the height of one of the higher Alpine passes. Conceive, then, a body of air rushing up the mountain, and going to the top of that pass. In climbing up this 11,000 feet the air gets into a place where it is not so much pressed upon as it was below. A portion of the atmosphere has been removed from above it, and the consequence is that the rising air expands, and the expansion is followed by a lowering of its temperature. The air becomes colder, and if it had in it as much moisture as it could hold, it would, in rising 11,000 feet, fall very nearly 40° Fahrenheit in temperature.



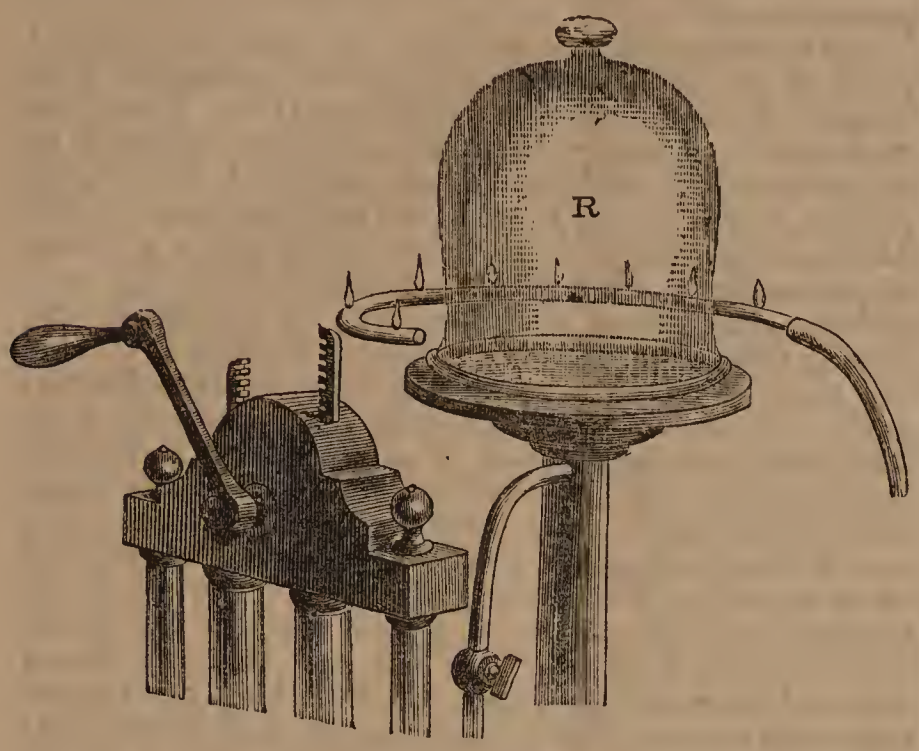
(Fig. 1.)

Now, you must remember that in order to preserve the vapour of this room in an an invisible state, a certain temperature is necessary. If you could at this moment introduce into this room the temperature of the polar regions, what would you obtain? First, the air of the room would thicken so as to form a fog, and then that fog would be chilled and fall as snow. This has occurred over and over again in Russia and elsewhere. So if you could only get the temperature of this room low enough you would see the now invisible aqueous vapour falling as snow. Even in London ball-rooms this may sometimes be observed. When the windows have been opened in the intervals of the dances the air has immediately become cooled, and a condensation of the vapour has taken place sufficient to make the atmosphere dim. Now imagine air charged with invisible vapour being carried up one of these high Alpine passes. If in this way it gets its temperature reduced to 32°, the air can no longer hold its vapour, that vapour then falls as snow, and that snow is deposited on the tops of the mountains.

I want now to show you how clouds are formed by the condensation of vapour. Here we have the receiver of our air-pump, enclosing a quantity of air which is charged with invisible aqueous vapour. Mr. Chapman will now place a lamp behind this glass receiver. I will send a beam of light through the receiver, and let it fall on the screen. At first you will not see any appearance of anything inside the receiver. I will then ask Mr. Cottrell to work the air-pump, and exhaust some of the air, and thus cause the remaining air to expand. This will reduce its temperature, and then you will see that the vapour within the receiver will become a fog. You now see no sign of any thing within the receiver; but we will now

exhaust the air. [The air-pump was then put in action, and a condensation of the vapour became immediately manifest.]

FIG. 2.



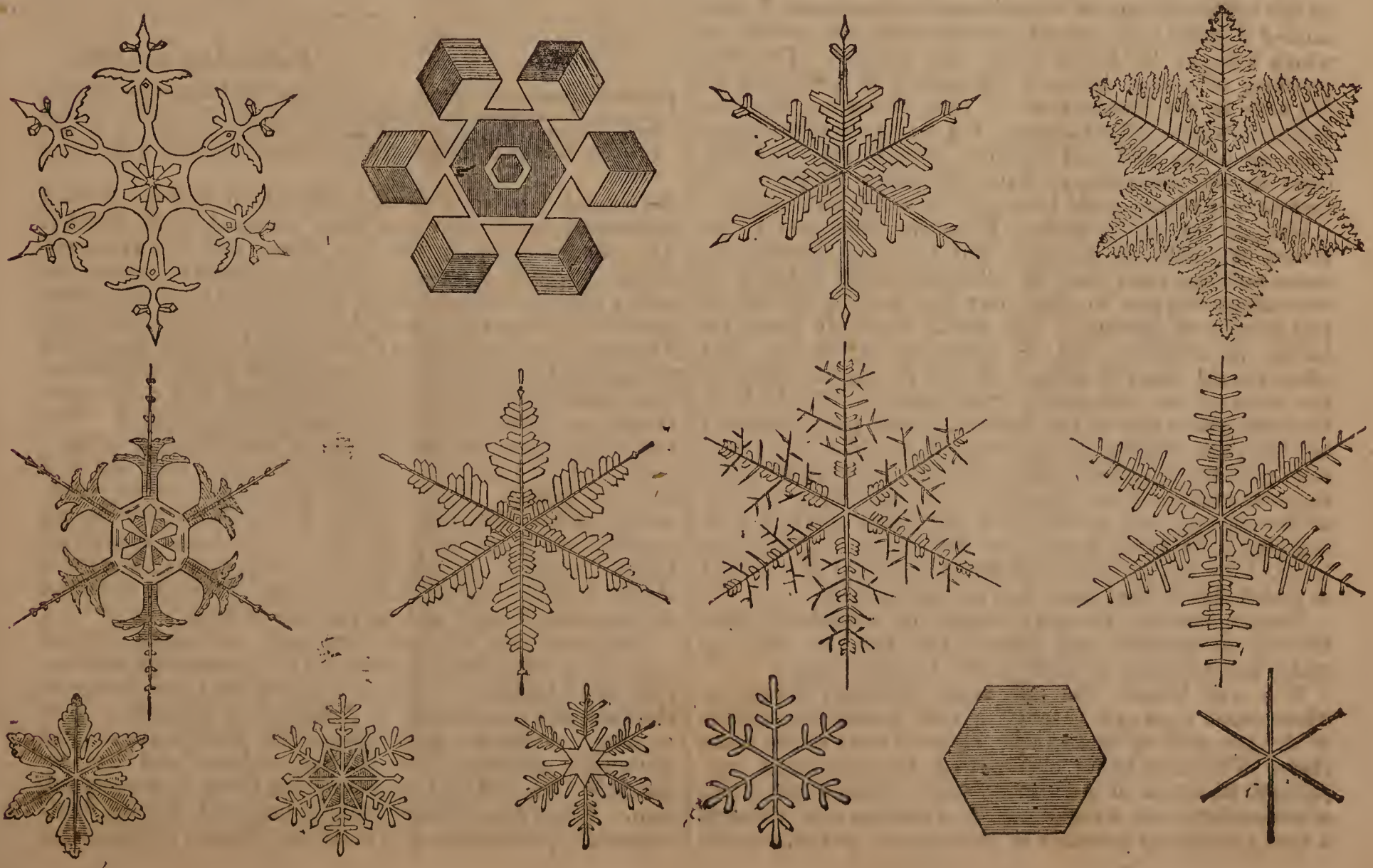
You see a cloud has now formed in the receiver, and when the air is allowed to re-enter, it causes the cloud to go entirely away, although the vapour itself is still there. We will work the pump again, and you will see that the cloud is again formed, and will be again illuminated by the light from the lamp. There it is. That is a true cloud which is formed in this way from the air of the room, and it is in this way that clouds are formed in the atmosphere by the expansion and consequent cooling of the air which rises from the surface of the sea.

These clouds may fall as rain, but as I have said, they may also fall as snow. I suppose that snow is such a familiar thing to every boy and girl here present, that it may seem to be hardly worth thinking about; but still this substance is one of the most wonderful and beautiful things in the whole world; and when snow is formed in a very still atmosphere, as I have often had the pleasure of seeing it formed in the Alps, it takes the form of those beautiful figures which are represented in the diagram yonder. (Fig. 3.)

It forms as small six-rayed stars. This is the form of the snow which goes on loading the Alpine mountains year after year; and when we look at these mountains and at the valleys connected with them, we find that the most wonderful series of appearances presents itself. On very closely observing the snow upon the Alpine slopes we find that it is in a state of motion. We find that the snow has been incessantly moving down the Alpine slopes into the valleys; and hence we have the valleys filled with rivers of ice. On standing for the first time beside one of these rivers of ice you would imagine that it was perfectly motionless, and that a body so rigid as ice could not move at all; but when you make proper observations, you find that the ice is perpetually moving down, and thus we have these glaciers of the Alps. I have no doubt that every boy here will one day visit those glaciers for himself. I have here a sketch of one of the most famous of those glaciers. It is called the "Mer de Glace," and is situated near Chamounix. This Mer de Glace has its great feeders from the snows that fall upon Mont Blanc and the series of mountains which are rudely sketched in this diagram. Here is a great cascade where the snow, after being half consolidated—squeezed together so as to form ice—actually moves down, forming a cascade of ice which comes along this valley. Here is another basin where the snows collect, and where its particles are squeezed into ice, and you have this ice also always in a state of motion.

Now let us look at the lines which I have drawn on the diagram. The mountains beside the glaciers are always sending down stones and dirt, and consequently you

FIG. 3.



always have lines of dirt carried down; and you see that where two glaciers have their sides turning and uniting as here shown, they form a line along the middle of the trunk of the glacier. Now, these lines which I have mentioned are called *moraines*. Those at the side are called *lateral moraines*, and those in the middle are called *medial moraines*. We have in the Mer de Glace these three moraines. If we examine this glacier we find that notwithstanding the rigidity of ice it moves down like a river. Eminent men have worked at this subject; Saussure worked at it a little—not much, and was followed by Bordier, who observed that ice behaved almost like a viscous body. He was the first to propound the fact that ice was of this character. He was followed by Rendu, who also took up the idea that ice behaved like a viscous body such as honey, or treacle, or tar, or paste. Then he was followed by Mr. Agassiz and another, and they determined the velocity with which this ice falls. Then came Principal Forbes, an eminent Scotchman, and his measurements pushed the question far beyond its former stage. And then came Mr. Huxley and myself; and we pushed the matter a little forward; and afterwards I did a little on my own account in reference to this question. It is in this way that scientific knowledge is accumulated. It goes rolling on and becoming bigger like a snow-ball, and thus it is that science grows and has grown to what it is at the present day.

(To be continued.)

ON THE

IDENTITY OF THE BODY IN THE ATMOSPHERE

WHICH DECOMPOSES IODIDE OF POTASSIUM, WITH OZONE.

By THOMAS ANDREWS, M.D., F.R.S.

It was assumed for many years, chiefly on the authority of Schœnbein, that the body in the atmosphere which colours iodide of potassium paper is identical with ozone; but this identity has of late been called in question, and as the subject is one of considerable importance, I submitted it lately to a careful investigation, the results of which I beg to lay briefly before the Society. The only property of ozone, hitherto recognised as belonging to the body in the atmosphere, is that of setting free the iodine in iodide of potassium; but as other substances, such as nitric acid and chlorine, which may possibly exist in the atmosphere, have the same property, no certain conclusion could be drawn from this fact alone.

One of the most striking properties of ozone is its power of oxidising mercury, and few experiments are more striking than that of allowing some bubbles of electrolytic oxygen to play over the surface of one or two pounds of mercury. The metal instantly loses its lustre, its mobility, and its convexity of surface, and when moved about it adheres in thin mirror-like films to the sides of the containing glass vessel. The body in the atmosphere acts in the same way upon pure mercury; but from the very minute quantity of it which is at any time present, the experiment requires some care in order that the effect may be observed. On passing a stream of atmospheric air, which gave the usual reaction with test-paper, for some hours over the surface of mercury in a U-tube, the metal was distinctly oxidised at the end at which the air first came into contact with it.

This experiment, however, cannot be considered conclusive, as mercury will tarnish and lose its mobility under the influence of many bodies besides ozone.

It is well known that all ozone reactions disappear when ozone is passed through a tube containing pellets of dry peroxide of manganese, or other body of the same class. The same thing occurs with the substance supposed to be ozone in the atmosphere. About eighty litres of atmospherirc air were drawn, at a uniform rate, through a tube containing peroxide of manganese, and afterwards made to play upon very delicate test-paper. Not the slighest coloration occurred, although the same paper was distinctly affected when ten litres of the same air, without the interposition of the manganese tube, were passed over it.

But the action of heat furnishes the most unequivocal proof of the identity of the body in the atmosphere with ozone. In a former communication (*Phil. Trans.* for 1856, p. 12). I showed that ozone, whether obtained by electrolysis or by the action of the electrical brush upon oxygen, is quickly destroyed at the temperature of 237° C. An apparatus was fitted up, by means of which a stream of atmospheric air could be heated to 260° C. in a globular glass vessel of the capacity of five litres. On leaving this vessel, the air was passed through a U-tube, one metre in length, whose sides were moistened internally with water, while the tube itself was cooled by being immersed in a vessel of cold water. On passing atmospheric air in a favourable state through this apparatus, at the rate of three litres per minute, the test-paper was distinctly tinged in two or three minutes, provided no heat was applied to the glass globe. But when the temperature of the air, as it passed through the globe, was maintained at 260° C., not the slightest action occurred upon the test-paper, however long the current continued to pass. Similar experiments, with an artificial atmosphere of ozone, that is, with the air of a large chamber containing a small quantity of electrolytic ozone, gave precisely the same results. On the other hand, when small quantities of chlorine or nitric acid vapour, largely diluted with air, were drawn through the same apparatus, the test-paper was equally affected, whether the glass globe was heated or not.

From these experiments I consider myself justified in concluding that the body in the atmosphere, which decomposes iodide of potassium, is identical with ozone.—*Proceedings of the Royal Society.*

FOREIGN SCIENCE.

PARIS, JAN. 14, 1868.

Double sulphocyanides of chromium—Ozone and antozone—Experimental demonstration. ACADEMY OF SCIENCES.—The mineral Woodwardite—Electrolysis of tartaric acid—Passage of electric currents through incandescent gases—Reestablishment of the voltaic arc.

An extended series of compounds, which may be termed chromosulphocyanides, has been obtained by M. Rœsler. This chemist finds, in the first place, that when concentrated solutions of 6 parts of sulphocyanide of potassium and 5 parts of chrome alum are mixed, the violet colour gradually passes to a wine red; heat quickens the reaction. The solution filtered from the sulphates which have been precipitated by alcohol, and evaporated just to crystallisation, yields sulphocyanide of chromium and potassium. It may be purified by recrystallisation from alcohol. The salt crystallises in deep coloured quadrilateral prisms, almost black; seen by transparent light, they are of a ruby-red colour. They are not altered by the air; submitted to heat they become very dark coloured, but during cooling take a fine red tint. At 110° the water of crystallisation is driven off, the salt becoming opaque; at a more elevated temperature it is decomposed. This salt dissolves in ·72 parts of water and ·94 parts of alcohol.

Sulphocyanide of chromium and potassium is not affected by sulphide of ammonium nor carbonated alkalies, even upon boiling. A dilute solution does not change in the cold, but sesquioxide of chromium is deposited upon heating. Ammonia only destroys the combination after prolonged ebullition. Weak hydrochloric acid has no action in the cold, but upon heating there is decomposition. When to a concentrated solution of the salt, concentrated hydrochloric acid is added, chloride of potassium

is separated, a yellow powder adhering which contains much sulphur; this pulverulent matter appears to be persulphocyanic acid. When the potassium salt is evaporated with hydrochloric acid, there is complete decomposition, with formation of chlorides of chromium and potassium.

Sulphocyanide of chromium and potassium does not precipitate the solutions of the alkaline earths, nor those of cadmium, cobalt, nickel, zinc, manganese, and iron. With sulphate of copper the red colour passes into violet blue. After the lapse of some time, oxide of copper is deposited. If heated, it is formed more rapidly.

Mercuric salts cause a voluminous red precipitate which collects upon ebullition; it dissolves but little in nitric acid. Mercurous salts give a yellow precipitate, changing into greenish brown; nitric acid oxidises this compound to the red one described above. The salts of tin slowly give rise to a white precipitate. The sulphocyanide of chromium and ammonium has been formed. It resembles the preceding compound crystallographically and chemically. It is prepared in a similar manner. Sulphocyanide of chromium and sodium is obtained by dissolving oxide of chromium in sulphuric acid and adding sulphocyanide of sodium. The mixture is boiled for some time, and on cooling, the sulphates are deposited. Alcohol is added, which dissolves the double sulphocyanide. The salt crystallises in small plates; it is deliquescent. In an atmosphere dried by sulphuric acid it loses water and falls to a powder of a clear red colour. At 110° the water of crystallisation is driven off, no further alteration being induced at this temperature. With reagents it manifests less stability than the salts already described. Sulphocyanide of chromium and barium is obtained by dissolving oxide of chromium in hydrochloric acid, removing the excess of hydrochloric acid by evaporation, and decomposing with sulphocyanide of barium. It is separated from chloride of barium by crystallisation. This salt crystallises in short four-sided prisms; it is deliquescent.

The barium salt furnishes double sulphocyanides by decomposition with the sulphates. A number of other metallic sulphocyanides have also been combined with sulphocyanide of chromium. The sulphocyanides of silver, lead, and zinc have been combined in this way.

All attempts to separate chromo-sulphocyanic acid failed; but M. Rœsler has found that in the decomposition of a solution of the lead or silver salt by sulphuretted hydrogen, an acid liquid of a deep red colour is obtained, which he thinks undoubtedly contains the acid.

An experiment of M. Schönbein's, illustrating the simultaneous formation of ozone and antozone, is said to be the following:—Into a flask of 500 c.c. capacity, and 3 or 4 centimetres in diameter across the neck, a little ether is poured, just enough to cover the bottom, and a spiral of red hot platinum is plunged into the vapours. It is necessary to avoid heating the flask too strongly. The platinum glows until all the ether has been destroyed. The experiment is repeated two or three times, and now the question is to demonstrate that both ozone and antozone are formed in this slow oxidation of the ether. The first is, of course, easily shown to be present by means of the iodide of potassium and starch paper. To show the presence of antozone the flask is rinsed with a small quantity of ether, which will then be sufficiently charged with peroxide of hydrogen to give clearly the perchromic acid reaction. Some solution of bichromate of potash is placed in a test tube, and a drop of sulphuric acid added, the ether with which the flask has been rinsed is then poured in, when the etherial layer becomes coloured a beautiful violet blue. The conclusion to be arrived at from this experiment is that during the formation of ozone, antozone is also formed,—this in the presence of water, being converted into peroxide of hydrogen.

The memoirs relating to chemistry and physics broug h before the Academy of Sciences on the 30th of December, were the following:—"On the Woodwardite of Cornwall," by M. Pisani; "Electrolysis of Tartaric Acid," by M. Bourgoin; "On the Passage of Electric Currents through Incandescent Gases," by M. Becquerel; and "The Spontaneous re-Establishment of the Voltaic Arc after an Extinction of Short Duration," by M. Le Roux. M. Pisani commenced by referring to the description given by Mr. Church of this mineral, and compared an analysis by this chemist with his own; the numbers only differed in one constituent, viz., the alumina, which in his specimen was less and which contained silica; this latter, however, was in insufficient quantity to constitute a silicate with the alumina.

He also gave an analysis of a Cornish mineral resembling Woodwardite. These are the percentage results:—Oxide of copper, 17·4; sulphuric acid, 4·7; alumina, 33·8; silica, 6·7; water, 38·7. The greatest difference, as shown by the analysis between this mineral and Woodwardite is in the alumina. M. Pisani observed that the mineral might be considered as a mixture of Langite with a very basic silicate of alumina analogous to Scarbroïte or to Schrœtterite, or as a mixture of Langite with a hydrate of alumina and mixed with a silicate of the allophane species. He does not imagine Woodwardite to be a new species of mineral, it may be considered as composed of a mixture similar to the new mineral just described.

M. Bourgoin's memoir is a continuation of his research on the electrolysis of organic acids. He has studied the action of the current upon neutral tartrate, on a mixture of tartrate and alkali, lastly, on free tartaric acid. To examine the fundamental action of the current on tartaric acid, a concentrated solution of the neutral tartrate of potash is conveniently operated upon. As soon as the current passes, the solution becomes alkaline at the negative pole; only a moderate disengagement of gas is produced at the two poles. The principal result is the formation of a white precipitate, which is slowly but continuously deposited from the positive electrode. Analysis shows this substance to be wholly cream of tartar. The solution at the positive pole remains neutral during the experiment. The gas evolved at the positive pole was composed of carbonic acid, oxygen, carbonic oxide, and nitrogen. Nearly the whole of the loss takes place at the positive pole. M. Bourgoin gives the following equation as expressing the fundamental action—

$$C_8H_4K_2O_{12} = (C_8H_4O_{10} + O_2) + K_2;$$

this secondary reaction follows—

$$C_8H_4O_{10} + H_2O_2 = C_8H_6O_{12}.$$

The tartaric acid thus regenerated at the positive pole forms with the neutral tartrate, cream of tartar; there is, however, some tartaric acid destroyed by oxidation. The action of the current on a mixture of neutral tartrate and alkali produces quite different results to those obtained with neutral tartrate only, notwithstanding that the fundamental action is the same. At the positive pole a mixture of carbonic acid, carbonic oxide, oxygen and hydride of ethylen is evolved. M. Berthelot discovered acetylen also in a sample of the gas sent him by M. Bourgoin. The decomposition of free tartaric acid yielded the same products as the neutral tartrate, though in different proportions. The carbonic acid is the dominant product from the first: the carbonic oxide diminishes as the experiment proceeds; the same is the case with the oxygen and nitrogen, though to a less extent. Acetic acid is formed at the positive pole. After the fifth day the experiment had been in progress, the solution in the neighbourhood of the positive pole contained a large quantity of acetic acid, which was isolated as acetate of baryta. The following equations explain the action of the current upon free tartaric acid: the fundamental reaction taking place is $C_8H_6O_{12} = (C_8H_4O_{10} + O_2) + H_2$, followed by the secondary reaction—

$$C_8H_4O_{10} + O_2 = 2C_2O_4 + C_4H_4O_4.$$

M. Becquerel's paper referred to one recently contributed by M. Bouchotte, who has observed that by the introduction of a voltameter of acidulated water in the cir-

cuit developed by a magneto-electric apparatus, producing two series of alternating currents, these may be made to give place to one series, or to currents of only one kind. M. Becquerel devoted himself chiefly to the explanation of the phenomenon.

M. Le Roux finds that results in regard to the establishment of the voltaic arc, similar to those given by induction currents, may be obtained with the ordinary currents from voltaic piles. With a nitric acid battety of 50 elements, the current may be interrupted for a time which may extend to about $\frac{1}{25}$th of a second, and then leap from one carbon to the other, although they be three millimètres apart.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Glycogen.—The amyloidical matter found in mollusks is, according to Bizio, glycogen. If the latter, after precipitation with alcohol, is allowed to dry gradually, it coheres together in lumps. Rapid desiccation leaves it as a fine powder, in which condition glycogen has commonly been observed. In contact with white of egg or casein, lactic acid fermentation slowly sets in. Dried at 100° C., its composition is $C_5H_{10}O_5$.—(*Comptes R.* lxv. 175).

Monamines derived from Aldehydes.—H. Schiff. Prolonged action of alcoholic ammonia upon acetic aldehyde, at ordinary temperature, gives rise to the formation of two bases—C_6H_9N or C_6H_7N (Picolin), distilling at 60°—70° C., soluble in water, and $N_2(C_2H_4)_3$ remaining in the residue after distillation. The latter (which has not been obtained pure) is decomposed by water and acids with formation of another soluble base—$C_6H_{11}NO$. The derivation of this third, a tertiary monamine from aldehyde-ammonia, is explained by the following equation :—

$$N\begin{cases} C_2H_4,OH \\ H_2 \end{cases} + 2C_2H_4O = 2H_2O + N\begin{cases} C_2H_4,OH \\ (C_2H_3)_2 \end{cases}$$

When aldehyde is treated with ammonia at 100°, two other bases are obtained—$C_{10}H_{15}NO$, and $C_8H_{13}NO$, of which the latter has already been noticed by Heintz and by Wislicenus. Hydro-œnanthamide $N(C_7H_{12})_3$ is decomposed by boiling water, and the compound

$$N(C_7H_{13})_2(C_7H_{14},OH)$$

formed, which shows no basic properties.

Related to these are valeral-ammonia and Erdmann's trioxyamylidene, to which the formulæ

$$N\begin{cases} C_5H_{10},OH \\ H_2 \end{cases}$$

and $N(C_5H_{10},OH)_3$ are given.

The reaction between acrolein and ammonia is somewhat different, inasmuch as first a combination of the two in equivalent proportions takes place, which then acts on an excess of acrolein, thus :—

$$2N\begin{cases} C_3H_4,OH \\ H_2 \end{cases} + 2C_3H_4O = 2H_2O + N_2\begin{cases} 2(C_3H_4,OH)' \\ 2(C_3H_4)'' \end{cases}$$

This base resembles closely those derived from acetic aldehyde. Ammonic sulphhydrate converts acrolein and œnanthol into acrothialdin $C_9H_{13}NS_2$, and œnanthothialdin $C_{21}H_{43}NS_2$.

From the reactions of these bodies the author concludes that the thialdines likewise are tertiary amines, in which three typical hydrogens are replaced by three radicals containing the sulphur, as sulphydril (SH), just as the bases above mentioned contain oxhydril (OH).—(*Comptes R.*, lxv. 320.)

Influence of Coloured Light on the Decomposition of Carbonic Anhydride by Plants.—L. Cailletet. The red and yellow rays of light are the most favourable in promoting the decomposition of carbonic anhydride by plants. Light which passed through a solution of iodine in carbonic disulphide prevents decomposition altogether. Under the influence of green light, not only does no decomposition take place, but new quantities of carbonic anhydride are formed. A fresh leaf exposed to sunlight, under a bell-jar of green glass, exhales nearly as much carbonic anhydride as it would in the dark.—(*Comptes R.* lxv. 322.)

Synthesis of Diethylated Toluol.—Lippmann and Longuinine. With a view of arriving at a clearer conception of the constitution of the radical amyl, and of finding a new method for the synthesis of aromatic hydrocarbons, the authors investigated the action of zincic ethide upon chlorobenzol (chloride of oil of bitter almonds). The reaction that takes place is the following—

$$C_6H_5,CHCl_2 + Zn(C_2H_5)_2 = C_6H_5,CH(C_2H_5)_2 + ZnCl_2$$

The hydrocarbon $C_{11}H_{16}$ must be considered as toluol in which 2 atoms of hydrogen of methyle are replaced by 2 of ethyl. Its density was found = 5·1107, calculated = 5·1245. Its boiling point when pure, is at about 180° C. or 15° lower than that of Fittig's amylbenzol, which has the same composition. It follows from this that the two are differently constituted, and that the formula of amyl is not correctly represented as—

$$C\begin{cases} (C_2H_5)_2 \\ H \end{cases}$$

(*Comptes R.* lxv., 349.)

Succinic Acid from Ethylidenic Chloride.—M. Simpson. The action of potassic hydrate upon ethylic cyanide resulting in the formation of succinic acid led the author to expect that an isomeric acid would be obtained if instead of ethylenic, ethylidenic cyanide were taken. No isomer, however, but ordinary succinic acid is formed, and this apparent anomaly is explained by the supposition that during the process of heating ethylidenic chloride with potassic cyanide (180° C. being the temperature required) the former is partially converted into ethylenic chloride by an exchange of places between hydrogen and chlorine :—

$$\begin{cases} CH_2H \\ CHCl_2 \end{cases} = \begin{cases} CH_2Cl \\ CH_2Cl \end{cases}$$

—(*Comptes R.*, lxv., 351.)

Aldehyde and Cyanhydric Acid.—M. Simpson and A. Gautier. Equal mol. of acetic aldehyde and dry cyanhydric acid unite by direct addition, when exposed for ten to twelve days to a temperature of 20° to 30° C. The body thus formed has the composition CNH,C_2H_4O. It is a colourless, oily liquid, boiling at about 183°, but rapidly resolving itself into its constituents at that temperature. It is soluble in water and in alcohol, absorbs ammonia readily, and when heated with it to 100°, is converted into a syrupy mass of basic properties. The action of chlorhydric acid and water upon cyanhydric aldehyde gives rise to the formation of *lactic acid*, according to the following equation :—

$$CNH,C_2H_4O + HCl + 2H_2O = NH_4Cl + C_3H_6O_3$$

(*Comptes R.*, lxv., 414.)

The Velocity of Light, according to a calculation recently published by Professor Chase, of Boston, is nearly the same as would be acquired in one year by a falling body under the influence of an accelerating force equivalent to the force of gravitation at the earth's surface, viz., 32 1-6 × 86,400 × 365¼ ÷ 5,280 = 192,254 miles per second.

CORRESPONDENCE.

CRYSTALLOGRAPHY AND THE BLOWPIPE.

To the Editor of the Chemical News.

SIR,—Below I have the pleasure to send you the translation of a letter I have just received from Professor Richter, head of the University of Freiberg, where he succeeded the late celebrated Plattner. It is indeed gratifying to me to think that my trifling endeavours should engage the attention of such a man.

His want of success in making the vesicles at first, may be explained by the fact of my not having mentioned (as I ought to have done) that, in blowing the vesicles, the bead should not be operated on until it has partially cooled down to a red heat, as at a high temperature the current of air is too strong for the small density of the fluid borax, which it soon bursts.

I take this opportunity of mentioning two facts in addition to those stated in my paper on this subject, published in the CHEMICAL NEWS of the 20th December. One, that the smallest imaginable particle of reduced metal, as for instance copper, may be clearly observed in one of these vesicles by a microscope, whereas in a bead it might be buried in the centre and escape observation. The other, that eight or ten vesicles kept by me for three weeks at Christmas, became first black and then clear after effloresceing, on being re-melted by a blowpipe flame projected from a spirit lamp,—showing the presence of free carbon, or organic matter, which could not have been there when the vesicles were formed. I will, with your permission, continue this subject shortly.—I am, &c.,

W. A. ROSS.

(Copy.)

Freiberg, 1st January, 1868.

DEAR SIR,—If I have not answered your letter of the 9th December before to-day, I trust you will kindly excuse the delay, which is owing to my having been so busy. The observation that various earths and metallic oxides may be dissolved in borax and phosphor salt, and preserved crystallised under certain circumstances, was made and described some years ago by George Emerson,* an American, and published in the *Proceedings* of the American Academy of Arts and Sciences, March, 1865, vol. vi., where drawings of the beads may be found. Also, G. Rose, of Berlin, has lately described the production of crystallised bodies by means of the blowpipe in borax and phosphor salt. The article is found in the monthly report of the *Proceedings* of the Royal Academy of Sciences in Berlin, for March and July, 1867, with drawings. I have verified by my experiments the statements of Messieurs Emerson and Rose, and suspect that the interesting discoveries made by you are intimately connected with those phenomena.†

I have also attempted to make borax vesicles from your description, but have not realised a satisfactory result, probably from unskilfulness in manipulation.

It would certainly prove a grateful task to study more closely this relation of bodies to each other, in order to be able to use it at the same time as a means of their recognition. I recommend to you the perusal of the abovementioned experiments of Messieurs Emerson and Rose, and I shall be glad if you will inform me of the result of your further labours,—With great esteem, I remain, yours, &c.

(Signed) THEODORE RICHTER.

To Captain W. A. Ross, R. A., Woolwich.

* This, as stated by me in the CHEMICAL NEWS of Jan. 3, is a misapprehension, the crystals having been formerly pointed out by Berzelius and others.

† They appear to have the relation to each other that masses of ice bear to the delicate crystals of frost.

VOLUMETRIC DETERMINATION OF IRON.

To the Editor of the Chemical News.

SIR,—The method proposed in the last week's CHEMICAL NEWS, by Captain Ross, for the volumetric assay of iron by the decolorisation of the iron solution coloured red by sulphocyanide by means of a standard solution of lime water, appears to be inapplicable, inasmuch as the amount of lime requisite depends, not on the quantity of iron present, but on that of the acid in the solution to be tested. The bleaching effect of the lime is caused by the decomposition of the ferric sulphocyanide, whereby ferric hydrate is precipitated, and calcium sulphocyanides produced; but this effect does not take place until not only the whole of the free acid present is neutralised, but also almost the whole of the iron precipitated as hydrate. In practical analysis a solution of iron without free acid is never obtained; nor could the solution be neutralised by addition of alkali, because the point when the free acid is just saturated cannot be observed either by test-papers (as ferric salts have an acid reaction) or by the commencement of a precipitate, as ferric hydrate is soluble in solutions of ferric salts.

I have frequently had occasion to examine acid liquids containing metals in solution with a view to the determination, firstly, of the free acid, and secondly, of the total acid free and combined (as, for example, in the waste manganese chloride of the bleaching powder works). This second quantity is readily ascertainable by adding a standard alkaline solution to the acid liquid examined until an alkaline reaction is observed; but whenever ferric salts were present, the exact determination of the first quantity was found impossible from the solubility of the ferric hydrate; even Kiefer's very convenient process for estimating free acid in solutions containing heavy metals by means of an ammoniacal solution of a cupric salt always indicated a larger amount of free acid than was really present, from the circumstance that as soon as a turbidity was produced by the precipitation of cupric hydrate from the neutralisation of the ammonia (the terminal reaction), it disappeared on agitation, the cupric hydrate apparently decomposing the ferric salt, and thereby being dissolved, whilst the newly-precipitated ferric hydrate also dissolved in the remaining ferric salt.

The destruction of the red ferric sulphocyanide by alkalies might be used as a terminal reaction in acidimetry; but on trial the sulphocyanide appears to be inferior to the ferrocyanide (Prussian blue) as an indicator; and this latter is not so convenient as litmus or cochineal tincture as usually employed.—I am, &c.,

CHARLES R. A. WRIGHT, B.Sc.

Chemical Laboratory, St. Mary's Hospital, W., Jan. 13, 1868.

MISCELLANEOUS.

Nitroglycerine.—While the explosive nature of this compound is still exciting attention, it will be interesting to give the evidence with regard to the cause of an explosion in the United States, resulting in fatal consequences, deposed to by Professor Doremus, one of the most distinguished of American chemists. The circumstances under which the accident took place are briefly these. The Central Railroad Company are making a deep cutting near South Bergen, N. J., and in effecting this they use nitroglycerine. A can containing about sixty pounds of this oil, having partially solidified, was carried by a workman to the blacksmith's shed, where it was placed in water, red hot bars of iron being plunged into the latter. Suddenly an explosion occurred, killing nine men.

At the inquest Professor Doremus said:—"On Dec. 2, I received from the coroner two bottles of nitroglycerine, with a request to report upon its properties; I have subjected it to ultimate chemical analysis, and find it is well-made nitroglycerine; the substance freezes at about 46°; it is made to decompose in a very peculiar way; on moistening paper with it, it burns with rapidity; it does not explode when red-hot copper is placed in it; we tried it with the most intense heat we can produce with a galvanic battery, with two hundred cells holding a gallon and a-half each; some nitroglycerine was placed in a cup and connected with one of the poles of the battery; through a pencil of gas-carbon the other poles of the battery were connected with the glycerine; no explosion ensued; but when the point touched the Britannia vessel the nitroglycerine took fire, a portion burning and the rest scattering about; this is as severe a test as we can submit it to in the way of heat under the pressure of the air; we, therefore, would conclude that nitroglycerine carried about exposed cannot explode, even if you drop a coal of fire into it; if the liquid is confined, or is under pressure, then an explosion will ensue; if paper be moistened with it and put on an anvil and a smart blow given with a hammer, a sharp detonation ensues; if gunpowder or the fulminates of mercury, silver, or gun-cotton be ignited in a vacuum by a galvanic battery, none of them will explode; if any gas be introduced so as to produce a gentle pressure during the decomposition, then a rapid evolution of gases will result; the results of decomposition in a vacuum differ from those under atmospheric pressure or when they are burnt in a pistol, musket, a cannon, or in a mine; where we have little or no pressure it is difficult to get these substances to burn rapidly; nitroglycerine is more difficult to explode than powder; in many respects it resembles gun-cotton, which is made in a similar way. If gun-cotton be immersed in protochloride of iron it turns into common cotton; the same experiment was tried with nitroglycerine by mixing it with proto-chloride of iron, and it reverted into common glycerine. There are four well-known varieties of gun-cotton made by employing acids of different strengths; they differ in chemical composition and properties, as well as in their explosive qualities; the late Minister of War in Austria, in 1862, stated to me that he had ordered 400 cannon for gun-cotton, and six months after he stated that he had ordered all the cannon to be changed and adapted to powder in consequence of spontaneous combustions; much less is known of nitroglycerine than of gun-cotton, and probably several varieties of this article may be formed, as of gun-cotton; this would explain cases of spontaneous explosion; if the nitroglycerine is not carefully washed to get rid of the acid, a gradual decomposition will ensue, producing gases which, if the vessel be closed, will explode. My opinion is that nitroglycerine should be used in the most careful hands; I do not think I would put it in the hands of a common labourer for blasting purposes; it is less dangerous in a frozen than a liquid state; I think concussion would explode frozen nitroglycerine. . . . . Since leaving I have learned that the can of nitroglycerine, the explosion of which proved so fatal, was full of frozen or solid nitroglycerine. Now, although the cork might have been removed, it is possible that the red-hot irons melted and decomposed the material at the bottom of the can, leaving a quantity of solid nitroglycerine above, which, by preventing the escape of the gases, produced the pressure required for an explosion. A red-hot iron, or the more intense heat of a powerful galvanic battery, will not explode the nitroglycerine unless it is under pressure. This stopper of frozen nitroglycerine might have closed the orifice of the can sufficiently to produce the required pressure." Amongst other evidence was the following:—Otto Burstenbinden said: I am now in the blasting business, and reside in the City of New York; nitroglycerine is composed of nitric acid, sulphuric acid, and glycerine oil; it is a liquid of light yellow colour, and about six-tenths heavier than water; in exploding it expands 10,400 times, and powder expands about 800 times; this compound called nitroglycerine will not explode by simple contact with fire; it requires about 360 degrees of heat to make it explode; it will not easily explode by concussion or friction when fluid, but it changes its qualities entirely when frozen; if frozen it will explode very easily by a stroke or slight concussion.—Wm. H. White said: I reside in Syracuse, and for the past two years have turned my attention to the use of nitroglycerine; I know that it is no more explosive when frozen than when in a liquid state; frozen, it is less liable to explode; it is very hard to explode a cartridge when frozen; a very heavy weight coming on nitroglycerine either in a frozen or liquid state would explode; I have seen a man let a can of frozen nitroglycerine fall from his shoulder on a rock without exploding it; I consider nitroglycerine twenty-five times safer than powder for blasting purposes; I have seen a chunk of nitroglycerine as big as my hat cut open with an axe.—The jury brought in the following verdict:—"We find that the deceased came to their deaths on the 25th day of November last by the explosion of a can of nitroglycerine, consequent upon the careless handling of the same by Thomas Burns, one of the deceased, and we censure the contractor, Colonel Schafner, for not being more careful in the selection of a man to use the article of nitroglycerine, and recommend that in future men should be employed in its use who understand its explosive qualities, and we request the Council of the town of Bergen to order that there shall not be any more than 100 pounds of nitroglycerine stored up or kept in the town of Bergen, the same to be stored in a fire-proof vault when not required for use."

Dietetic Salt.—One of the great evils that owes its origin to the scientific enterprise of the present age is that any promising scientific scheme, after being brought into prominent notice, becomes for the time being quite the fashion, and is then entirely forgotten, often, too, from mere caprice. We hope that this fate may still be averted from Dr. Lankester's ingenious scheme of supplying necessary but frequently overlooked articles of diet, by means of his dietetic salt. This compound is a proposed substitute for ordinary table salt, chloride of sodium being a notable constituent; but, in addition to this, which is far from being the sole or even most important inorganic constituent of our food, we have phosphate of lime, chloride of potassium, sulphates of potash and soda, with smaller quantities of magnesian and iron salts. The argument for their use is very strong. Leaving out the large proportion of epidemics, almost all the common diseases are directly traceable by modern physicians to dietetic errors; and those that certainly are due in part to deficiency of inorganic food form by no means a contemptible list. Scurvy is known to arise from a deficiency of the salts of potash. Scrofula and consumption, rickets, and softening of the bones, occur when the phosphates of lime and other bases are deficient. Anæmia, chlorosis, and a variety of nervous disorders, are the result of an absence of iron, and are at once cured by the use of this agent as a remedy. In such cases, the medical man is in the habit of prescribing medicines containing these agents; and there can, therefore, be no doubt that the habitual use of these substances in the food, in the same way as common salt is employed, would be a means of preventing the occurrence of a large number of diseases. The quantities of the saline ingredients employed, in addition to common salt, are so calculated that they shall be supplied in the same proportion by its use, as they exist in the human blood, and are got rid of in the body. Dietetic salt is one of those simple but useful applications of science of which the value is at once perceived; it deserves to hold a prominent place in the rank of articles of food, and it is to be hoped that it will not be lost in the crowd of similar inventions.

CONTEMPORARY SCIENTIFIC PRESS.

(Under this heading it is intended to give the titles of all the chemical papers which are published in the principal scientific periodicals of the Continent. Articles which are merely reprints or abstracts of papers already noticed will be omitted. Abstracts of the more important papers here announced will appear in future numbers of the "Chemical News.")

Comptes Rendus.

October 14, 1867.

Chasles, "Answer to Faugère's Letter, on the Authenticity of the Newton and Pascal Correspondence." Morin, "Observations on some Correspondence between James II. and Louis XIV., recently brought under the notice of the Academy, *àpropos* of the Newton and Pascal Controversy." Le Verrier, "On the Authenticity of the Newton and Pascal Correspondence." Sir D. Brewster, "On the Question of the Application of Dioptric Lenses to Lighthouses." J. Fournet, "On the Path of Thunder Storms over various Parts of Switzerland." J. Lemaire, "On the Presence of Bacteriums and other Organisms in Emanations from the Human Body." Faugere, "Letter on the Authenticity of the Newton and Pascal Correspondence." G. Kirchhoff, "On the Theory of Sun Spots." Fordos and Gelis, "Remarks on Riche's and Kolb's Memoirs on the Nature of the Chlorides used for Bleaching."

October 21.

Sir D. Brewster, "Additional Letter to Chevreul on the Nature of the Relations which existed between Newton and Pascal." Chasles, "Reply to Le Verrler's Note respecting the Authenticity of the Pascal and Newton Correspondence. "Observations on Faugere's last Letter." "Reply to Sir David Brewster's Letter to Chevreul." Babinet, "Note on the Precise Date of the Establishment by Sir Isaac Newton of the Laws of Attraction." Faye, "Remarks on Kirchhoff's Letter, published in the *Comptes Rendus* for October 14, on the Theory of Sun Spots." C. Deville and Janssen, "On the Submarine Eruption which took place between the Islands of Terceira and Graciosa, Azores, on the 1st of June, 1867." Chevreul, "On the same Subject." Fouque, "On the Gases disengaged during the Submarine Eruption at the Azores on the 1st of June, 1867." A. Dronke, "Note on the Formation of Crystals of Gypsum." D'Archiac, "Remarks on the Foregoing Paper."

Sitzungsberichte der Kaiserlichen Akademie der Wissenschaften zu Wien. (Mathematisch-naturwissen-schaftliche Classe.)

April—May, 1867.

E. Schwartz, "On the Use of Picric Acid and other Substances for preparing Microscopic Objects in Two or more Colours." "Report on the Prize of 1000 Florins for an Essay on the Progress of Mineralogy during the Years 1862-65."

Poggendorff's *Annalen der Physik.*

August 7, 1867.

E. Edlund, "On the Property possessed by a Voltaic Current of causing Solids to expand independently of the Heat produced by the Passage of such Current." C. Rammelsberg, "On the Phosphites." E. Schone, "On the Compounds of Sulphur with the Metals of the Alkalies." H. W. Schroder van der Kolk, "On the Mechanical Energy of Chemical Combination: Combustion." "Reply to H. Deville's Remarks on the Author's Paper on the Theory of Dissociation." F. Melde, "On a peculiar Mode of Formation of Sound Pulses and on a Method of Counting the same." R. Weber, "On some Compounds of Chloride of Platinum and of Chloride of Gold." F. Goppelsroder, "On a Fluorescent Substance obtained from Fustic." K. L. Bauer, "On the Refraction of Light and on the Angle of Minimum Deviation in Prisms." H. Gerlach, "Contributions to the Mechanical Theory of the Voltaic Current." Hoh, "On some Remarkable Effects of Lightning." J. C. Poggendorff, "On the Mutual Reaction of Two Induction Machines.

NOTES AND QUERIES.

Aniline Dyes.—Could you kindly inform me as to the best method of stripping off the aniline dyes of goods, and if any book is published on the manufacture and method of dyeing these colours ?—W. P. B.

Mordant for Green on Cotton.—If W. T. C. will send his address, T. Charlesworth, jun., Leicester, will send him the required mordant.

Mordant for Green on Cotton.—Je viens de lire dans votre dernier numero, qu'un de vos correspondants cherche á avoir le procédé d'application du nouveau vert sur coton. Comme je suis aussi bien teinturier que fabricant de produits chimiques, veuillez dire á votre client de m'envoyer quelques livres de coton, que je teindrai par mon procédé en nouveau vert. Ci-inclus un échantillon de coton teint par mon procédé. Dans l'attente de vos nouvelles ; veuillez recevoir, Monsieur le Directeur, &c.—Pierre Clavel. Bâle, le 13 Janv., 1868. [The specimen of dyed cotton mentioned by M. Clavel, can be seen at our office.]

Diphenylamine.—Could you inform me how I might make diphenylamine ? There is a process mentioned in "Watt's Chem. Dict."; but so expensive, that it is impossible to use it. There must be another plan of making it, I believe, for I find a process which speaks of commercial diphenylamine.—Ignoramus.

Extraction of Oil.—Dyeing Turkey Red.—Will you kindly inform your correspondent "E," of Notes and Queries of 20th of December last, that if he will correspond with me on the subject on which he inquires, I shall be happy to give him all information necessary.—Rudolph Schomburg, Ueele, Belgium, 2 Jan. 1868.

Constant Galvanic Current.—The following observations may have occurred to others, but not having met with them published, they may be of value as tending to the perfection of our scientific instruments, by providing the source of a constant galvanic current, of large quantity and very great intensity. The bichromate of potash battery furnishes a current of great force, and its simplicity, economy, and convenience of management would make it preferable to the double fluid batteries, but for its want of constancy when a current of large quantity is required. Experimenting with it lately, I became satisfied of the cause of this defect. Although there may be a large reservoir of liquid, only the stratum between the plates is active, and as no gas being given off there is no circulation ; this soon becomes exhausted, and as it is renewed merely by diffusion, can only maintain a current equivalent to the fresh supply of liquid thus obtained. I therefore used a thin beaker as the containing vessel and placed it over a Bunsen's burner capable of maintaining a moderate circulation of the liquid, and as I expected, the battery now gave its fullest force with absolute constancy until the complete exhaustion of the exciting fluid. Mechanical stirring of the liquid or motion of the plates will produce a similar result ; and thus by any of the various modes which may be employed, this battery can be made to yield a current more powerful than any other known form, without giving off any noxious gases, and as absolutely constant as can be desired.—John T. Sprague.

MEETINGS FOR THE WEEK.

Monday.—Medical, 8 p,m.

—— Society of Arts, 8 p.m. Cantor Lectures, "On Food," by Dr. Letheby. Lecture 1. Varieties of Food ; their Chemical Composition, and their Nutritive Value.

Wednesday.—Society of Arts, 8 p.m. "On the Reports of the Artisans selected to visit the Paris Universal Exhibition of 1867," by W. Hawes, Esq.

—— Geological, 8 p.m. "On the Speeton Clay," by John W. Judd, Esq. F.G.S. 2. "Notice of the Hessle Drift as it appeared in Sections above forty years since," by Professor John Phillips, F.R.S., F.G.S., &c.

Thursday.—Royal, 8½ p.m.

—— Zoological 8½ p.m.

—— Royal Society Club, 6 p.m.

Friday.—Royal Institution, 8 p.m. "On Faraday as a Discoverer," by Dr. Tyndall, F.R.S.

Saturday.—Royal Institution, 3. "On the Non-metallic Elements," by Professor Roscoe, F.R.S.

TO CORRESPONDENTS.

W. Bird Herapath.—Received with thanks. A proof shall be forwarded shortly.

Moonstone.—Amongst mineralogists the name moonstone is applied to one of the varieties of felspar. Fine specimens possess a certain value, but they cannot legitimately be called gems. Adularia is another name for the same stone.

Physic.—Test the deposits for urate of soda. They are probably "chalk stones."

O. Reiman.—There are no less than 52 separate products of the destructive distillation of coal. It would occupy nearly the whole of this column to give the list. Dr. Frankland's Lectures on Coal Gas (reported in this journal in the spring of last year) will give you full information on this point.

H. D. S.—There are many reasons against the supposition that the chemistry of silicium and of boron would be as extensive as that of carbon if fully investigated. The gaseous character of the oxides of carbon, as contrasted with the fixed nature of the silicium and boron oxides, and the difference between the hydrogen compounds of one and the others, are much against the view which some chemists have adopted. At the same time it must not be forgotten that the number of organic compounds containing silicium is increasing daily.

Surprise.—We have the best reason to believe that the rumour of the "distinction," which it is said is about to be conferred, is unfounded. It would in fact be almost an insult to offer it to such a man.

W. Gellett.—Dr. Odling will deliver Four Lectures on Chemical Combination, after Easter, at the Royal Institution.

Student.—Apply to Ernest Hart, Esq., Dean of the School, St. Mary's Hospital Medical School.

Haffa.—The existence of native zinc in nature is considered somewhat problematical.

H. Hordliczka.—Your communication has been forwarded to the required address. You will most likely hear direct by post. If any difficulty occurs about sending letters we will communicate through this column.

N., Hoxton.—Write to the Secretary, Burlington House, W.

Communications have been received from Dr. Muspratt ; J. C. Lee ; H. R. Marsden ; D. Dawson ; Dr. Quesneville ; S. Rowland ; L. Wundt ; Rev. J. T. Burt ; J. L. Tgelström ; Runcorn Soap and Alkali Company ; F. C. Clayton ; T. Charlesworth, jun. ; Captain W. A. Ross ; J. F. Sprague ; J. How ; J. Walker (with enclosure) ; Dr. Andrews (with enclosure) ; V. Cruse ; Dr. Herapath, F.R.S. ; Dr. W. Kellner ; J. Butterfield ; R. Nicholson ; F. C. Calvert and Co. ; Rev. A. Rigg ; Dr. Letheby (with enclosure) ; W. H. Exall (with enclosure) ; M. Murphy (with enclosure) ; L. Benzon ; J. C. Bell ; J. Sprague (with enclosure) ; E. M. Delf ; J. Spiller ; W. J. Day ; Professor Heaton ; C. R. C. Tichborne (with enclosure).

Books Received.—"Street Tramways for London," by Charles Mackay, LL.D. London : P. S. King ; "First Step in Chemistry," by R. Galloway, F.C.S., fourth edition. London : Churchill & Sons.

Scholl's Patent Platinum Gas Light Perfecter.—Extract from Report by Dr. Letheby:—

"The results have been very remarkable, for they show an average increase of 63 per cent on the illuminating power of the gas. I am of opinion, therefore, that the invention is of great practical value.' Price One Shilling each for Fish-tail Burners. To be had retail of Gas-fitters and Ironmongers, and (wholesale only) of JOHN SCHOLL, 41 and 42, Berwick Street, Oxford Street, London, W. Terms on application. N.B.—A specimen sent free on receipt of 12 stamps.

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Pharmaceutical Society of Great Britain.—SCHOOL of PHARMACY.—SESSION from OCTOBER to JULY.

LECTURES.—On Chemistry and Pharmacy, by Professor Redwood. On Botany and Materia Medica, by Professor Bentley.

LABORATORY for Practical Instruction in General and Pharmaceutical Chemistry.—Director, Dr. Attfield; Assistant, Mr. Tilden.

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Instruction in Practical Chemistry and Evening Classes for the Study of Chemistry, Botany, Materia Medica, &c.
TO PHARMACEUTICAL AND OTHER STUDENTS.

Mr. J. C. BRAITHWAITE, for thirteen years Principal Instructor in the Laboratories of the Pharmaceutical Society of Great Britain, and Demonstrator of Practical Pharmacy, Pharmaceutical Latin, &c., wishes to inform his old Pupils and others that he continues to devote his whole attention to Education. The Session 1867—1868 will commence on the 1st of October, when

Mr. BRAITHWAITE'S Laboratory, which has been enlarged during the recess, will be re-opened at 10 a.m. for Instruction in Practical Chemistry as applied to Pharmacy, Medicine, Analysis, &c. Pupils can enter at any period. Terms moderate.

THE CHEMICAL and TOXICOLOGICAL CLASS meets as usual every Monday and Thursday Evening, at 8 p.m., commencing October 3rd.

The LATIN CLASS for the reading of the Pharmacopœia, Physicians' Prescriptions, &c., every Tuesday and Friday Evening, at 8 p.m., commencing October 1st.

The BOTANICAL and MATERIA MEDICA CLASS, every Wednesday and Saturday Evening, at 8 p.m. The usual EXCURSIONS for the STUDY of PRACTICAL BOTANY will commence at 10 a.m., October 2nd.

Fee to either of the above Classes Half-a-Guinea per Month; to the Botanical Excursions only. Half-a-Guinea per Eight Lessons, payable in advance. Pupils can enter at any period.

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Mr. Braithwaite receives a few Pupils to board in his house.

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Mr. J. Tennant, Geologist, 149, Strand, London, W.C., can supply Elementary Collections of Minerals, Rocks, and Fossils, to illustrate the Works of Ansted, Lyell, Jukes, and others, on the following terms:—

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For the most delicate Chemical Assay, and Bullion Balances, were awarded to L. OERTLING of Store Street, by the Jurors of the Great Exhibition of 1851, and the Paris Exhibition of 1855, which honour has been confirmed by the award of the Prize Medal of the International Exhibition of 1862, to the present firm of

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CANDLES, GLYCERINE AND SOAP. A Gold Medal was awarded at the Paris Exhibition to Price's Patent Candle Company, Limited, for "Candles, Glycerine, and Soap"—the only one to any British Exhibitor for these three things combined. The chief Candles of the Company are their "BELMONTINE" and "PRICE'S PARAFFINE" for those who must have the extreme transparency of pure Paraffine; their GOLD MEDAL PALMITINE" and "SHERWOOD PALMITINE" for those who, while desiring candles of great beauty, require also steady brilliancy of light and freedom from smoke and smell; their good old-fashioned "BELMONT SPERM AND WAX," and "BEST," "No. 2," "No. 3," and "BATTERSEA" COMPOSITES for those who require only perfect burning without caring for transparency; and their "CHAMBER" Candles, hard, and of small diameter to avoid the dropping of grease when carried. Their new toilet soap, "PRICE'S SOLIDIFIED GLYCERINE" contains half its weight of their distilled Glycerine, and should be the one toilet soap in use, especially in winter, because of its admirable effects in preventing chapping of the hands and face. There ought also to be in every house one of the sealed bottles of their patent distilled Glycerine, known everywhere as "PRICE'S GLYCERINE," two or three drops of which, mixed with three or four times as much water, will in a day or two remove chapping and roughness of skin, whether of adults or children; and when this is effected, a single drop of the undiluted Glycerine applied once a day will prevent the recurrence of the chapping and roughness. Insist on having "Price's Glycerine" in the Company's own sealed bottles, quantities of cheap impure Glycerine being now sold in the shops because of the low rate at which the dealers can buy it in comparison with Price's. All the good medical authorities abroad as well as at home order "PRICE'S" as the one only Glycerine to be used.

"PRICE'S NEW PATENT NIGHT LIGHTS" for burning in the wide glasses, are believed to be the very best Night Lights made. "PRICE'S CHILD'S NIGHT LIGHTS" are known everywhere and are excellent for burning without a glass.

THE CHEMICAL NEWS.

VOL. XVII. No. 425.

ON SOME POINTS IN CHEMICAL GEOLOGY.

By DAVID FORBES, F.R.S., &c.

No. II.—DR. STERRY HUNT'S GEOLOGICAL CHEMISTRY.

IN the CHEMICAL NEWS of October 4, 1867, I commenced some remarks under this title, for the express purpose of exciting more interest in the application of chemistry to geology, and with the hope of starting a discussion, which might at the same time enliven as well as elucidate the subject. Accepting Dr. Hunt's invitation, his views, being the most recent, were first selected for consideration; and, although that gentleman now appears greatly astounded at my presuming to differ from his opinions, it is still highly gratifying to find that he has at last condescended to reply.

As this reply, however, contains absolutely nothing which can in any way affect or modify the opinions which I have already expressed on the views of Dr. Hunt, or even require a reconsideration of the arguments upon which those opinions were based, I am enabled to reply *tout de suite*.

Dr. Hunt adopts a line of argument which is an elaborate attempt to convince his readers of the utter incompetency and ignorance of his reviewer; yet at the same time, it is amusing to observe that the character and tone of his remarks, in conjunction with his studious avoidance of some of the knotty points, and more important arguments brought forward in opposition to his views, are strikingly suggestive of his being in reality ill at ease, and possibly afflicted with a presentiment that there may, after all, be some rickety points in his theoretical views.

Men who live in glass houses should not throw stones; Dr. Hunt's accusations of ignorance will appear strange to those who have paid attention to some of his sweeping assertions: amongst others, for example, when he emphatically declares that quartz "can only be generated by aqueous agencies," geologists will infer that Dr. Hunt must be ignorant of the most important fact that quartz is found in abundance in volcanic lavas in many parts of the world, although not in Canada.

Had Dr. Hunt remained content with his Canadian laurels, he would probably have enjoyed them in peace without having his opinions disputed, but when he now aspires to be recognised in Europe, he cannot complain if his views be criticised by any, or all, of those interested in the subject; an ordeal which must be undergone before he can expect them to receive general acceptance, for surely he does not issue them as axioms or oracles.

Europe differs greatly from Canada, and, amongst other things, in close competition being the order of the day. No man in Europe can expect to retain any portion of the field of science exclusively for himself, or to travel alone on any of the many different roads which lead to one and the same scientific truth.

If real progress is to be made in science, the student must reason for himself and not be content with accepting, merely on authority, opinions which are inconsistent with his own deductions or experiments; nor should he be deterred by the opposition to be expected from those already in office or authority, who are sure to be jealous of intruders on what they imagine to be their own domain, and, doubtless, also dislike having their peace of mind disturbed by innovations.

A discussion of this nature may be carried on in two ways; either by considering the main points of the argument first, before engaging the minor details, or the reverse. Dr. Hunt prefers the latter course, which no doubt is best suited to the defence of a weak cause, but which, as his rather rambling remarks in last week's CHEMICAL NEWS* will show, is not calculated to convey to his readers any very clear idea of the exact points at issue, and likely to confuse by the number of minor details having little or no bearing on the main questions.

It is therefore most important to me that no misunderstanding should arise as to the exact points on which I have presumed to differ from the principles of chemical geology which Dr. Hunt has recently brought before the scientific public in Europe.

Expressed in as few words as possible, I object to the following of Dr. Hunt's assumptions or assertions:

1. That the earth is solid to the core.
2. That the surface of the earth immediately previous to its entire solidification was "a liquid bath of no great depth surrounding the solid nucleus."
3. That the original atmosphere contained "the whole of the chlorine in the form of hydrochloric acid—the sulphur as sulphurous acid."
4. That the saltness of the sea is due to a rain of hydrochloric acid "flooding the half cooled-crust" with a highly-heated acid deluge.
5. That the whole of "the calcareous strata—the marbles and various limestones which we find on the earth's surface"—have been precipitated from the sea by carbonate of soda.
6. That all the magnesian limestones and gypseous beds were formed in a dense atmosphere of carbonic acid.
7. That quartz "can only be generated by aqueous agencies."
8. "That granite is in every case a rock of sedimentary origin."
9. That volcanic rocks are merely ordinary sedimentary beds melted by being "depressed so that they come within the action of the earth's central heat,"

Any minor differences fall naturally under these heads, and I may add that the perusal of Dr. Hunt's defence has confirmed me more than ever in the belief that the above premises are unsound; and I shall now endeavour as concisely as possible to examine the arguments *pro et contra*.

1. That the earth is solid to the core.

Dr. Hunt seems to imagine that if the earth is not solid to the core, it can only consist of an immense central sphere of molten matter covered by a thin external crust or shell: for he wastes all his arguments in attempting to upset this theory, to which I had never given my adhesion. I have preferred adopting in the main the hypothesis of Bunsen, no mean authority, and when opposing Dr. Hunt's view, simply asserted my opinion, that the earth still encloses "a vast reservoir or reservoirs of still fluid igneous matter in its interior," and the main argument with which I support this opinion is, that I consider that the molten lava ejected from volcanoes must be derived from some such source. This is a very simple but common sense view of the case, which I imagine Dr. Hunt will find some difficulty in refuting.

2. That the earth's surface immediately previous to its entire solidification was "a liquid bath of no great depth surrounding the solid nucleus."

Hopkins has taken into favourable consideration, the supposition that the earth actually was solid both in its centre and crust, and yet might retain fluid igneous matter in the intermediate space, and taking a somewhat similar

* It is necessary to explain here that many of Dr. Hunt's observations refer to a previous communication in the October number of the *Geological Magazine*, and not to the subsequent one in the CHEMICAL NEWS of October 4th, which, as is distinctly stated therein, is only supplementary to the former, and to be read in conjunction with the same. Yet Dr. Hunt indulges in the absurd accusation that the contents of that communication "have for some unknown reason been withheld from the readers of the CHEMICAL NEWS."

view of the case, I believe that, even allowing that the solidification actually did commence at the centre, that it still could not have reached the exterior before, on the other hand, the surface itself had also solidified and formed a crust commencing from the exterior, due to the external cooling action. In opposition to this, Dr. Hunt states that silicates when cold are from one-seventh to one-sixteenth part more dense than when molten, and would at once sink down into the fluid mass below, and further adds that no crust could be formed unless the laws of gravity were suspended. I do not know what Dr. Hunt's ideas of the laws of gravity may be; but would again ask how far he imagines a crust of sp. gr. 2·6 could sink down into a molten sphere of a mean sp. gr. 5·3 ?.

I will not, however, repeat the other arguments which I have used in the *Geological Magazine*, but content myself by bringing forward one not before employed by me in support of my opinion.

Some experiments which I am now engaged in, on the effect of heat upon bodies which contract in cooling, *i.e.*, which are more dense when cold than when molten, show in the cases tried, that a body upon the first application of heat expands and continues to do so up to near its melting point, when it contracts at the instant of fusion; in other words, although the substance when cold was heavier than when molten, yet the same substance expanded by heat was lighter than when molten. Thus some metals were found to float about (like ice upon water) upon the surface of a molten bath of the same metal, into which they were placed in a heated condition*. It appears probable that the same phenomena would account for such a crust as Dr. Hunt disputes, not sinking, but floating on the molten bath below.

That the earth may possibly have solidified at the centre first, is not disputed by me, nor does its so doing in any way affect my theoretical views. The object of my observations on this head were to show that we are altogether too ignorant of the character of the central mass of the earth, and of the effect likely to be produced by such enormous pressures, to be enabled to reason on such insufficient data with any confidence in the result.

3. That the original atmosphere contained "the whole of the chlorine in the form of hydrochloric acid —the sulphur as sulphuric acid.

The perusal of Dr. Hunt's remarks does not in any way tend to modify the conclusions I had previously arrived at on this head; I still believe that chemists will not be disposed to regard an atmosphere containing enormous volumes of sulphurous acid, steam, and oxygen in excess, or in other words, which resembles a great sulphuric acid chamber, as *probable*, and as Dr. Hunt does admit that they would slowly unite to form sulphuric acid, it merely becomes a question of time as to whether they united slowly or quickly.

The arguments I advance against supposing that such an atmosphere ever did exist, are that I consider that the sulphur would unite mainly with the heavier metals, and the chlorine mainly with the alkaline metals, and I consequently infer, that these elements never went into the atmosphere in any such quantity as Dr. Hunt imagines.

Dr. Hunt in opposition states that sulphides could not be formed, since oxygen was in excess. Metallurgists know that sulphides are far less easily oxidisable than are generally imagined, and that they are produced in both blast and air furnaces, where the waste gases still contain unconsumed oxygen; and that time is an important element in this consideration.

But we have no proof whatever of any great excess of oxygen in the primeval atmosphere; on the contrary, we know that a vast amount of the oxygen now present in the air, must have been derived from the decomposition of the carbonic acid, when the immense supplies of carbon, afterwards buried in the various sedimentary formations were extracted from the atmosphere by the action of vegetable life. The slight excess of oxygen which no doubt was present would further be so diffused through the enormous volume of carbonic acid, nitrogen, and aqueous vapour, that it cannot be imagined to have exercised other than a most feeble oxidising action.

The carbonic acid, also, being so infinitely more dense, and present in so overwhelming quantity, would further act as a powerful shield against the very oxidising action which Dr. Hunt lays so much stress upon.

That the chlorine, also, did not go into the atmosphere as Dr. Hunt imagines (combined with hydrogen as hydrochloric acid), I infer, from the well known greater affinity which it has for sodium than for hydrogen, and the volatility of sodium would be far more likely to bring it in contact with the chlorine than with the silica.

The idea that the action of the feeble excess of oxygen above alluded to, in connection with silica and steam, would prevent the formation of chloride of sodium, is not of much weight; since the chloride of sodium would be formed as a vapour in the atmosphere, whilst the silica remained below in the earth's mass, in the solid form.

But Dr. Hunt next writes, "Even, if as Mr. Forbes supposes, the chloride of sodium were to be formed in the heated atmosphere, it would be precipitated into the intensely heated bath," &c.; *precipitated!* when it would be in the state of vapour at this temperature.

Metallurgists know how indifferent chloride of sodium is when fused with silicates, and to this property is due the employment of what is termed a salt cover in assays; however well the salt may be intermixed, once the mass is fused, it rises and swims on the top, and (if the heat be not too elevated, or protracted, as to volatilize it entirely) presents, upon cooling, a well defined crystalline crust of salt, below which is found the unaltered silicate slag, and below this again the button of metal, pure, or more or less in combination with sulphur, arsenic, antimony, &c., as the case may be; thus presenting, on the small scale, an illustration of what I have supposed may have occurred in nature, in which case, also, the cover, or crust of salt, would act as a shield against oxidation.

In a potter's kiln, the vapour of salt under confinement, merely glazes the surface of the ware to a minute depth, and this very glaze protects the silicates from further action: but both the potter's kiln and Gossage's soda process are worked under forced circumstances, not applicable in this argument; and when Dr. Hunt explains that in his illustration of this subject, he merely used the words "*if the elements were made to react upon one another*," is it not rather he who is trifling with the subject when he supposes conditions which never could occur in nature in the case referred to.

4. That the saltness of the sea is due to a rain of hydrochloric acid, "flooding the half cooled crust" with a highly heated acid deluge.

This assumption requires no further comments than those included under the preceding head, where I have endeavoured to show that the whole of the chlorine did not ascend into the atmosphere as hydrochloric acid, and, consequently, could not flood the earth with the hot acid deluge insisted on by Dr. Hunt.

5. That the whole of "the calcareous strata, the marbles, and various limestones which we find on the earth's surface" have been precipitated from the sea by carbonate of soda.

Geologists have long agreed that sedimentary limestones are the products of the action of organic life, and microscopists, in confirming this, have further proved that they do not possess the character of precipitates. Dr. Hunt evades any reply to these objections, but asks a question in return; requesting to know what becomes of the acid in case, as I contend, animals can utilise the salts of lime

* As a metallurgist I have frequently observed such cases, but for a long time did not understand the explanation; I have to thank my friend Mr. Hackney for directing my attention to the behaviour of Bessemer steel under these circumstances, as it gives much trouble to the workmen by persistently floating high on the surface of the melted steel (even when in pieces of 40 lbs. and more) as long as its temperature is below its fusing point.

contained in the sea. As is well known, sulphur plays a very important part in vital economy, entering both into the composition of organism, and being also given off as sulphuretted hydrogen in the gaseous form. I see, therefore, many reasons for believing that animals do assimilate the sulphate of lime, which we know is contained in such an enormous quantity in the ocean.

6. That all the magnesian limestone and gypseous strata were formed in a dense atmosphere of carbonic acid.

In 1846, when in Birmingham, I was informed that for some years the manufacture of magnesian preparations was based upon the reactions of the compounds of magnesia with carbonic acid in a compressed atmosphere of carbonic acid. In 1849, Mr. Osborne, a gentleman connected with a similar manufactory in Ireland, fully confirmed these statements, and shortly after the publication of Dr. Hunt's paper in the *Comptes Rendus*, Dr. Lawson, in the course of conversation, expressed his surprise at Dr. Hunt being unaware of this, since he knew that the principle had long been use in a manufactory at Cork.

Dr. Hunt has further applied this principle,* and obtained very interesting results, which he considered to be the counterparts of nature's operations; and remembering that there are dolomite beds in the lower silurian strata of Canada, at once asks geologists to believe the rather hasty generalisation that all the magnesian limestones and gypseous beds were formed in a dense atmosphere of carbonic acid.

Geologists, however, well knowing that the grand development of magnesian limestones and gypseous strata occurred in periods when air-breathing animals existed on the surface of the globe, could not believe that these animals actually lived in a dense atmosphere of carbonic acid; and had some of the more modern great gypseous formations occurred in Canada, Dr. Hunt would probably not have brought forward this theory.

7. That quartz "can only be generated by aqueous agencies."

Dr. Hunt, wisely, no doubt, does not take any notice of my arguments against this assertion, since they are facts, not opinions; and consist merely in pointing out that the volcanic lavas of Italy, Hungary, Peru, Bolivia, Chili, &c., contain abundance of quartz, often in well-defined crystals. In connection with this, I may here extract a passage from a letter received from Mr. Sorby, who writes: "I have splendid cases of recent lavas with quartz, both in the shape of small crystals and as rounded masses, like those seen in some older rocks, and this quartz, in both cases (crystals and rounded masses), contains splendid glass cavities just like those in the felspars, the Arran pitchstone, and the various lavas; thus we have complete proof, according to my views, that quartz both can and has crystallised out from a melted mass of rock."

Now, in face of such facts, what importance, may I ask, can be attached to such of Dr. Hunt's dogmatic assertions as "that the composition of the primitive crust would have excluded free silica;" that quartz "is only the result of a secondary process," &c.?

8. "That granite is in every case a rock of sedimentary origin."

Dr. Hunt makes this assertion in opposition to the opinion of many able men who have well studied the subject. If he, however, only founds this opinion on the presence of quartz in granite, the value to be attached to it may be inferred from the remarks contained in the preceding paragraph.

If he speaks as a geologist, it may fairly be inquired whether he considers his Canadian experience sufficient to enable him to arrive at such sweeping generalisations.

Sir Charles Lyell has stated that three things were essential to a geologist, namely, "to travel, to travel, and to travel;" and such advice may be recommended to Dr. Sterry Hunt before he ventures again to generalise for the world on the strength of a local knowledge of a very minute part of the same.

9. That volcanic rocks are ordinary sedimentary beds melted by being "depressed so that they come within the action of the earth's central heat."

In the *Geologcal Magazine* I ventured to inquire of "the author of this ingenious theory, by what mechanical arrangement he supposes strata on the surface of the earth to be lowered down into a globe solid to the core;" and again, "How are we, according to this theory, to account for the fact, that volcanic rocks, taken from any quarter of the world, no matter how far distant from one another—from Iceland or Terra del Fuego, from the islands of the West Indies, or from those of Polynesia,—that in all cases such rocks possess an absolute identity in chemical and mineralogical composition, in physical and optical properties. Can any geologist be expected to believe that such rocks have been formed by the melting up of a mere mechanical aggregate of rock débris, possessing no analogy whatsoever, and whose chemical composition, &c., is known to vary to the widest imaginable extremes?" Questions as yet unanswered.

Before concluding these remarks, I would here acknowthat Dr. Hunt has discovered an inaccuracy which occurs in my communication to the *Geological Magazine*, where the position of steam in the imaginary original atmosphere is by accident placed below that of air, although steam is in reality lighter, as a moment's reflection would have shown. This error has not the most minute influence on any of my generalisations, as it is perfectly immaterial whether this stratum be above or below that of air.

I shall always be ready to admit at once any error which may be found in my communications; still Dr. Hunt is quite entitled to make the most of such a blunder if he thinks it will support his views; at the same time I trust that he will also be equally candid in cases where he may be found tripping.

Dr. Hunt alludes to a rough sketch of some of my views contained in the *Geological Magazine*; but, as I have already accepted the invitation of the Council of the Chemical Society to give a lecture on chemical geology (20th February next), Dr. Hunt will thus be enabled to take my views into full consideration, and after comparing them with his own, I trust will give us the benefit of his scrutiny; for, as I regard the ultimate object of all my labours as being the attainment of scientific truth, I am as fully prepared to be corrected in points where I may be proved to be wrong, as to defend those which I hold to be right.

London, January 20th, 1868.

Simple Method for the Extraction of Phosphoric Acid from Glass.—From a consideration of the nature of either the basic or acid * materials used in the manufacture of glass, the presence of phosphoric acid in this substance would naturally be suspected, but as far as I know, this acid does not enter for certainty into any of the analyses of glass hitherto published; I am therefore induced to describe the following simple process for its extraction. Pound the glass to be examined extremely fine, and shake it up with twice its volume of weak ammonia, and allow it to rest until the supernatant liquid is clear, which will be in about 24 hours afterwards; the clear liquid is then ready for the molybdenum test. By this process I have already detected phosphoric acid in German glass test tubes, in Bohemian combustion tubes, and in several other glasses, into the manufacture of which lead does not enter.—*William Skey, New Zealand.*

* *Vide* CHEMICAL NEWS, Sept. 13, 1867, p. 148.

* See in a former paper entitled, "On the property of silicic and Tungstic Acids to retain Phosphoric Acid, &c., CHEM. NEWS, Vol. xvi. p. 187.

ON

HEAT AND COLD;

A COURSE OF

SIX LECTURES*

(Adapted to a Juvenile auditory),

DELIVERED AT

THE ROYAL INSTITUTION OF GREAT BRITAIN,

(CHRISTMAS, 1867—8),

BY

JOHN TYNDALL, Esq., LL.D., F.R.S.

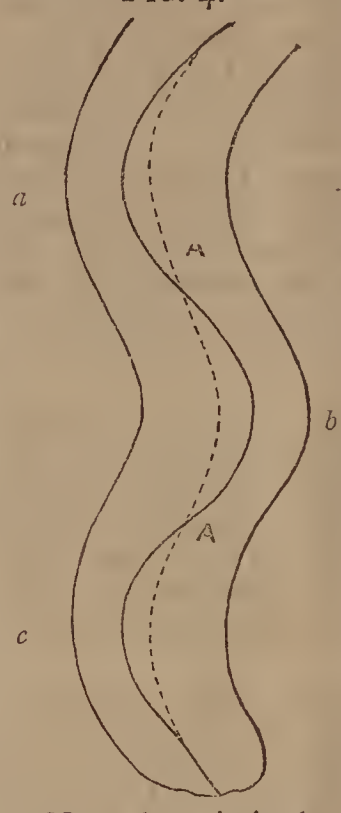
LECTURE III.

(Continued from page 32.)

The Geysers of Iceland.

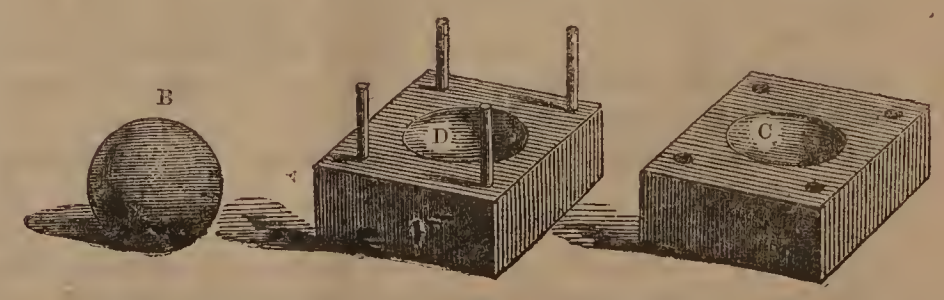
I want to show you now how it is that ice can behave like treacle, or honey, or tar—how it is that it behaves like lava, or paste, or a viscous body. In order to make this plain I have asked Mr. Cottrell to bring me in a mass of ice; and I hope to be able to show you by experiments in this room that we can make ice behave almost like a piece of paste—that we can mould it into any form we please. Here is our ice, and we will place it on the table in this blanket. It is clinging to the blanket, being, in fact, frozen to it. I will show you how, from an apparently little thing, we can get an explanation of a fact observed in the glaciers. This explanation is due to a little fact first observed by the greatest experimental philosopher that this world ever produced—a man who is to my feeling almost living here amongst us at the present moment—a man who lectured to the boys here, and who himself had all the tenderness, and all the brightness, and all the joyousness of a boy. I say it is by a little observation of this great man that we are able to explain those facts observed in connection with the glaciers, and to show how it is that a body so brittle as ice can behave almost like lava. I will show you the brittleness of ice. I have here a pointed instrument, a small awl, and if I prick this into the ice you see that it chips off little pieces, and that the ice breaks as clearly as any crystal would break. Now just observe what occurs among these glaciers. If we make accurate measurements upon this *mer de glace* we ascertain a very striking fact. You see in the diagram a great white glacier. Here you see another, and you see another there. I measured the width of the first glacier, and it was 1,134 yards. The second glacier is 825 yards; and the third 638 yards. If you add these together, the sum of the widths of these three tributaries of the Mer de Glace is 2,597 yards. Now, all of these three tributaries of the Mer de Glace are squeezed into a space, which measures only 893 yards,— a channel only one-third of the width of the sum of the three tributaries. Now it is one of the wonderful properties of this ice that it can be thus squeezed into a narrow bed. If we take a number of stakes and set them in a perfectly straight line across this channel, and allow them to remain there for a day, and observe their position on the following day, we shall find that they are no longer in a straight line. In the observation that was made there were no fewer than 16 stakes fixed in the ice in a straight line. The stakes nearest one side of the glacier moved at the rate of 7 inches in a day; the next stake moved at the rate of 8 inches—the next 13 inches—the next 15 inches—the next 19 inches, and the next 20 inches; and then the speed began to fall off, and fell back to 15 inches at the other side of the glacier. These numbers prove a fact which is also observed in the case of rivers—that the middle of the line moves more quickly than the sides. In the same way, as was proved by Principal Forbes, the top of the glacier moves more quickly than the bottom, or the part nearest its bed, which is held back by the friction of the bed. When I visited the Mer de Glace in 1857 there was a precipice of ice, and I measured the motion of that precipice at the top and at the bottom. The top stake moved 6 inches, while the middle stake moved 4½ inches, and the bottom stake moved 2½ inches. This shows that the top of the glacier moved more quickly than its foot. Furthermore—and this is a point of great importance—if you had a river flowing through a straight valley, the middle of the river would be its point of quickest motion; but if you had a river flowing through a valley of this kind (Fig. 4) the point of quickest motion would be always that point where it is curved. It is exactly the same with a glacier. This on a large scale will represent the bed of the Mer de Glace from actual measurement. At the parts A A the point of swiftest motion is really the centre of the glacier. Here, again, at *a* and *c*, the point of swiftest motion is on one side of the centre. Here, again, at *b*, it crosses to the other side of the centre. The dotted line is the centre, and the continuous line marks the points of the quickest motion on the Mer de Glace.

FIG. 4.



Now, how is it that a glacier is thus able to behave as a river? We will see. I will now cut two pieces from this block of ice. We see that the ice is now melting in the atmosphere of this room, and there is no surplus cold in it to enable it to freeze again; and still, strange to say—(and this was the observation that Mr. Faraday made)—if we place those pieces of ice together, though the surfaces are now melting, they instantly freeze together. Although there is no surplus cold in the ice, the mere bringing them together causes the film of water, which a moment ago was moisture, to become ice. This curious freezing together has received the name of "regelation," a term for which those who first worked at the subject were indebted to Dr. Hooker. In consequence of this freezing together you can actually convert snow into ice. Every boy knows the state of snow which is fit for a snowball. It ought to be soft, and yet by proper pressure you can make it perfectly hard if you are wickedly inclined. Now, I have no snow here, but I will try and obtain snow by scraping the surface of the ice. In this way I get a kind of snow, and here is a flannel in which to receive it. I will take this snow and put it into a proper mould C D, and

FIG. 5.

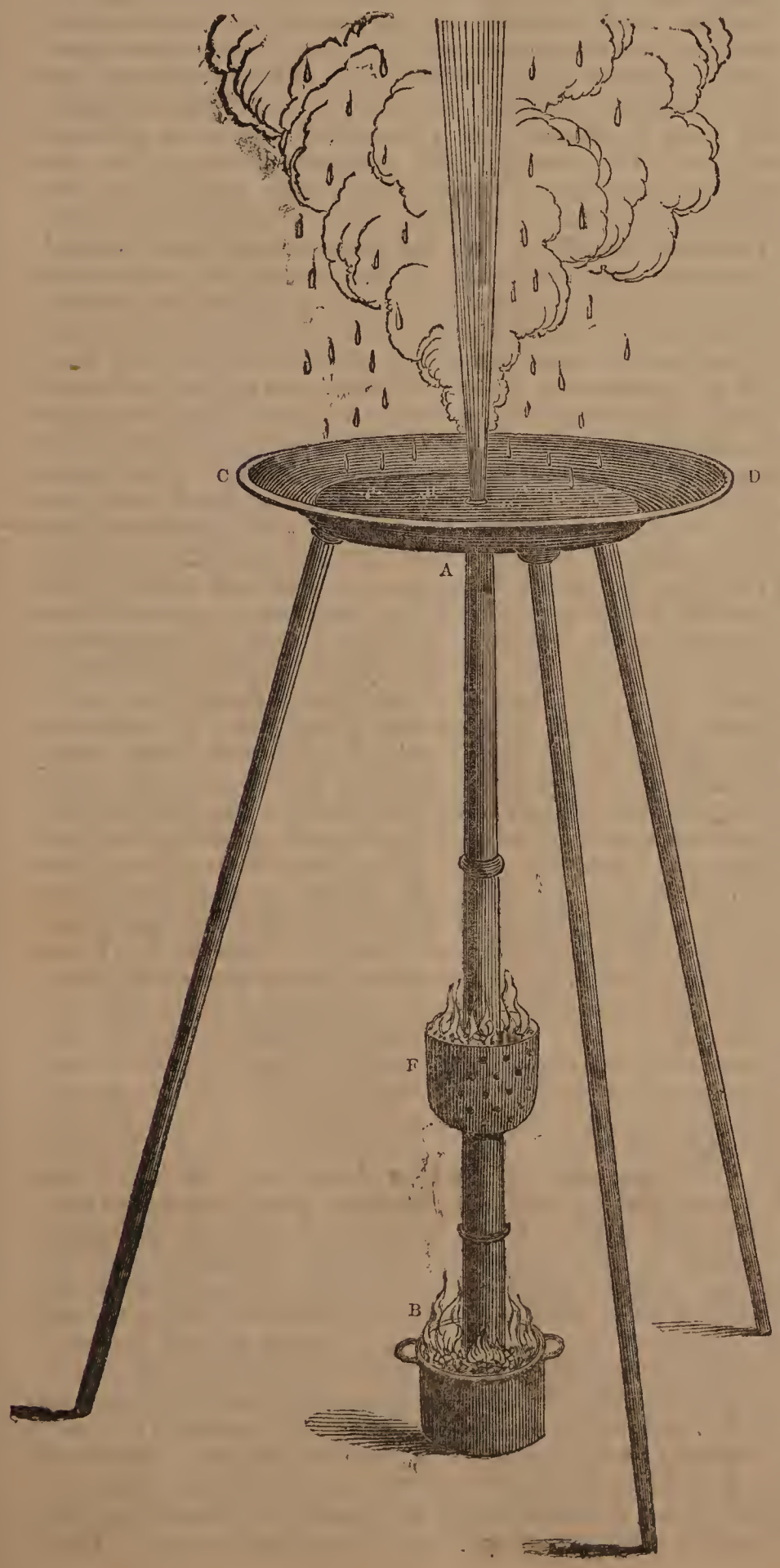


squeeze it together. In the absence of real snow I make the snow required for the experiment by crumbling the ice in this way. I will now make a snowball, and I am enabled to do this by the power which the small particles of ice have of freezing together in the manner I have just indicated. I cannot by my hand squeeze strongly enough the mould containing these particles of scraped ice; and therefore I will place the mould under the hydraulic press, as this machine is called. In this way I hope to obtain a snowball. [The operation described was then

* Reported verbatim, by permission of the Author, for this journal.

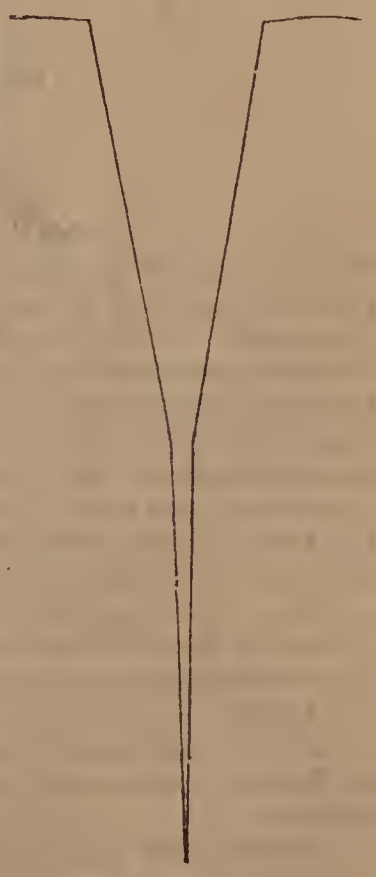
performed, and the mould, on being withdrawn from the press, was found to contain a ball of solid ice.] Now, here we have a snowball (B), such as you have never seen before, and this is due to the fact that on bringing the surfaces of the little particles of ice in contact they freeze together. This is not an ordinary snowball at all, and it is one which no boy would like to be hit with. It is a ball of solid ice, produced from the small particles which have frozen together in virtue of this wonderful property called regelation; and it is in virtue of this property that ice on the surface of water, though shattered into pieces, will mend itself; and all the tearings and ruptures of the glaciers are mended by means of this quality of regelation which was discovered by Mr. Faraday. I have here several experiments arranged to illustrate this subject. [Particles of scraped ice were then moulded into the form of rings and hemispherical cups, by the same means as had been employed in the production of the solid ball. Two hemispherical cups were afterwards placed with their edges in contact, when they froze together and formed a hollow sphere of ice.] These experiments will show you on a small scale how possible it is for particles of a substance perfectly brittle to freeze together wherever they touch, on account of the substance possessing the power of regelation. You see that a substance of this character behaves as if it were not brittle at all, and acts like a paste. In this way we might make statuettes, or, in fact, mould the ice into any form we pleased. You might drink out of these cups, and the ice of which they are made would cool the water for you. I am sorry I have not a little cooled wine to offer you from a cup of this kind. (Laughter.) I have made champagne glasses and all manner of things by thus compressing ice. In this way by these small experiments we illustrate and make plain to ourselves those wonderful things that go on among the glaciers of the Alps; and we entirely clear up the difficulty as to how it is that a body so brittle as ice can behave as a viscous body. I must now leave this subject of ice and its properties.

Fig. 6.



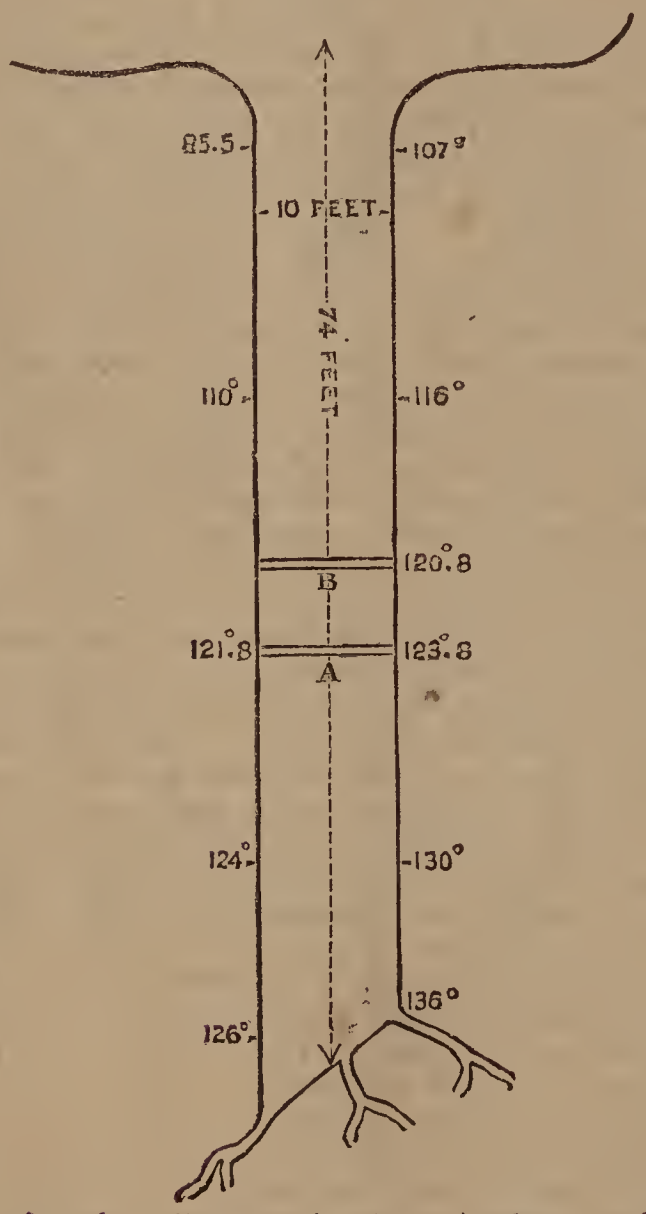
There is in operation before you an apparatus for illsutrating the action of the geysers in Iceland; and in the other room is a beautiful painting of the geysers, presented by our president, Sir Henry Holland, who was there in 1810 with Sir George Mackenzie. In a short time this tube will throw out a column of water, but I do not think I shall be able to make the operation plain to you in this lecture. When Sir Henry Holland and Sir George Mackenzie visited the great geyser, Sir George Mackenzie supposed that the geyser had underneath it a great cavern, and that this was partly filled with water, the geyser itself being a tube. He supposed the water to become heated beneath, and the steam to force the water up into the tube. This is the theory given by Sir George Mackenzie; but it is not at all necessary to suppose the existence of this cavern. The spring itself has built its own tube, and the tube is a sufficient apparatus to produce these wonderful eruptions that astonish everybody who has ever seen them. The geyser tube is represented here in section (see Fig. 8). It is seventy-four feet deep, and is lined with a most beautiful plaster. It opens out at the top into a basin fifty-two feet wide in one direction, and sixty feet wide in the other. [The apparatus for illustrating the geyser was then put in action, and a thick stream of boiling water was presently ejected upwards. (See Fig. 6)]. Now I must make another eruption for you. I want to produce an imitation of the spring called the strokkur (shown in section at Fig. 7). This is a very celebrated spring which you will see in Sir Henry Holland's painting beside the real geyser. (I must explain in the next lecture how it is that we have two fires in this apparatus.) It is usual for the natives of Iceland to stop the mouth of the strokkur by throwing in clods. I will now imitate that practice by putting in a cork at the end of the tube. In a short time the cork will be ejected, and I should not be at all surprised if the water reached the ceiling. I think the last experiment made at the strokkur was made by Commander Forbes. He wrapped a leg of mutton in a towel and stopped the mouth of the strokkur by means of that leg of mutton. The leg of mutton came out well cooked, and was projected to a great height in the air. Various people have estimated the height of

Fig. 7.



these eruptions in Iceland. Sir Henry Holland tells me that he saw one of more than one hundred feet; and Sir George Mackenzie gives ninety feet as the height of the eruption. The earlier observers made the height very much more. Two Danes, named Aulafsen and Paulson, who were the first to observe the height, state that the geyser pitched its water to a height of 360 feet. Two observations, which may be regarded as perfectly trustworthy, were made by Bunsen, of Heidelburg, and the height was measured by a theodolite. In the last of these observations, which was made on the 16th of July, 1841, the height was estimated at 162 feet, and we may rely upon this observation as being accurate. Now, as I have said, the tube of the geyser is the cause of the eruption; and when we see an eruption produced by a small tube, as in this model, we may regard it as proved that it alone is a sufficient cause, and that there is no need for the supposition that there is a cavern underneath. Bunsen suspended thermometers at various depths below the basin of the geyser to ascertain the temperature of the water. I have marked on this diagram the various temperatures

Fig. 8.



which he found at different depths. At the top the temperature was 8·45° C., and extended to 126·5° C. as the depth increased. Now, how is it that the water does not boil in the geyser when the temperature is over 100° C.? Every boy here will be able to tell me that it is because the water at that depth has to bear not only the pressure of the atmosphere, but also of the mass of water which is above it in the tube. For this reason it cannot boil at the temperature which Bunsen ascertained. At the depth at which the water in the geyser was found to have a temperature of 126·5°, the boiling temperature would be 136°. At no point does the temperature of the water reach the boiling point for the pressure to which it is subjected.

[At this stage of the lecture the cork flew from the mouth of the model of the strokkur, and a copious stream of boiling water was projected to the ceiling of the theatre.]

I must defer the explanation of the action of geysers until the next lecture.

DR. LETHEBY ON FOOD.

Lecture I.

(from our own reporter.)

On Monday evening, the 20th instant, Dr. Letheby, at the rooms of the Society of Arts, John Street, Adelphi, delivered the first of a series of four lectures "On Food." The Chairman—W. Hawes, Esq. President of the Society—having briefly introduced the lecturer as "our best authority on the subject about to be illustrated," Dr. Letheby commenced his lecture by observing that the economy of food was a subject of national importance; that muscular strength was co-equal with the amount and quality of food taken into the body; and that calamity and actual want, the absence of proper diet, the neglect of protection against weather, and disordered sanitary appliances, fell heaviest upon those least able to bear the burden. Bad, however, as the immediate consequences are, they are nothing to the sickly race which comes afterwards. We must not overlook the nutritive value of our raw materials of foods, and we must therefore endeavour to erect a standard of comparison between different articles. The erecting this standard is a subject of the greatest difficulty. First, where some article may be considered without any other—milk or bread for example. Then the difficulty is that the proportions of these several constituencies differ in a great degree from the constituents of most other articles by certain different proportions of the several constituents. Again, as nitrogenous matter in food is the most important object, chemists ask how much nitrogenous matter is there in any given article of food? But even this test was not correct, even though it bore the support of Liebig, for upon his principle that an adult must take into his frame per diem 1,200 grains of nitrogenous matter; provided that he lived solely on beer or porter, he would have to drink 180 pounds of the said liquid to make up the required number of grains. The question now comes, what is the exact relative proportion of nitrogen and carbon necessary to sustain man without much labour. Dr. Lyon Playfair has sought in our workhouses, Dr. Edward Smith amongst the Lancashire weavers, to see the minimum a person can exist upon. Dr. Lyon Playfair says 4,100 grains of carbon to 190 grains of nitrogen; and Dr. Edward Smith (who observed the Lancanshire weavers just at the point when the food was failing to support life), that the proportion an adult woman required was 3,900 grains of carbon and 180 grains of nitrogen; whilst for an adult man 4,300 grains of carbon and 200 grains of nitrogen were required; the average of which is 4,100 grains of carbon and 190 grains nitrogen (thus agreeing with the statement of Dr. Lyon Playfair), contained in two pounds three ounces of bread. This statement almost agrees with another set of observations taken on a different principle; but as regards the chemical properties of the nutritive value of food, it may be convenient to observe that the relative proportions of the two in any article must be as 100 of carbon to 5½ of nitrogen.

I will now proceed to take a glance at the various kinds of food, first of all remarking that there is no such thing as animal food, but that all food, directly or indirectly, belongs to the vegetable kingdom. Animals have no power to construct food; their functions, instead of building up, pull down food, and although therefore whilst we are eating meat we may say we are eating animal, yet indirectly we are eating animal food; and it is with this primary idea that all food belongs to the vegetable kingdom that I commence my review of the raw materials of food.

Wheat is the first and the most important. There are two kinds, the summer and the winter wheat. Seasons, soils, and climates affect the quality of the crop; the warmer the weather, the richer the grain. Here

(pointing to the wall) is a diagram showing the construction of wheat. A cone, the outer covering of which is composed of fibrous and woody matter, in the interior of which a farina peculiar in its form is to be observed. There are various kinds of wheat, the best white tails, fine pollards, coarse pollards, and bran; and although taken per bushel the bran is the cheapest, yet taken as per 20 lbs. it is found to be dearer than many shades superior quality. Dr. Letheby then went into the details of the chemical composition of wheat, showing that it was composed upon a principle which made it a most wholesome food, stating that the relative proportions of nitrogen and carbon were as 1 of the former to $6\frac{7}{10}$ths of the latter. He also observed that whilst the best wheat was made into flour, he thought there was a fact which proved that the coarser kinds were rather too much neglected, for, in proportion as we went lower down the scale in coarseness it was found at the same time that the quantity of nitrogenous matter increased in a large degree. Dr. Letheby then successively went through the various kinds of grains, barley, oats, rye, maize, rice, showing in each case the chemical composition which indicated their respective nutritive values; showing the reasons that they either possessed no gluten, or else too much fat—why it was impossible to make them into bread; showing also, as in the case of rice, that a combination with some substance containing a great amount of nitrogenous matter was required to make them palatable; passing to a consideration of succulent vegetables, devoting some space to the consideration of the potato, stating what an effectual remedy it was against scurvy, and saying a few words respecting the other well known vegetables; then proceeding to a brief consideration of fish food; and turning his attention lastly to animal food, referred to the preference given by the poorer classes to bacon instead of butcher's meat, for the reason that it was cheaper to buy, easier to keep, was easier to cook, and less of it wasted away during this operation, and possessed a greater relish; concluding by observing that whilst the large amount of provisions that were required to feed near upon three millions of souls, were brought almost to our very doors with the regularity of clock-work, that this regularity was effected not by Governmental help nor municipal interference, but by the mighty influence of free trade.

Loud and prolonged applause greeted Dr. Letheby at the close of his lecture, and he informed his audience that the very perfect specimens by which he had illustrated his lecture had been kindly lent him for the occasion by Mr. Twining from his Economical Museum at Twickenham.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, January 16.

Dr. WARREN DE LA RUE, F.R.S., &c., President, in the Chair.

THE minutes of the previous meeting were read and confirmed. Messrs. G. W. Child, Edward Chapman, W. G. Mason, P. Griess, and Captain Alexander Walker were duly elected.

Mr. Martin Murphy, of the College of Chemistry, Liverpool, was proposed for election, and the certificate read for the first time. The names of the candidates read for the second time were—Herbert M'Leod, Thomas Charlesworth, Robert Schenk, and John Wallace Hozier.

A paper on the "*Isomeric Forms of Valeric Acid*," by Mr. ALEXANDER PEDLER, was read by the Secretary.

The author separated the two varieties of amylic alcohol known as active and inactive, by conversion into baric sulphamylates, and fractionally crystallising. The amylic alcohol was then separated from these salts. By oxidation, valeric acid was obtained from the two varieties. The valeric acid yielded by the inactive variety (*i.e.*, that resulting from the further treatment of the less soluble sulphamylate) boiled at 175° C., and had no action on a polarised ray. The valeric acid yielded by the rotating alcohol (separated from the soluble sulphamylate) boiled at about 170° C. It rotated the ray 43° to the right.

Dr. DEBUS advanced some hypothetical views concerning thioformic acid—the acid which is obtained in combination with lead, when formiate of lead is treated with sulphuretted hydrogen. It contains the elements carbon, hydrogen, sulphur, and oxygen. Dr. Debus referred to the inability of Professor Limpricht and Mr. Herst, who have analysed this substance, to obtain concordant results. He showed that a relation might be traced in the analytical results between the carbon and the hydrogen, while in the case of the sulphur and oxygen there was an utter absence of this. But if the numbers of the sulphur and oxygen were added together they then gave figures bearing constant relation to the carbon and hydrogen. Dr. Debus gave the following formulæ as the expression of the analytical results obtained for two specimens:—

(1.) $CH_2O+2(CH_2S)$.

(2.) $CH_2O+3(CH_2S)$.

Dr. Frankland's lecture "*On Water Analysis*" followed.

Dr. FRANKLAND said—"Having for some time past been engaged, in conjunction with my late pupil, Mr. Armstrong, in endeavours to place some of the determinations of water analysis upon a sounder basis, I proposed to give the results of our experiments to the Society in the usual form of a paper, when our indefatigable senior Secretary suggested that the paper should be elevated to the rank of a lecture. To this suggestion I was at first much opposed, considering that the Society had already, and even quite recently, received several important communications on this branch of chemical analysis. At last, however, I yielded to Dr. Odling's persuasion, but, in doing so, distinctly cast upon his shoulders the responsibility of summoning the Fellows to hear, under the title of a lecture on water analysis, what I fear will merely prove a dry communication on a few points only connected with this large subject; for, in the first place, I have no intention of discussing the whole subject, but only that portion of it which deals with those determinations comprehended under the term 'partial analysis of a water;' and secondly, even in reference to this corner of the subject, I have, except in one department of it, little that is novel to bring forward. So many difficulties surrounded the subject at the time this investigation was undertaken, that it was perhaps considered the least satisfactory of analytical processes. The difficulty chiefly experienced by water analysts was the determination of the organic matters and the mineral products derived from these, viz., nitrous and nitric acids and ammonia." The lecturer mentioned the names of Hofmann and Blythe, Weltzien, Dr. Miller, and Dr. Angus Smith, and the ways in which they had severally contributed to the improvements of the analytical processes regarding water. The determinations usually made in the partial analysis of a water are—*a*. The total solid constituents. *b*. The organic and other volatile matters. *c*. The oxygen required to oxidise organic matter. *d*. The nitrous and nitric acids. *e*. The ammonia.

These processes were considered *seriatim*.

(*a*.) In the method usually adopted for the determination of the total solid constituents—the evaporation of the water with addition of sodic carbonate and drying at 120° to 130° C.—there are two prominent sources of error; firstly, the salts of ammonia are converted into carbonate, which volatilises during the operation; secondly, urea when present is decomposed, and some of the products are volatilised. In an experiment made to test this point

44 per cent of urea was lost. These defects are lessened by not adding the alkaline carbonate, and by drying at 100° instead of at 120°—130°. A residue dried at 100° sometimes retains the elements of water, but these are chemically combined, and the amount obtained can therefore be fairly considered as representing only the solid constituents. With the exception of water containing much calcic and magnesic chlorides and sulphates, the difference made by drying at the one temperature or the other is not great. A water analysed (Thames water) gave as the total amount of solid matter in 100,000 parts, 27·02 parts when dried at 100° C.; and 26·54 parts at 120°—130°C.

Dr. Frankland did not, however, consider the information afforded by this determination as great. In estimating the loss upon ignition, suffered by this residue, it must be previously heated to the higher temperature.

(*b.*) In the determination of the volatile matter by the loss upon ignition, the magnesic and calcic carbonates are causticised, and have then to be recarbonated. In this operation all the organic matter cannot be expelled, —notably the case with water containing urea. Experiments were made with water containing known weights of urea and sodic carbonate. In three experiments only 14·6, 28·2, and 42·1 per cent respectively became expelled; 85·4, 71·8, and 57·9 per cent remaining in the residue in these cases. Dr. Frankland suggested the possibilty of some of the elements of the urea being fixed in the condition of cyanate and cyanurate. A remarkable error is introduced in the case of some waters in recarbonating the alkaline earths, even with a pure solution of carbonic anhydride, the apparent amount of earthy carbonates being greatly in excess of the real amount. In such a case of course the process cannot be used.

(*c.*) Estimating the amount of oxygen required to oxidise organic matter. Potassic permanganate has been commonly used in making this determination. A close examination of the process, however, has led to its indications being found unreliable. In experiments made upon nine kinds of organic matter, only one, oxalic acid, was completely oxidised in six hours. In the case of urea, hippuric acid, and creatin, the oxygen, abstracted from the permanganate, only represented $\frac{1}{50}$th of that required by theory.

(*d.*) Estimation of the nitrous and nitric acids. In this determination, Pew's process has been much used. It turns upon the conversion of stannous chloride into stannic chloride in the presence of nitric acid. Messrs. Chapman and Schenk have pointed out that this change is effected by many organic substances.

The lecturer recorded experiments upon this subject, which showed that the indications obtained in treating starch and sugar by this process were incorrect. 1 grm. starch digested for 20 minutes in a sealed tube with 3 c.c. of a solution of stannous chloride produced an oxidation equivalent to ·00375 grm. nitric anhydride; ·1 grm. sugar at 150° C. produced an oxidation equivalent to ·007 grm. nitric anhydride.

(*e.*) Estimation of the ammonia. This is usually effected by distilling with baryta water or sodic carbonate, and determining the ammonia either by neutralisation or by Nessler's test. It is liable to give inaccurate results in the case of waters recently contaminated with sewage, owing to the gradual decomposition of urea. Dr. Frankland in describing the processes and modifications he proposed to substitute, divided the water analysis into four, viz. the following determinations:

1. The total solid constituents.
2. The organic carbon and nitrogen.
3. The nitrogen in the form of nitrates and nitrites.
4. The ammonia.

(1.) *Estimation of the total solid constituents.*.—For this purpose ½ litre of water is evaporated as rapidly as possible, and the residue dried at 100° C.

(2.) *Estimation of the organic carbon and nitrogen.*—The lecturer had no process to offer for the direct determination of the organic matter, but he was able to estimate the carbon and nitrogen, which were its most important elements, with accuracy. It is necessary in the first place to expel the carbonic anhydride. Sulphuric acid has been found to effect this easily, and for many reasons it has been found the most convenient acid. The solution is boiled for a couple of minutes with a small quantity of sulphuric acid and then evaporated; for this purpose hemispherical glass dishes have been found far more convenient than platinum. The evaporation is conducted in vacuo, and the residue dried at a steam heat. The heat is applied at the *top* of the bell, by means of a current of hot air; applied in this way the water never boils. Five samples of water could be evaporated at the same time in the apparatus shown to the Society.

The residue is mixed with plumbic chromate and transferred to a combustion tube, the dish being rinsed with the chromate; a layer of pure cupric oxide is also added. The tube is sealed at one end and drawn out at the other to about the same size as the tube of the Sprengel's pump which it has to join. The anterior portion of the tube (the position of the layer of pure cupric oxide) is heated, and the tube then exhausted for 5 or 10 minutes. The combustion is now made, and the tube again exhausted, and the resulting gases collected over mercury.

A gaseous mixture is obtained, containing free oxygen. After absorbing the oxygen by pyrogallic acid, the volume of the gaseous mixture is accurately measured. The whole of the carbon is obtained in the form of carbonic acid, the nitrogen partly as such, with nitric acid and nitric oxide. The amount of nitrogen found is made up of the nitrogen of the ammonia and the organic nitrogen; the former must therefore be subtracted.

Cupric oxide made from nitrate is not admissible. It is best obtained by oxidising sheets of copper. It may be obtained tolerably pure from blue-vitriol makers. In experiments made upon solutions of known weights of sugar treated by the whole process these results were obtained:—

	Found.	Calculated.
Carbon	·01306	·01460
	·00440	·00480
	·00530	·00510

A solution of a known weight of sugar and chloride of ammonium gave—

	Found.	Calculated.
Carbon	·00434	·00484
Nitrogen	·00254	·00246

Dr. Frankland had examined Messrs. Wanklyn, Chapman, and Smith's method of estimating the albuminoid nitrogen in water. Numerous experiments were made side by side with the combustion process now brought forward. These are some of the results obtained in samples of water artificially made, representing water of average quality:—

Permanganate Method.	Combustion.
·006	·010
·002	·010
·016	·068
·016	·042
·022	·076

Where the quantity was small they agreed better.

·001	·001
·004	·009
·003	·004
·012	·012

The permanganate method in several cases gave nitrogen where the combustion process showed none.

(3.) Estimating the nitrogen in the form of nitrates and nitrites, Dr. Frankland has found a process devised many years ago by Mr. Walter Crum, to give very good results.

A concentrated solution of the nitrate or nitrite is mixed with rather more than an equal volume of sulphuric acid and agitated with mercury in as finely divided a state as pos-

sible. It is convenient in this operation to use the residue obtained in the determination of the total solid constituents. Chlorides must not be present. To remove chlorine, argentic sulphate is added to the residue dissolved in 15 or 20 c.c. of water, the solution filtered, and evaporated to a small bulk. It is then transferred to a vessel standing over mercury. The vessel may be described as a narrow eprouvette drawn out at the top into a narrow tube with a stopcock, carrying above a cup-shaped piece. The last portions of the fluid are washed in with the acid itself. The tube has been filled up to the tap completely with mercury, and care is necessary in allowing the descent of the fluid from the cup to the lower vessel to allow no air to enter the latter. The tap being carefully closed, the thumb is slipped under the end of the tube, which is then withdrawn and shaken. In this operation a short column of mercury must always remain between the thumb and the solutions. A strong pressure is produced, and the tube is occasionally returned to the trough, and the egress of some of the mercury permitted. The pressure is due to the formation of gaseous oxides of nitrogen. The nitrogen is determined in the resulting gas. The reduction of the nitrates and nitrites by this means was shown to the Society, the process described being performed experimentally, and a considerable quantity of gas was obtained.

This process has been tried with known quantities of nitre, also with uric and hippuric acids, and found to give satisfactory results.

(4.) Determination of the ammonia. Dr. Frankland considered it advisable to decolorise the water before using Nessler's colour test, using for this purpose calcic chloride, sodic carbonate, and a few drops of potassic hydrate. The distillate from this gave accurate indications.

The President, in returning the customary vote of thanks, took occasion to inquire whether the reduction of the nitrates was carried to nitrogen.

The subject being evidently a fertile one for discussion, the speakers were limited to a few minutes each.

Mr. Wanklyn wished to know whether the comparative experiments with the process devised by himself and colleagues were made with natural or artificial waters. He maintained that their process gave constant results with from 1 to 6 parts of albumen in 100,000 of water.

Professor Abel had thought the process alluded to by the last speaker might have been serviceable to him, and had instituted experiments to check their results. He had been totally unable to obtain concordant results.

Mr. Dugald Campbell's experience with the process was similar to Professor Abel's.

Mr. Chapman attributed the different results obtained by Dr. Frankland by the combustion process and their process, to sources of error in the former.

Mr. Thorpe, calculating the nitrogen as albumen, obtained results agreeing with other determinations, and found the process valuable as a method of controlling his results.

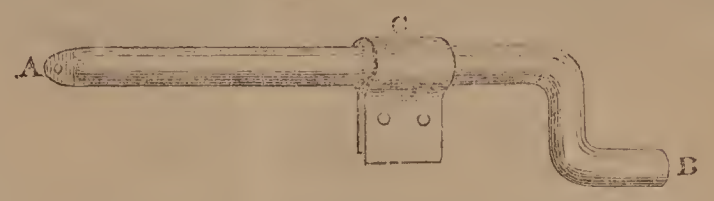
Dr. Vœlcker remarked that M. Nessler had told him of a simple method of separating the ammonia when present in moderately large quantity. The Nessler test, shaken in a bottle with the water, gave a precipitate which contained all the ammonia. By separating this by deposition, and treating it with sulphide of potassium, and distilling, all the ammonia will be separated, and may be collected in a solution of standard acid.

Mr. Smee and Mr. Hawksley, the engineer, also took part in the discussion.

The lecturer replied to the many remarks that had been made. The question put by the President was one of great interest, but he was unable to say definitely whether the reduction was carried to nitrogen. He should act upon the point suggested by Dr. Vœlcker. At a late hour the Society adjourned. Mr. Wanklyn wished to continue the discussion, and he therefore possesses the right to speak at the commencement of the next meeting.

NOTES ON LECTURE EXPERIMENTS.

Preparing Coils of Wire.—I have found the following simple apparatus extremely serviceable in preparing coils of steel wire for combustion in oxygen, and for coiling wires for battery connections. The apparatus was devised by Mr. Waite, of this town:



A B is a rod of iron of the diameter it is desired the coils shall be made. The extremity A is flattened, and a hole drilled through, while the other end is bent twice at right angles to form a winch. C is a piece of sheet iron bent so as to fit loosely round the bar, and when the lower part is fastened in a vice, serves as a support for the rod A. B. To prepare a coil of wire, the lower part of the support C is clamped in a vice; a long piece of this wire is then threaded through the hole at the extremity A, and the two ends held while an assistant turns the winch. The wire when twisted is held against the bar with the left hand, while the winch is turned with the right, and the wire is thus wound on the rod. On cutting the wire at A, the coil may be removed, and is ready for use.

C. J. Woodward.

Midland Institute, Birmingham, January 6th, 1868.

CORRESPONDENCE.

ANTISEPTICS.

To the Editor of the Chemical News.

Sir,—I have for some time been a reader of your journal, and frequently find therein articles which interest me as a manufacturer. There is one subject upon which I am anxious to obtain practical information in which I am directly interested, and which is seldom spoken of in works relating to organic chemistry, at least in any works which it has been my fortune to consult; I allude to the subject of antiseptics. Can you point, for instance, to any rule, by an acknowledged authority, which defines the quantity of any chloride, say of mercury, zinc, &c., which, when in solution, will, with certainty, protect from mould or other change, whether produced by a change of climate, a moist atmosphere, warmth or cold, another given quantity of material which contains a large proportion of nitrogenous matter, such as mucilage, &c.; and is there any other antiseptic more active or certain in its preservative properties than the substance named, which is equally economical, and which can be used advantageously in an extensive business where considerable quantities would be required? If you can afford me practical information on this subject, or direct me where to find it, you will confer a favour, and much oblige,

A Constant Reader.

Philadelphia, Penna., U.S., Dec. 28, 1867.

[Carbolic acid or (if the odour be an objection) oil of cloves will produce the desired effect. About one part in 1,000 will, in ordinary cases, be sufficient.—Ed. *C.N.*]

HYDROUS, NOT HYDRATED.

To the Editor of the Chemical News.

Sir,—Most writers on modern chemistry regard hydrates as altogether distinct from what they call hydrated substances, yet retain the respective names, to the great risk of confusion; a hydrate is stated to be a body containing hydroxyl (HO), a hydrated substance one containing

water (H_2O). Why not let the word "hydrated" follow its parent, and relate to hydrates only? Salts without water are appropriately spoken of as anhydrous, salts with water may surely be termed hydrous. I should, for instance, consider the body having the formula Mg_2HO as a hydrate, that having the formula $MgCO_3$, $3H_2O$ as a hydrous carbonate, not a hydrated carbonate.—I am, &c.,

JOHN ATTFIELD.

DETECTION OF OZONE.

To the Editor of the Chemical News.

SIR,—During the past year I have on several occasions exposed moistened silver-leaf to the atmosphere, with a view to determine how far it might be used as a trustworthy test for ascertaining the presence or absence of ozone.

Whenever the ordinary test (potassic iodide and starch) indicated that ozone was present, some part of the silver-leaf was oxidised, and the greater the amount of ozone, the quicker was the oxidation of the silver effected.

The silver-leaf was exposed freely to the atmosphere, and was kept moistened by causing distilled water to pass over it, the water being conducted from an adjacent vessel by two or three threads of common darning cotton.

By observing the time which elapses before the silver-leaf is oxidised, an idea may be formed of the relative amount of ozone present on any given day.—I am, &c.,

R. C. C. LIPPINCOTT.

Bournemouth, Jan. 13.

MISCELLANEOUS.

New Scientific Club.—We have received information of the establishment of a scientific club in Dublin. This club is not intended as a channel for the promulgation of original matter, but is simply designed to give to the members that early information of advances made by others in science, which in London springs from the unorganised conversation of the learned societies. It is proposed to deal with one group of sciences—the group of physical sciences, including physics, chemistry, and the applications of mathematics to physics as in astronomy, mechanics, electricity, &c.; but excluding pure mathematics on the one hand, and physiology, geology, and the other natural sciences on the other. The number of ordinary members is limited to twenty-five, but visitors are to be freely admitted by members' introduction. The annual payment for an ordinary member is £1. The scientific men in our provincial towns may take a hint from this Dublin Scientific Club. It will be remembered that a meeting in Glasgow was recently convened to forward the formation of a similar institution there.

Amber.—Recently the local correspondent of a Geelong journal announced the discovery of a supposed amber mine at Rokewood, The Age pooh-poohed the matter, and suggested that the substance was simply a description of gum found in lignite deposits. The author of the statement, as to the substance being amber, has replied, and says:—"I, for one, will confess that I never heard of this sort of gum referred to, except close to the surface of the roots of trees, and certainly not at a depth of 70 feet; but for the benefit of the curious I will give you a *verbatim* copy of the written opinion of a professional mineralogist, who resides at Ballarat, and whose ability and experience in such matters is, I am told, well known there and elsewhere. 'The resinous substance left with me for examination is undoubtedly amber, and has not previously, to my knowledge, been found in this colony; making therefore another addition to our colonial minerals. The colour of the said substance is brown, streaked yellowish white, transparent, conchoidal fracture, lustre waxy. Specific gravity 1·1. Acquires resinous electricity by friction; contains empyreumatic oil and succinic acid, and corresponds in all other respects with the brown amber of Europe. (Signed) A. T. Abel.' I have also been shown the substance obtained from the mine by Messrs. M'Keeman, draper, Ballarat, and Blair, miner, Break o' Day, either of which gentlemen I have no doubt would be most happy to show it to any gentleman interested in the development of our mineral resources. Ths mine, at which men are now employed, is situated at Grassy Gully, about eight miles from Rokewood, in the direction of the Mount Misery Ranges."—*Ballarat Evening Post, July* 4, 1867.

Petroleum for Steamship Boilers in the United States Navy.—After careful and long-continued trials, the Secretary of the United States Navy finally reports against the employment of petroleum as a fuel in steamships. He says:—"The act approved April 17, 1866, appropriated five thousand dollars for testing the use of petroleum as a fuel for marine boilers. An elaborate series of experiments has been made at the New York and Boston Navy Yards. The conclusion arrived at is that convenience, comfort, health, and safety are against the use of petroleum in steam vessels, and that the only advantage thus far shown is a not very important reduction in bulk and weight of fuel carried."

Lead Floating on Molten Iron.—Some experiments have been made in Germany which seem to show that molten lead when dropped upon liquid iron remains floating on the surface of the latter. As the specific gravity of lead (11·5) is more than one-half greater than that of cast-iron (7), there arose some discussion on this subject, which has been recently closed in a very satisfactory manner by the researches of Professor Karmarsch, of Hanover. An iron-master in the vicinity of that town had sent to the Professor some samples of such drops of lead lying imbedded in the surface of a cast-iron block, and which had been produced in the manner above described. Professor Karmarsch found, upon close examination, that these drops of lead, instead of being solid globules, as was supposed at first sight, were all found to be hollow, forming bubbles composed of metallic skin, and apparently empty in the centre, so far as his observation was carried. He explains the whole by supposing that the molten lead, at the temperature to which it is raised by the contact with the liquid iron, forms an incipient vapour of lead, which is prevented from escaping by the skin of solidifying metal which forms on the top. The lead vapour, according to this explanation, keeps the lead resting upon the surface of the iron. It seems that in large quantities the result is different, since it is known that lead is occasionally tapped from the bottom of the blast furnaces which smelt certain classes of ores containing lead, and in these cases the lead is found below the liquid iron according to its greater specific gravity.

Tyrian Purple.—The Tyrians were probably the only people of antiquity who made dyeing their chief occupation, and the staple of their commerce. The opulence of Tyre seems to have proceeded, in a great measure, from the sale of its rich and durable purple. It is unanimously asserted by all writers that a Tyrian was the inventor of the purple dye, about 1,500 years B.C., and that the king of Phœnicia was so captivated with the colour, that he made purple one of his principal ornaments, and that, for many centuries after, Tyrian purple became a badge of royalty. So highly prized was this colour, that in the time of Augustus, a pound of wool dyed with it, cost at Rome a sum nearly equal to thirty pounds sterling. The Tyrian purple is now generally believed to have been derived from two different kinds of shell-fish, described by Pliny under the names *purpura* and *buccinum*, and was extracted from a small vessel or sac in their throats to the amount of one drop from

each animal; but an inferior substance was obtained by crushing the whole substance of the *buccinum*. At first it is a colourless liquid, but by exposure to air and light it assumes successively a citron yellow, green, azure, red, and, in the course of forty-eight hours, a brilliant purple hue. If the liquid be evaporated to dryness soon after being collected, the residue does not become tinged in this manner. These circumstances correspond with the minute description of the manner of catching the purple dye fish given in the work of an eye-witness, Eudocia Macrembolitissa, daughter of the Emperor Constantine the Eighth, who lived in the eleventh century. The colour is remarkable for its durability. Plutarch observes, in his life of Alexander, that, at the taking of Susa, the Greeks found, in the Royal treasury of Darius, a quantity of purple cloth, of the value of five thousand talents, which still retained its beauty, though it had lain there one hundred and ninety years. This colour resists the action even of alkalies, and most acids. Pliny states that the Tyrians gave the first ground of their purple dye by the unprepared liquor of the *purpura*, and then improved or heightened it by the liquor of the *buccinum*. In this manner they prepared their double-dyed purple—*purpura dibapha*—which was so called, either because it was immersed in two different liquors, or because it was first dyed in the wool and then in the yarn.—*Prof. Dussauce.*

CONTEMPORARY SCIENTIFIC PRESS.

Under this heading it is intended to give the titles of all the chemical papers which are published in the principal scientific periodicals of the Continent. Articles which are merely reprints or abstracts of papers already noticed will be omitted. Abstracts of the more important papers here announced will appear in future numbers of the "Chemical News.")

Annalen der Chemie und Pharmacie.
September, 1867.

O. Loew, "On Sulphonaphthalic Acid." G. Wischin, "On Phenylenediethylacetone and Ethylenediethylacetone." F. Gauhe, "On Sulphurous Cyanide and other Decomposition Products of Sulphurous Chloride." O. Rembold, "On Quino-tannic and Quinovatannic Acids." A. Grabowski, "On Rhatania-tannic Acid." G. Malin, "On Filico-tannic Acid." A. Grabowski, "On Filicic Acid." O. Rembold, "On the Tannic Acid of the Root Bark of the Pomegranate Tree." H. Hlasiwetz, "On the Relations between the Tannic Acids, Glucosides, Phlobaphenes and Resins." L. Carius, "On Chlorous Anhydride and Benzol." C. Glaser, "Researches on some new Derivatives of Cinnamic Acid." E. Linnemann, "On the Transformation of the Bromides of the Hydrocarbons belonging to the the Series CnH2n into Acetones of the Fatty Acids containing the same Quantity of Carbon." W. Heintz, "On Phosphate of Zinc and Phosphate of Zinc and Ammonia." C. O. Cech, "On a new Method of forming Viridic Acid." F. Beilstein, "On the Behaviour of Toluol towards Bromine." A. Gescher, "On Sulphide of Copper and Ammonium."

Bulletin de la Société Chimique de Paris.
September 1867.

E. Jungfleisch, "On some Mutual Relations between the Melting Points, Boiling Points, Densities, and Specific Volumes of some Chlorinated Derivativss of Benzene." C. Friedel and A. Ladenburg, "On a Bromide of Propylene derived from Acetone." D. Gernez, "Resumé of the Author's Investigations on Supersaturated Solutions." C. Marignac, "On an Analysis of Æschynite." "On the Separation of Niobic from Titanic Acid."

Mémoires de la Société des Ingenieurs Civils de Paris.
January—March, 1867.

Limet, "Analysis of a Specimen of Boiler scale." Simonin, "On the Coal Fields and Mineral Veins of the Old and New World." E. Flachat, "On the same subject." E. Flachat, "On the Speciemn of Boiler Scale exhibited by Limet." Farcot, Limet, Tresca, on the same subject. Ribail, "On the Use of Caustic Soda for preventing the Formation of Boiler Scale in Locomotives." Foucou "Note on the Deposits of and Modes of obtaining Petroleum in North America, together with some Account of the Theories which have been proposed to account for its Origin." W. Clarke, "On the Modes of obtaining Petroleum in America." A. Morin, "Report on Technical Education." Delonchant, "On the Use of Plates of Mica for indicating the various Planes in Models for teaching Descriptive Geometry." Arson, "Analysis of a Deposit from a Feed-water Heater." Flachat, "Note on some Spherical Concretions found in the Boilers of the Transatlantic Steamer "Ville de Paris."

Mittheilungen des Gewerbe-Vereins für Hannover.
No. 3. 1867.

Buresch, "On the English Alkali Act, 1863." G. E. Landsberg, "Some Experiments on the Comparative Calorific Power of Piesberg and Ibbenburen Coal." Heeren, on the same subject. Begemann, "On the Official Analysis of Milk." C. Sostmann, "On the Use of Paraffin for checking the violent Ebullition of Syrup in Evaporating and Vacuum Pans." "On the Manufacture of Albumen." R. Niccol, "On the Presence of Microscopic Insects in Raw Sugar." Cameron, Hassal, on the same subject. L. Elsner, "On the Sublimation of certain Bodies at a White Heat."

No. 4.

Ruhlmann, "Improved Processes for the Manufacture of Oatmeal and of Oil." P. Legrand, "An improved Iron Barrel for holding Spirits." H. Violette, "On the Preparation of Copal and other Resins for the Manufacture of Varnish." L. Rinman, "On the Presence of Nitrogen in Steel and Pig Iron, and on the Condition of Carbon in Hard and Soft Steel."

NOTES AND QUERIES.

Determination of free Sulphuric Acid.—I want to determine the free sulphuric acid in superphosphates, and do not quite know how to do it. Is it possible to do it by shaking the solution with oxide of lead and to determine afterwards the sulphate of lead formed? Can any of your correspondents tell me if this plan would do, or can they tell me of another way.—V. C.

To Prevent Water Freezing.—Can any of your readers oblige by answering the following questions? 1. What proportion of salt must be added to water to prevent it freezing, when exposed to the coldest weather known in this country? 2. What percentage of alcohol should water contain for the same purpose? 3. Is there any other cheap substance which would effect the same object, and which would not attack iron?—Volta.

Bleaching Calico.—Your correspondent G. L. desires to receive information relating to "Bleaching Powder Solutions." It is not absolutely necessary to throw away the solutions after you have passed a certain quantity of fabrics through them, unless in the case of very fine goods, or where there are any coloured ornaments in the cloth to preserve; the solution, if it has been kept too long, is apt to injure the fabric or decolorise the ornaments. The usual method for testing solutions of bleaching powder is by means of the sulphate of indigo test. The hydrometer being a very fallacious test, I would recommend your correspondent to make himself acquainted with the chlorimeter test introduced by the late Mr. Walter Crum, and which is so easily manipulated that a workman of ordinary intelligence can easily understand it. The chlorimeter test can, if I mistake not, be procured from Griffin, who I have no doubt will give every information regarding it.—A. G. S.

Detection of Magnesia in the presence of Manganese.—When a compound is perfectly free from magnesia, but contains manganese, together with phosphates insoluble in water, on adding ammonia and hydric disodic phosphate (Na_2HPO_4) a precipitate is almost invariably obtained; this precipitate has very much the appearance of triple phosphate, and might be readily mistaken for it; but analysis shows that it does not contain any magnesia. Its formation is due to MnO being carried down with Fe_2O_3 on boiling with potash; by boiling with chloride of ammonium the MnO is dissolved out; now on treating with Na_2HPO_4 and ammonia, the precipitate in question is obtained, which adheres to the sides of the vessel where it has been rubbed by the rod, in exactly the same manner as the magnesia precipitate. This reaction does not take place if the precipitate containing MnO be exposed to the air for some time, for then it absorbs oxygen, and forms MnO_2, which is insoluble in chloride of ammonium. Mg may be readily detected in the presence of Mn by dissolving the precipitate in HCl, neutralising with ammonia, and precipitating the Mn by means of ammonie sulphide, and again adding Na_2HPO_4 and ammonia, when the Mg will be precipitated, and this may be know to be free from Mn by its not colouring a borax bead.—A. Liversidge.

TO CORRESPONDENTS.

*** Owing to pressure on our space the "Answers to Correspondents" will be given in our next.

Communications have been received from F. C. Calvert & Co. (with enclosure); W. Sugg; E. G. Tosh (with enclosure); Magnesium Metal Co.; E. Cetti; T. H. Rowney (with enclosure); Dr. R. Oxland; J. Heywood; Dr. A Wuth (with enclosure); J. Slessor (with enclosure); R. Talling; R. Rumney (with enclosure); E. D. Day; W. Wilkinson (with enclosure); J. H. Kiel; W. Smith; W. Bailey and Son (with enclosure); J. Hill (with enclosure); E. W. Bartlett; J. Scrivener; H. Hankey (with enclosure); Professor Heaton; J. How; W. Gordon; E. M. Delf; J. Sprague (with enclosure); E. Goodchild (with enclosure); J. Emerson Reynolds (with enclosures); J. Browning (with enclosure); D. Forbes, F.R.S. (with enclosure); Rev. B. W. Gibsone (with enclosure); W. Wood (with enclosure); Archd. Liversedge (with enclosure); C. J. Woodward (with enclosure); Dr. Day; Dr. H. Debus; Dr. Attfield (with enclosure); Dr. Frankland, F.R.S. (with enclosure).

Books Received.—"Inorganic Chemistry," by Charles W. Eliot and Frank H. Storer. Second edition, revised. London: John Van Voorst; "Le Moniteur Scientifique"; "Journal of Materia Medica" "American Gas Light Journal"; "Scientific American"; "Etude Descriptive, Théorique et Experimentale sur les Météorites." Par M. Stanislas Meunier. Paris: Aux Bureaux du Cosmos, 7, Rue Perronet Près la rue des Saints-pères; "A Treatise on Frictional Electricity in Theory and Practice," by Sir William Snow Harris, F.R.S. Edited by Charles Tomlinson, F.R.S. London: Virtue & Co., 1867.

MEETINGS FOR THE WEEK.

MONDAY.—Royal Geographical, 8½.
WEDNESDAY.—Society of Arts, 8.
THURSDAY.—Royal, 8½.
—— Philosophical Club, 6.
—— Royal Institution, 3. Professor Tyndall, "On the Discoveries of Faraday."
FRIDAY.—Royal Institution, 8. Rev. F. W. Farrar, "On Public School Education.
SATURDAY.—Royal Institution, 3. Professor Roscoe, "On the Non-Metallic Elements.

PATENTS.

Communicated by Mr. VAUGHAN, F.C.S., Patent Agent, 54, Chancery Lane, W.C.

GRANTS OF PROVISIONAL PROTECTION FOR SIX MONTHS.

3359. E. Belknap, Mortimer Street, Cavendish Square, Middlesex, "Improvements in the treatment of the solution of malt for brewing."—Petition recorded November 27, 1867.

3384. J. Baylis, Durdham Down, Bristol, "An improved chemical preparation or compound to be used in preparing mixed textile fabrics for dyeing or colouring."—November 28, 1867.

3388. T. Rose, Oxton, Cheshire, and R. E. Gibson, New Brighton, Cheshire, "An improved mode of treating cotton seed to obtain oil therefrom, and in machinery employed therein."

3389. C. Albisser, Mincing Lane, London, "Improvements in the preparation of sulphate of magnesia, applicable to the treatment of the crude potash, salts of Stassfurt, and the refuse from the manufacture of chloride of potassium."—A communication from J. Vorster, and H. Grüneberg, Cologne, Prussia.—November 29, 1867.

3405. W. R. Lake, Southampton Buildings, Chancery Lane, "An improved mode of and means for clarifying saccharine solutions."—A communication from J. E. Freund, New York, U.S.A.,—November 30, 1867.

3440. J. Gjers, Middlesbrough, Yorkshire, "Certain improvements in the manufacture of cast steel and homogeneous iron."—December 3, 1867.

3463. S. Perkins, and W. Smellie, Gorton, near Manchester, "Improvements in the manufacture of malleable metal of a steely quality, partly from Bessemer 'scrap' or other Bessemer metal."

3469. P. G. L. G. Designolle, Rue de la Seine, and J. Casthelaz, Rue Ste. Croix de la Bretonnerie, France, "Improvements in the manufacture of explosive and fulminating powders."—December 5, 1867.

3473. J. Durrans, Thurlstone, near Penistone, Yorkshire, "An improved material or composition to be employed for covering or coating the interior surfaces of moulds, crucibles, or ducts, previous to their receiving the molten metal in the process of casting, and for other purposes."—December 6, 1867.

3483. R. B. Jones, Nelson Terrace, City Road, Middlesex, and W. Powell, Circus Place, Finsbury, Middlesex, "Improvements for the prevention of incrustation in steam boilers."—December 7, 1867.

3499. L. Rose, Leith, Scotland, "An improved mode of preserving vegetable juices."

3502. C. Martin, Chancery Lane, W. Barrett, and T. S. Webb, Norton, Durham, "Improvements in the treatment and reduction of titaniferous iron ores, and in the manufacture of iron, and in the construction of furnaces to be employed therein."—December 9, 1867.

3517. A. M. Clark, Chancery Lane, "An improved process for the reduction of tin, so as to render it applicable for coating metals and for other puposes."—A communication from E. Cornélis, Boulevart St. Martin, Paris.

CANDLES.—If you do not want your candles exclusively for show, but with pleasantness of appearance require excellence of burning, buy "PRICE'S GOLD MEDAL PALMITINE," or their "SHERWOOD PALMITINE," or their good old-fashioned "BELMONT SPERM," or "BELMONT WAX," or their "BEST," No. 2," "No. 3," or "BATTERSEA" COMPOSITE," in preference to the finest, and most transparent Paraffine candles. But if you must have the extreme transparency of pure Paraffine, "PRICE'S PARAFFINE" or their "BELMONTINE" will give it to you in perfection, and at a more moderate price than is usually charged for any other really first-class Paraffine Candles.

The new toilet soap, "PRICE'S SOLIDIFIED GLYCERINE," containing half its weight of their concentrated distilled Glycerine, should be in general use in every house before the winter comes on, because of its admirable effects in preventing chapping of the hands and face. In every house there ought also to be one of the sealed bottles of their concentrated Distilled Glycerine, known everywhere as "PRICE'S GLYCERINE," and prescribed by the most eminent medical men abroad as well as at home, as the one only Glycerine for medicinal use whether externally or internally.

PRICE'S FANCY SOAPS of the different sorts usually made are excellent, and command a constantly increasing sale. The "Solidified Glycerine" spoken of above is, however, the one fancy soap to use.

"PRICE'S NEW PATENT NIGHT LIGHTS," for burning in the wide glasses are believed to be the very best Night Lights made. "PRICE'S CHILDS' NIGHT LIGHTS," for burning without glasses, and their different sorts of "CHAMBER CANDLES" are so well-known, and so generally appreciated as not to need any special notice here.

PRACTICAL CHEMISTRY.
Laboratory, 60, Gower Street, Bedford Square, W.C.

Mr. Henry Matthews, F.C.S., is prepared to give Instruction in all branches of PRACTICAL CHEMISTRY, particularly in its Application to MEDICINE, AGRICULTURE, and COMMERCE.

The Laboratory is open daily, except Saturday, from Ten to Five o'clock; on Saturday from Ten till One o'elock; and from October to March on Monday and Friday Evenings from Six to Nine o'clock.

Mr. Matthews is also prepared to undertake ANALYSES of every description.

For Particulars and Prospectuses apply to Mr. Henry Matthews, at the Laboratory, 60, Gower-street, Bedford Square, W.C.

Berners College of Chemistry.—Experimental MILITARY and NAVAL SCIENCES, under the direction of Professor E. V. GARDNER, F.E.S., &c.; of the late Royal Polytechnic Institution and the Royal Naval College.

The Laboratory and Class Rooms are open daily.

Especial facilities for persons preparing for Government and other examinations.

Private Pupils will find every convenience.

Analyses, Assays, and Practical Investigations connected with Patents, &c., conducted.

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Instruction in Practical Chemistry and Evening Classes for the Study of Chemistry, Botany, Materia Medica, &c.

TO PHARMACEUTICAL AND OTHER STUDENTS.

Mr. J. C. BRAITHWAITE, for thirteen years Principal Instructor in the Laboratories of the Pharmaceutical Society of Great Britain, and Demonstrator of Practical Pharmacy, Pharmaceutical Latin, &c., wishes to inform his old Pupils and others that he continues to devote his whole attention to Education. The Session 1867—1868 will commence on the 1st of October, when

Mr. BRAITHWAITE'S Laboratory, which has been enlarged during the recess, will be re-opened at 10 a.m. for Instruction in Practical Chemistry as applied to Pharmacy, Medicine, Analysis, &c. Pupils can enter at any period. Terms moderate.

THE CHEMICAL and TOXICOLOGICAL CLASS meets as usual every Monday and Thursday Evening, at 8 p.m., commencing October 3rd.

The LATIN CLASS for the reading of the Pharmacopœia, Physicians' Prescriptions, &c., every Tuesday and Friday Evening, at 8 p.m., commencing October 1st.

The BOTANICAL and MATERIA MEDICA CLASS, every Wednesday and Saturday Evening, at 8 p.m. The usual EXCURSIONS for the STUDY of PRACTICAL BOTANY will commence at 10 a.m., October 2nd.

Fee to either of the above Classes Half-a-Guinea per Month; to the Botanical Excursions only. Half-a-Guinea per Eight Lessons, payable in advance. Pupils can enter at any period.

Address, 54, Kentish Town Road, N.W.
Mr. Braithwaite receives a few Pupils to board in his house.

PARIS EXHIBITION TWO GOLD MEDALS.

Liebig's Company's Extract of Meat, as distinguished from "Liebig's Extract of Meat," which name is daily more used for all sorts of extracts. Warranted genuine and of perfect flavour by Baron Liebig, whose signature is on every genuine jar. Cheapest and purest stock for Soups, Entrees and Sauces, highly strengthening for Children and Invalids. 1 lb. 14s, ½-lb. 7s 6d, ¼-lb. 4s, 2-oz. 2s, equivalent to 1d. half-a-pint of best beef-tea. Retail, of Fortnum and Mason, all Italian Warehousemen, Chemists and Grocers. Wholesale, of Crosse and Blackwell; E. Lazenby and Sons; John Burgess and Son; Barron, Harveys, and Co.; Barclay and Sons; Burgoyne, Burbidges, and Squire; Wm. Edwards; M. E. Foster; Langtons, Scott, and Edden; S. Maw and Son; G. S. Pedler; T. and H. Smith and Co., London; Duncan, Flockhart, and Co.; John Mackay; H. C. Baildon, Edinburgh; Southall, Son, and Dymond, Birmingham; Wm. Smeeton, Leeds; Raimes and Co., and B. Westworth, Liverpool; all wholesale houses, and of Liebig's Extract of Meat Company (Limited), 43; Mark Lane, E.C.

Methylated Spirits.—David Smith Kidd, Licensed Maker, Commercial Street, Shoreditch, N.E. Also, FINISH, FUSEL OIL, and RECT. NAPHTHA.

Scholl's Patent Platinum Gas Light Perfecter.—Extract from Report by Dr. Letheby:—

"The results have been very remarkable, for they show an average increase of 63 per cent on the illuminating power of the gas. I am of opinion, therefore, that the invention is of great practical value." Price One Shilling each for Fish-tail Burners. To be had retail of Gas-fitters and Ironmongers, and (wholesale only) of JOHN SCHOLL, 41 and 42, Berwick Street, Oxford Street, London, W. Terms on application. N.B.—A specimen sent free on receipt of 12 stamps.

THE CHEMICAL NEWS.

Vol. XVII. No. 426.

ON

HEAT AND COLD;

A COURSE OF

SIX LECTURES*

(*Adapted to a Juvenile auditory*),

DELIVERED AT

THE ROYAL INSTITUTION OF GREAT BRITAIN,

(*CHRISTMAS*, 1867—8),

BY

JOHN TYNDALL, Esq., LL.D., F.R.S.

Lecture IV.

The Geysers of Iceland (continued).—The mechanical equivalent of heat.—Consumption of heat.

In our last lecture I intended, if time permitted, to explain the action of the geyser of Iceland, but at the end of the lecture I found that the time was insufficient for the purpose; and I promised then to explain this wonderful spring in the lecture of to-day; but when I came to look at the other matter before me I found that it was so abundant that I really could not get the subject of the geyser into it. In order to help myself, therefore, I printed 500 copies of an account of the geyser which I gave in this room 14 or 15 years ago; and I trust each young philosopher present has furnished himself with a copy of this description of the geyser, from which I have no doubt you will understand its philosophy—particularly by the help of your friends—when you read this paper at home, just as well as if I had tried to explain it to you here.

"The surface of Iceland slopes gradually from the coast towards the centre, where the general level is about 2,000 feet above the surface of the sea. On this, as a pedestal, are planted the Jökull or icy mountains of the region, which extend both ways in a north-easterly direction. Along this chain the active volcanoes of the island are encountered, and in the same general direction the thermal springs occur, thus suggesting a common origin for them and the volcanoes. From the ridges and chasms which diverge from the mountains mighty masses of steam are observed to issue at intervals, and where the escape takes place at the mouth of a cavern and the resonance of the cave lends its aid, the sound of the steam is like that of thunder. Lower down in the more porous strata we have smoking mud pools, where a repulsive blue-black aluminous paste is boiled, rising at times into huge bladders, which on bursting scatter their slimy spray to a height of fifteen or twenty feet. From the base of the hills upwards extend the glaciers, and on their shoulders are placed the immense snow-fields which crown the summits. From the arches and fissures of the glaciers, vast masses of water issue, falling at times in cascades over walls of ice, and spreading for miles and miles over the country before they find definite outlet. Extensive morasses are thus formed, which add to the monotony of the dismal landscape. Intercepted by the cracks and fissures of the land, a portion of these waters is conducted to the hot rocks underneath; here meeting with the volcanic gases which traverse these underground regions, both travel together, to issue at the first convenient opportunity either as an eruption of steam or as a boiling spring.

"In the Great Geyser we have a tube ten feet wide and seventy feet deep; it expands at its summit into a basin, which from north to south measures fifty-two feet across, and in the perpendicular direction sixty feet. The interior of the tube and basin is coated with a beautiful smooth plaster, so hard as to resist the blows of a hammer. The first question that presents itself is, how was this wonderful tube constructed? How was this perfect plaster laid on? A glance at the constitution of the Geyser water will perhaps furnish the first surmise. In 1,000 parts of the water the following constituents are found:—

Silica	0·5097
Carbonate of Soda	0·1939
Carbonate of Ammonia	0·0083
Sulphate of Soda	0·1070
Sulphate of Potash	0·0475
Sulphate of Magnesia	0·0042
Chloride of Sodium	0·2521
Sulphide of Sodium	0·0088
Carbonic Acid	0·0557

"The lining of the tube is silica, evidently derived from the water; and hence the conjecture may arise that the water deposited the substance against the sides of the tube and basin. But the water deposits no sediment, even when cooled down to the freezing point. It may be bottled up and kept for years as clear as crystal, and without the slightest precipitate. A specimen brought from Iceland and analysed in this Institution was found perfectly free from sediment. Further, an attempt to answer the question in this way would imply that we took it for granted that the shaft was made by some foreign agency, and that the spring merely lined it. A painting of the Geyser, the property of Sir Henry Holland—himself an eye-witness of these wonderful phenomena,—was exhibited. The painting, from a sketch taken on the spot, might be relied on. We find here that the basin rests upon the summit of a mound; this mound is about forty feet in height, and a glance at it is sufficient to show that it has been deposited by the Geyser. But in building the mound, *the spring must also have formed the tube which perforates the mound*: and thus we learn that the Geyser is the architect of its own tube. If we place a quantity of the Geyser water in an evaporating basin, the following takes place: in the centre the fluid deposits nothing, but at the edges where it is drawn up the sides of the basin by capillary attraction, and thus subjected to a quick evaporation, we find silica deposited; round the edge we find a ring of silica thus laid on, and not until the evaporation has continued for a considerable time, do we find the slightest turbidity in the central portions of the water. This experiment is the microscopic representant, if the term be permitted, of Nature's operations in Iceland. Imagine the case of a simple thermal spring whose waters trickle over its side down a gentle incline; the water thus exposed evaporates speedily, and silica is deposited. This deposit gradually elevates the side over which the water passes until finally the stream has to choose another course; here the ground becomes elevated by the deposit as before, and the stream has to move forward—thus it is compelled to travel round and round, discharging its silica and deepening the shaft in which it dwells, until finally, in the course of centuries, the simple spring has produced that wonderful apparatus which has so long puzzled and astonished both the traveller and the philosopher.

"Before an eruption, the water fills both the tube and basin, detonations are heard at intervals, and after the detonation a violent ebullition in the basin is observed; the column of water in the pipe appears to be lifted up, thus forming an eminence in the centre of the basin and

* Reported verbatim, by permission of the Author, for this journal.

causing the water to flow over its rim. The detonations are evidently due to the production of steam in the subterranean depths, which rising into the cooler water of the tube, becomes suddenly condensed and produces explosions. Between the interval of two eruptions, the temperature of the water in the tube gradually increases, but even immediately before an eruption, at no part of the tube is the water at its boiling temperature. How then is an eruption possible? Bunsen succeeded in determining the temperature of the water a few minutes before a great eruption; and his observations furnish the key of the entire enigma. A little below the centre he found the water within two degrees of its boiling point, that is, within two degrees of the point at which water boils under the pressure of the atmosphere, *plus the pressure of the superincumbent column of water*. The actual temperature at thirty feet above the bottom of the Geyser was 122° Centigrade, its boiling point being 124°. We have just alluded to the detonations and the lifting of the Geyser column by the entrance of steam from beneath. These detonations and the accompanying elevation of the column are, as before stated, heard and observed at various intervals before an eruption. Imagine, then, the section of water at thirty feet above the bottom to be raised six feet by the entrance of a mass of vapour below. The liquid spreads out in the basin, overflows its rim, and thus the elevated section has six feet less of water pressure upon it; its boiling-point under this diminished pressure is 121°; hence in its new position its actual temperature (122°) is a degree above the boiling-point. This excess is at once applied to the generation of steam; the column is lifted higher, and its pressure further lessened; more steam is developed underneath; and thus, after a few convulsive efforts, the upper part of the column of water, through the sudden boiling up from the middle downwards, is ejected with immense velocity, and we have the Geyser eruption in all its grandeur. By its contact with the atmosphere the water is cooled, falls back into the basin, sinks into the tube through which it gradually rises again, and finally fills the basin. The detonations are heard at intervals, and ebullitions observed; but not until the temperature of the water in the tube has once more *nearly* attained its boiling point is the lifting of the column able to produce an eruption.

"In the regularly formed tube the water nowhere quite attains the boiling-point. In the canals which feed the tube, the steam which causes the detonation and lifting of the column must therefore be formed. These canals are in fact nothing more than the irregular continuation of the tube itself. The tube is therefore the sole and sufficient cause of the eruptions. Its sufficiency was experimentally shown during the lecture. A tube of galvanised iron six feet long was surrounded by a basin; a fire was placed underneath and one near its centre to imitate the lateral heating of the Geyser tube. At intervals of five or six minutes throughout the lecture eruptions took place; the water was discharged into the atmosphere, fell back into the basin, filled the tube, became heated again, and was discharged as before.

"Sir Geo. Mackenzie, it is well known, was the first to introduce the idea of a subterranean cavern to account for the phenomena of the Geyser. His hypothesis met with general acceptance, and was even adopted undoubtingly by some of those who accompanied Bunsen to Iceland. It is unnecessary to introduce the solid objections which might be urged against this hypothesis, for the tube being proved sufficient, the hypothetical cavern disappears with the necessity which gave it birth.

"A moment's reflection will suggest to us that there must be a limit to the operations of the Geyser. When the tube has reached such an altitude that the water in the depths below, owing to the increased pressure, cannot attain its boiling-point, the eruptions of necessity cease. The spring, however, continues to deposit its silica, and forms a *laug* or cistern. Some of these in Iceland are of a depth of thirty or forty feet. Their beauty is indescribable: over the surface a light vapour curls, in the depths the water is of the purest azure, and tints with its own hue the fantastic incrustations on the cistern walls; while at the bottom is observed the mouth of the once mighty Geyser. There are in Iceland traces of vast, but now extinct, Geyser operations. Mounds are observed whose shafts are filled with rubbish, the water having forced a way underneath and retired to other scenes of action. We have in fact the Geyser in its youth, manhood, old age, and death, here presented to us:—in its youth, as a simple thermal spring; in its manhood, as the eruptive spring; in its old age, as the tranquil *laug*; while its death is recorded by the ruined shaft and mound which testify the fact of its once active existence.

"Next to the Great Geyser the Strokkur is the most famous eruptive spring of Iceland. The depth of its tube is forty-four feet. It is not, however, cylindrical like that of the Geyser, but funnel-shaped. At the mouth it is eight feet in diameter, but it diminishes gradually, until near the centre the diameter is only ten inches. By casting stones and peat into the tube and thus stopping it, eruptions can be forced which in point of height often exceed those of the Great Geyser. Its action was illustrated experimentally in the lecture, by stopping the galvanised iron tube before alluded to loosely with a cork. After some time the cork was forced up, and the pent-up heat converting itself suddenly into steam, the water was ejected to a considerable height; thus demonstrating that in this case the tube alone is the sufficient cause of the phenomenon."

Throughout the lectures that have been hitherto given I have had occasion to admire the attention and patience of my younger hearers. My hearers are of different ages, but although I have been obliged to mention certain things that could not possibly be understood by the very young boys, and to mention some elementary facts which were, perhaps, very well understood by the older boys, yet the young boys have been patient when I spoke to the elder ones, and the elder ones have been patient when I spoke to the younger boys; and for this I feel very thankful. With reference to the present lecture I have to address all the boys, especially the elder ones, for I have to explain a term or two very much used at the present time in connection with the subject of heat.

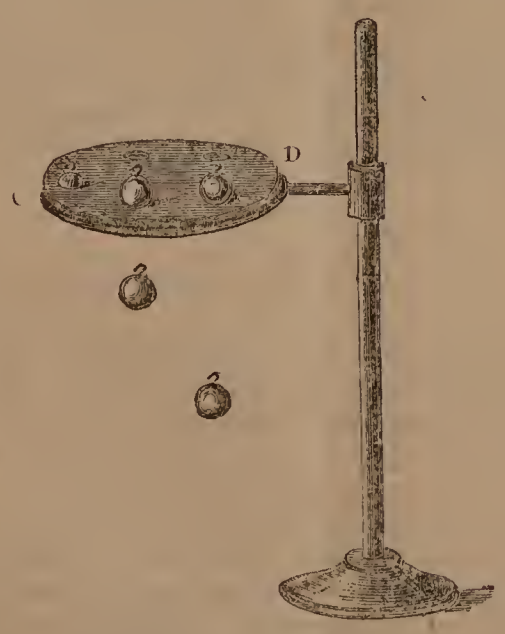
If you carry a pound of any substance whatever to a height of 772 feet above the earth's surface, and allow it to drop down upon the earth from that height, you always get the same amount of heat generated, and that amount of heat would be just sufficient—I mean neither more nor less than sufficient—to raise the temperature of one pound of water one degree Fahrenheit. Thus, if you conceive a pound weight falling from this great height, 772 feet, and conceive all the heat generated by its collision with the earth collected together and put into a pound of water, that pound of water would have its temperature elevated one degree. Now, by proper means we can reverse this process, and by means of heat we can lift the pound weight. If we lift the pound weight to a height of 772 feet, of course we should then be pulling it, as it were, away from the earth which attracts it; and in order to lift this pound weight to that height we should consume—in fact, annihilate, destroy—an amount of heat equal to that which would raise a pound of water one degree in temperature; so that the amount of heat consumed in lifting the weight 772 feet is exactly equal to what is generated when the weight falls from a height of 772 feet. Now, if we lift one pound of matter one foot from the ground, a certain term is employed. It is called "*the foot-pound*;" and if you lift a pound weight to 772 feet it is 772 foot-pounds; or if you lift 772 pounds to the height of a foot you have 772 foot-pounds. Now, this quantity of 772 foot-pounds, which would raise the temperature of a pound of water one degree, is termed "*the mechanical equivalent of heat.*"

In lifting a weight from the earth we are overcoming attraction of the earth, and in doing this we consume heat, if heat be the agent which lifts the weight. Now, I have

asked you over and over again to figure the atoms of solid bodies such as this I hold in my hand. As a general rule, when heat is communicated to a body the atoms are forced asunder. You know the enormous power and force with which these atoms may attract each other, for I showed you that when an iron bar was cooled the contractible force pulling together its atoms—the mutual attraction of its atoms on cooling—was sufficient to smash the steel bar which you saw broken in front of the table. Now, we have amongst the atoms of bodies pulling each other together an action substantially the same as that which occurs when we separate the weight from the earth. To this action we may give a name. Let us call this work which occurs in a body "atomic work" if you like—work done on the atoms. This work necessitates a consumption of heat. Heat is consumed in this way; and what I want you now to bear in mind is that the amount of heat consumed is very different indeed in different bodies; and consequently some bodies, in order to raise them one degree in temperature, require more heat than others. In order to raise one pound of the liquid metal mercury one degree in temperature a certain amount of heat must be imparted to it. It would require *thirty times* that amount of heat to raise a pound of water one degree in temperature. Water requires thirty times the quantity of heat required by mercury, simply because the work to be done is a great deal more than that necessitated in the case of mercury. Now, I want to show you what follows from this action. It would appear, in consequence of this atomic work which I have been speaking of, as if the water had a power of storing up heat thirty times greater than the power possessed by mercury; and, indeed, formerly people thought that heat *was* something stored up, and they called the amount of heat which it was needful to impart to a body to raise its temperature one degree its "capacity for heat." They looked at a body as a kind of vessel for heat, and hence they used this term "capacity for heat." It was found by experiment that the capacity for heat (as the term went) was very different in different bodies; and the amount of heat which a body had stored up was determined by what the body could do—by the amount of ice or wax which it could melt.

I have here a vessel of hot oil, and in it I have spheres of metal of different kinds. They are all equally hot at the present time; but you will find that these spheres of metal have very different powers in melting bodies. They

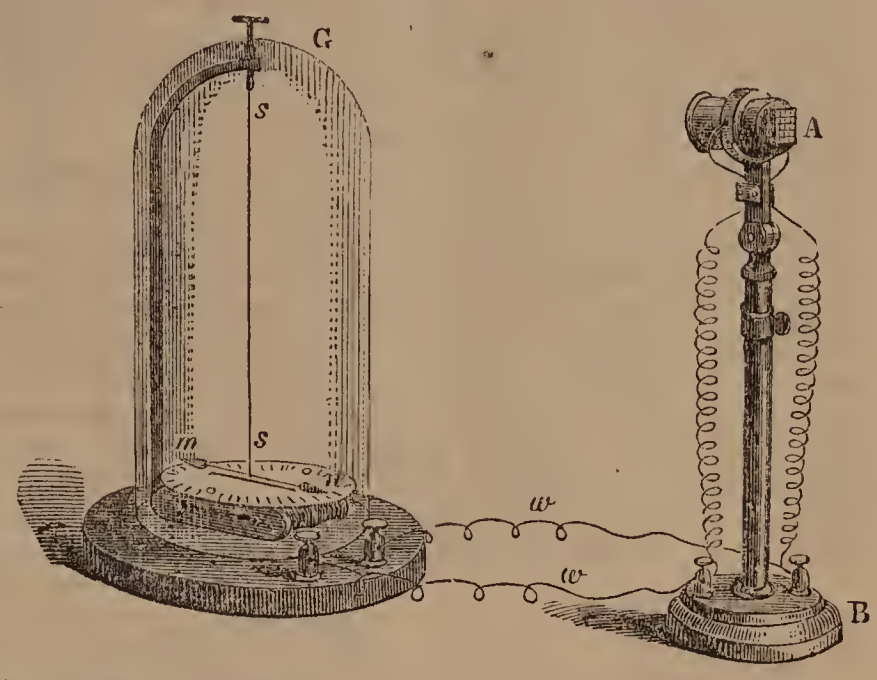
Fig. 1.



will be placed on a flat piece of wax, D (Fig. 1), and their heat will act upon that piece of wax. Some will force their way through, and others will not. This ball of copper will go through the wax first. The tin will go partly through. The bismuth certainly will not go through, although it is just as hot as the copper. Here, too, we have a ball of lead which is not competent to melt its way through the wax. The ball of iron will go through. Here is a ball of zinc; I think that will go through; but I am sure that the lead and tin and bismuth will not do so. [The balls of copper, iron, and zinc melted their passage through the slab of wax, and fell to the ground one after the other. The three other balls did not perforate the wax.] This illustrates the different amounts of heat possessed by these bodies, although they are all at the same temperature.

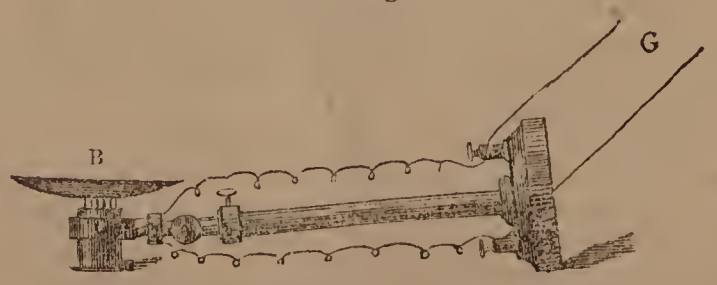
We must now go on considering the heat consumed; and I must rapidly make a few experiments illustrative of the consumption of heat in this work of forcing the particles of bodies asunder or changing their position. One of the most remarkable cases of the consumption of heat occurs when a body is caused to pass from the solid state to the liquid. Here, A B (Fig. 2), I have a beautiful instrument [the thermo-electric pile], which has been intro-

Fig. 2.



duced to your attention before. It is a kind of thermometer, and I want to show you how we can make use of this instrument for the purpose of ascertaining whether we have cold or heat. I cannot go into the full explanation of the thing; but if you observe the needle *m n* of the galvanometer G, to which it is connected by the wires *w w*, you will see how wonderfully delicate the instrument is. It is more delicate than any thermometer whatever. I will turn the face of that instrument towards me, or I will breathe against it, or I might allow any young philosopher present to breathe against it. The warmth of his breath would at once make itself evident by causing that magnetic needle to move. Now, as I breathe against this pile, you observe that the red end of the needle comes towards me. When the needle returns to its former position and comes to rest, I will try the effect of cold upon the instrument, which, you will remember, is called a thermo-electric pile. (You see I can stop the needle by means of this other needle in a moment.) I will now put a piece of this ice in a spoon, and on the cold spoon coming in contact with the face of the pile you will see that the red end of the needle will move towards you, and away from me. Thus, in this instrument we have the means of telling whether heat or cold has been imparted. We now again bring the needle to rest. And now we have made the acquaintance of this beautiful instrument, I will proceed to experiment with it. Here is a little flat

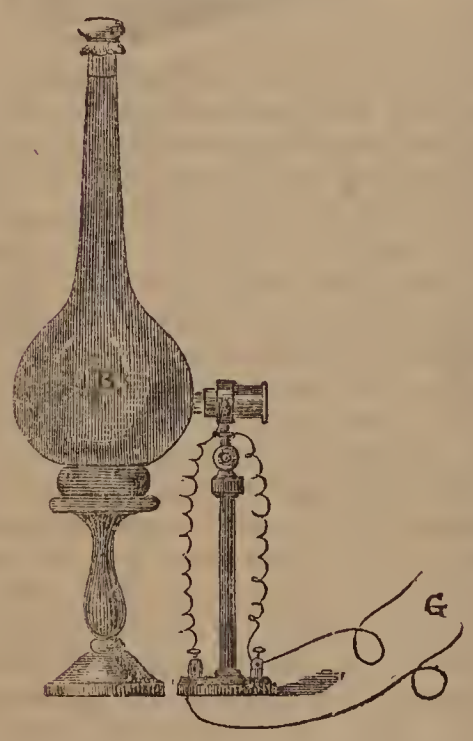
Fig. 3.



basin, B, which I place upon the face of the pile, thus; and you observe that although that dish has been up to the present time resting upon the table it has become a little warm, and causes the red end of the needle to move

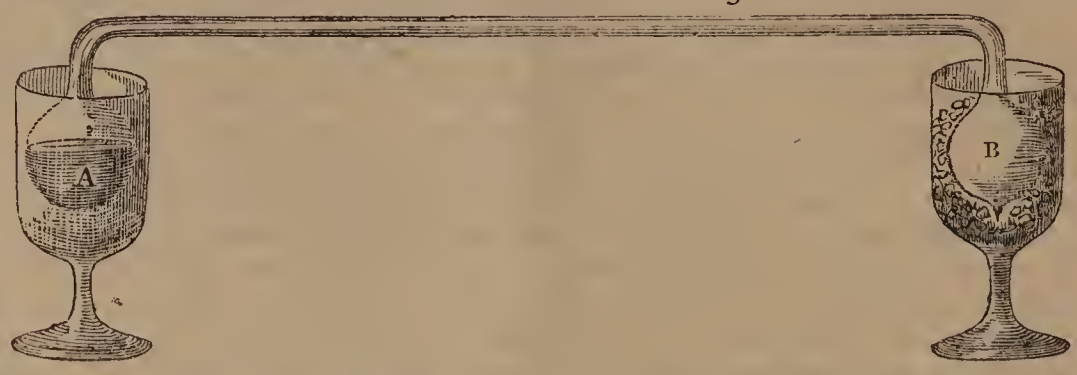
towards me. But when I pour a little cold water into this dish you see the suddenness of the movement of the red end of the needle towards you. I will now warm this water by dipping my finger into it, and after a time you will see that the needle will come down in consequence of the warmth imparted to it by my hand, and come back on the other side of the middle line. [After a pause.] You see that the needle now comes to my side, showing that the water is warmed by my finger. And now I might take sugar, or salt, or saltpetre, which would be still better, and put a little of the powder of that saltpetre into the water. That powder would become liquefied, and on its melting the warmth of the water is consumed—is used up, and the water is thereby chilled. Now, in making this experiment I will confine myself to a particular substance called sulphate of soda. You see that there is now a very great deal of heat imparted to the water by my finger, and that the needle comes very much on my side of the middle line. I will now pour into the water some powdered sulphate of soda, and you find that the water immediately becomes chilled by melting that sulphate of soda. This, then, is a consumption of heat by the act of liquefying or melting the sulphate of soda. I want now to make another experiment. It is a very instructive one. I want to show you the reverse of the last experiment. When dissolved sulphate of soda is permitted to solidify—become solid—you get out of it the heat that was expended in rendering it liquid. I have in this flask, B (Fig. 4), somedissolved sulphate of soda. It was carefully melted last night, and has been carefully kept apart from any thing which could disturb it. We will allow the face of the pile to rest against the bottle; and now I want to cause that body to solidify before your eyes. I can cause it to become crystallised sulphate of soda, like that which was dissolved in that dish a moment ago. You will see the liquid in the flask become more and more opaque, and when it begins to solidify opposite the face of the pile it will give out heat — the heat that was expended in melting it, and you will then see the red end of the needle come towards me. I will now open the neck of the flask, and throw a crystal of sulphate of soda into the solution. [This was done, and the contents of the flask began to solidify from the top downwards.] You now see the compound crystallfsing; and the moment that portion opposite the face of the pile becomes solid, heat will be communicated to the face of the pile, and we shall get a deflection (as it is called) of the red end of the needle in the direction in which I stand. [After a pause]—What I predicted was quite right. There we get out of the sulphate of soda the heat that was expended in melting it. There is the movement of the needle caused by the heat.

Fig. 4.



I might go on in this way, and show you that when a body is evaporated you also get a very large amount of heat consumed—used up—in order to evaporate it. In order to convert a pound of water at 212° Fahrenheit into steam at 212° Fahrenheit, an enormous amount of heat is required. It requires as much heat as would raise 967 pounds of water 1° Fahrenheit; and this heat is insensible to the thermometer, although it is so great. The reason that I employed a mixture of ice and salt as a freezing mixture in a former experiment, was that the action of the salt produces a liquefaction of the ice, and on that liquefaction taking place a large quantity of heat is consumed—so much that the temperature of the liquid is reduced far below the temperature of the ice itself. I am going to illustrate this point by the development of cold by vapourisation; and if things go fairly I should not wonder if I could freeze water before your eyes by means of its own evaporation. An experiment has been arranged there for the purpose. Here are two bulbs, A and B, in this apparatus (Fig. 5), and the water which was in one of them has been frozen in this room since the lecture began. One end of this has been placed in a freezing mixture far away from the bulb where the water is frozen. This instrument is called a "cryophorus," or ice carrier. Water was placed in one bulb, and the air was taken from the interior of the instrument. The other bulb was placed in a freezing mixture, and as the vapour came over from the water it was condensed by the freezing mixture, and the vapourisation which took place has been sufficient to freeze the water.

Fig. 5.



So much, then, for the heat consumed in causing a body to pass from the liquid state to the state of vapour. I have on the table various substances which would enable me to illustrate this in a very satisfactory manner. For instance I will take a little alcohol, and warm it by placing my finger into it, thus. I see there is a great amount of heat in the face of the pile. I have no doubt that the evaporation of the alcohol will very soon cause the end of the needle to come down; or if I take a substance that can vapourise more rapidly than alcohol—this substance, ether—it would not take an instant in order to overcome the heat which is the cause of that deflection. I will cause evaporation to go on a little more quickly, and if the needle be not held fast by some accident we shall soon find the heat which causes the present large amount of deflection entirely abolished, and the needle will move down. Now you see the needle comes back. We get an enormous amount of cold by the evaporation of ether, so much that we can easily freeze water by it.

(To be continued.)

Isoxylol, Preliminary notice.—R. Fittig. Mesitylenic acid, the product of the reaction of diluted nitric acid upon mesitylene, is decomposed by being heated with caustic lime according to the equation—

$$C_9H_{10}O_2 = C_8H_{10} + CO_2.$$

The new hydro-carbon isoxylol resembles its isomer xylol very closely in many points, but widely differs from it in its behaviour towards oxidising agents. Chromic acid, for instance, converts xylol into terephtalic acid, while isoxylol is oxidised to isophtalic acid, isomeric with the former. This new acid is readily soluble in alcohol, almost insoluble in cold, sparingly soluble in hot water. From the latter it crystallises in long needles, which fuse above 300°C. A dibasic homologue of isophtalic of the composition $C_9H_8O_4$ has been obtained by slow oxidation of mesitylenic acid, besides tribasic trimesinic acid, described on a former occasion.—(*Zeitschr. Chem.*, N.F. iii., 526.)

ON THE INFLUENCE OF

APERTURE IN DIMINISHING THE INTENSITY OF THE COLOUR OF STARS.*

By JOHN BROWNING, Esq., F.R.A.S.

AT the last meeting of the Society some remarks were made on the subject of the amount of colour visible on the moon during the late lunar eclipse.

I had previously stated that I had failed to detect either the coppery or the blue tints generally seen during the occurrence of this phenomenon. As my observation did not agree with those of several well-known observers, I have given the matter some attention, and endeavoured to ascertain from what caase the discrepancy proceeded.

Mr. Slack suggested that probably my having used a telescope of larger diameter than those employed by most of the observers would prove the explanation desired, and since then I have heard that our able Secretary, Mr. Huggins, is of the same opinion. The result of my inquiries completely confirms this suggestion. I find that while most observers who use telescopes of only three or four inches aperture speak of the colour as being less than usual, yet very noticeable, observers who use telescopes of seven or eight inches aperture saw very little colour. Neither Mr. Barnes nor myself, observing with a 10¼-aperture, nor Mr. With or his nephew, employing a 12¼-inch silvered-glass speculum, could detect any colour at all.

It is true that I failed equally in detecting colour with a 4-inch object-glass, but I account for this by supposing that the sensitiveness of my eye to faint-coloured light had been injured by the glare of the moon in the large aperture. Experimenting in connection with this subject, I have noticed that the chocolate colour of the so-called belts of *Jupiter* is much more perceptible with 6-inches apertures than with 12 inches. Again, a small star in the cluster in *Perseus* appears of an indigo-blue with 8½ inches, Prussian-blue with 10¼-inches, and royal-blue with 12¼ inches of aperture. It follows from this that colours estimated by comparison with the ingenious chromatic scale of Admiral Smyth, in which each colour is represented of four different degrees of intensity, will not possess any relative value unless taken in connection with the aperture employed when the colour was estimated. Were due allowance made for this disturbing influence of variation of aperture, I think many discrepancies between the colours attributed to double stars by different observers might probably be reconciled.

Note.—An enlarged diagram of Smyth's chromatic scale, and another showing the apparent difference in the colour of a star when seen with apertures of 4 inches and 12 inches, was exhibited and described at the time the paper was read.

* Proceedings of the Royal Astronomical Society.

OBSERVATIONS ON THE

NATIVE HYDRATES OF IRON.

By GEORGE J. BRUSH.

WITH ANALYSIS OF TURGITE.

By CHARLES S. RODMAN.

THE well known iron mines of Salisbury, Conn., have long enjoyed a reputation among mineralogists as furnishing superior specimens of *limonite*, and hitherto this has been thought to be the only ferric hydrate occurring in quantity at this locality. Minute crystals of supposed Göthite have occasionally been found, but not in quantity sufficient to render certain their mineralogical determination.

On a recent visit to these mines Mr. Rodman obtained a considerable number of specimens, lining pockets in the ore, which had the usual brilliant metallic lustre on the interior surface, and showed on the fracture a fibrous structure, but differed from brown hæmatite in having a decidedly red colour, and in affording when pulverised a red powder, closely resembling that of ordinary red hæmatite. This red layer was in some cases an inch or more in thickness, and was deposited on a bed of limonite (brown hæmatite); the line of demarcation between the brown and the red ore was so perfect, in most instances, as to readily admit of a complete separation of the two minerals.

An examination of this red ore showed it to be an oxide of iron, containing not far from 5 per cent of water, a number of specimens yielding very uniform results; and a complete analysis proved the mineral to be a ferric hydrate with the formula Fe_2O_3,½HO, identical with the *Turgite* of Hermann,† and with Breithaupt's *hydro-hæmatite*, as analysed by Fritzsche.‡ The physical characters are so nearly those of ordinary anhydrous hæmatite that it is difficult to distinguish the species without having recourse to an estimation of the loss on ignition. The turgite yields an abundance of water when heated in the closed tube, and it decrepitates in a remarkable manner. Hardness, about 5·5. G.=4·14. For analysis the mineral was carefully dried over sulphuric acid until of constant weight, and this desiccated mineral was then heated for several hours in an air bath at 100° C. without showing any further diminution of weight. The amount of hygroscopic moisture abstracted from the air-dried mineral by treatment in the desiccator was 1·40 per cent. The iron in one instance was determined by titration with permanganate of potash; in the second case it was thrown down by ammonia, the precipitate washed, dried and weighed, and then the iron was separated from the silica and alumina by Deville's method by first reducing with hydrogen, and subsequently volatilising the iron by heating in a current of dry hydrochloric acid gas. The analytical results were all obtained by Mr. Rodman. Composition:

	1.	2.	Mean.
Ferric oxide	91·45	91·29	91·36
Manganic oxide	0·67	0·55	·61
Alumina	0·75	.....	·75
Silica	0·22	0·24	·23
Phosphoric acid, sulphuric acid, and cobaltic oxide	traces	.....	traces
Insoluble in acid	1·83	.....	1·83
Water	5·20	5·21	5·20
	100·12		99·98

Other determinations of water on different specimens gave 5·02 and 5·09 per cent.

Five grammes of the mineral yielded only minute traces of sulphuric acid, and three grammes showed but an unweighable trace of phosphoric acid. A very perceptible trace of cobalt was found even on examination of one gramme of the mineral. The portion insoluble in acid proved on analysis to consist entirely of silica, and excluding this, with the small amount of silica and alumina found in the soluble portion, the result of the analysis is

Fe_2O_3 94·00	Mn_2O_3 0·63	HO 5·35	=99·98
Oxygen, 28·20	0·19	4·75	
28·39			

giving the oxygen ratio 6 : 1 or Fe_2O_3,½HO.

This result confirms the conclusions of Hermann and Breithaupt, that there is a native ferric-hydrate with one half an equivalent of water. Several years since the attention of the writer was called to this subject by Prof. W. T. Roepper, of Bethlehem, who stated that he had found Breithaupt's hydrohæmatite to be of frequent occurrence with the limonite ores of the Lehigh valley. A water determination on the Lehigh mineral made by Prof. Roepper, and kindly communicated for this article, gave 5·34 per cent, and Prof. Roepper calls especial attention to the characteristic decrepitation of this mineral when heated. On examination of the specimens of limonite in the Yale College collection, a fine specimen of the red hydrate was found occurring with the limonite of Düssel-

† Journal für praktische Chemie, xxxiii, 97.

‡ Breithaupt, Vollständiges Handbuch der Mineralogie, iii, 840.

dorf in Prussia, this yielded on examination by Mr. Rodman 4·75 per cent water. Another specimen was found from Ioditz in Bavaria, besides numerous specimens from Salisbury in Connecticut. A mineral of like composition has also been found by Bergemann* at the Louisa Mine near Horhausen in Prussia. From these numerous localities it would appear that the mineral is of common occurrence. It has heretofore been confounded by most mineralogists with hæmatite, which it so strongly resembles in physical characters. It may be readily distinguished from hæmatite by simply heating a fragment in the closed tube, when it decrepitates violently and gives off large amount of water.

Hermann does not give the pyrognostic characters of *turgite*, but Breithaupt, in his description of *hydro-hematite*, makes particular mention of its characteristic decrepitation when heated. The turgite is described by Hermann as being associated with copper ores; its chemical composition is however identical with hydrohæmatite, and as it has priority of publication, the species must bear the name of turgite, and hydrohæmatite be used only as a synonym.

We have therefore three well defined hydrates of iron occurring native and forming three distinct and well-established mineral species, differing from each other in physical characters and in their relative content of water.

Turgite	$Fe_2O_3 + \frac{1}{2}HO$
Göthite	$Fe_2O_3 + HO$
Limonite	$Fe_2O_3 + 1\frac{1}{2}HO$

Two other hydrates have been described containing respectively two and three atoms of water. Murray* found in a brown iron ore from Huttenrode in the Hartz—

Fe_2O_3 81·41, HO 17·96, SiO_3, 0·17, Carbon 0·46 = 100, giving the formula $Fe_2O_3 + 2HO$.

A compound of similar composition from Kilbride in Ireland, having a pitchy colour, analysed by Haughton, gave Fe_2O_3 77·15, HO 20·43, SiO_3 0·30, Al_2O_3 tr., PO_5 1·60 = 99·48.

Xanthosiderite also appears to be a mineral of like composition, but its mixture with a silicate of unknown composition renders it difficult to conclude positively that it belongs here.

A. H. Church† has analysed a stalactite of a rust-coloured ferric hydrate from Botallack mine in Cornwall, which gave:—

Fe_2O_3 73·70, HO 24·40, loss, PO_5, and organic matter 1·76 = 100, giving the formula $Fe_2O_3 + 3HO = Fe_2O_3$ 74·77, HO 25·33.

Other analyses of ferric hydrates by many different analysts, and from a great range of localities, give an amount of water which corresponds to one or the other of these last two hydrates; but as these contain also either organic matter, phosphoric acid, or silica in the combined state, it is impossible, without further investigation, to know to what hydrate to refer them.

The artificial ferric hydrate precipitated by ammonia from ferric chloride varies in composition according to the method of treatment. Schaffner obtained a hydrate with one atom, Gmelin with two atoms, and Wittstein with three atoms of water; this last kept for some time under water became crystalline, and was converted into a hydrate with one and a half atoms of water. Recent investigations by E. Davies‡ show that the ordinary precipitated ferric hydrate loses water on being boiled in water; in one case the amount of water was reduced to 3·52 per cent. Similar experiments conducted in this laboratory by Mr. Rodman showed that by continued boiling in water the amount of water remaining in the hydrate could be reduced even to two per cent. These facts, as Mr. Davies suggests, explain in a very satisfactory manner the association of the different ferric hydrates in nature, and do not necessarily demand the supposition of great heat to account for the large beds of anhydrous hæmatite found in different parts of the world.

* Rammelsberg, Handbuch für Mineralchemie, 988.
† Journ. Chem. Society, II. iii. 214.
‡ Journ. Chem. Society, II. iv. 69.

FOREIGN SCIENCE.

PARIS, JAN. 28, 1868.

Science in the prisons.—The gallic fermentation.—Spectra of flames issuing from furnaces.—Action of the Alkaline Silicates on the Animal Economy. ACADEMY OF SCIENCE, January 6.—On the molecular theory of bodies.—Employment of the electric light.—Estimation of small quantities of peroxide of hydrogen.—Action of chloride of cyanogen on zinc ethyl.—Sitting of Jan. 13th.—Electro-capillary actions.—Blue colouring matter of certain dead woods. Sitting of Jan. 20th.—Titles of papers.

As showing an advantage, unrecognised, perhaps, by many, of living under enlightened rulers in a country where chemical science is appreciated, the mention of a strange fact related in one of the scientific journals may find place here. The narrator visiting a prison asked his guide, are the prisoners well nourished? "Mon Dieu, Monsieur," the man replied, "the bill of fare for each day has been prepared by a special commission, 33 per cent nitrogenous matter, 27 albuminoid, 15 of gelatin, 18 of fibrin, 7 of hydrated matter." The guide also informed him that each prisoner had, besides, the right to 10 cubic metres of respirable air, 10,000 litres!

Some account of M. Van Tieghem's memoir on the gallic fermentation, presented to the Academy of Sciences, has been promised for these columns. M. Van Tieghem has treated the subject elaborately. At the outset he alludes to the diverse opinions which have been expressed with regard to the causes, besides the oxygen of the atmosphere, which lead to the transformation of tannin into gallic acid. One opinion attributes this result to a slow oxidation, and to the pre-existence of a soluble ferment; and some have admitted, while others have denied, the presence of sugar in the products.

1. Tannin does not undergo the metamorphosis when protected from the atmosphere. If a series of flasks be filled entirely with a solution of tannin or a filtered infusion of nut-galls, and placed in vacuo for 24 hours, then saturated with carbonic acid, carefully corked and heated, and finally sealed while hot, the solution will remain unchanged for any length of time. The transformation of tannin into gallic acid is not, then, due to the pre-existence of a soluble ferment.

2. Tannin does not undergo metamorphosis by simple contact with the air.

A solution of tannin introduced into a series of flasks drawn out at the neck and curved, boiled for some minutes, and left in a quiet place at a temperature of about 25° C., will remain unchanged for any length of time.

3. For tannin to undergo the metamorphosis, the development of a species of fungus in the solution is essential and sufficient. The gases composing the atmosphere alone effect no change, but the atmosphere carries to the solution spores, and these require for their germination oxygen. Under these influences the tannin splits up into gallic acid and glucose, the elements of water becoming fixed. When the transformation is complete, the whole of the gallic acid indicated by theory is found, but the glucose is always in less and somewhat variable proportion; the vegetation assimilates a part of it. Thus the sugar from the tannin furnishes the hydrocarbon aliments necessary to vegetable life. For the reaction described to take place, the plant must be developed in the interior of the solution; if only developed on the surface, the amount of vegetation germinated is immensely greater, but the reaction then takes quite a different form. Large quantities of carbonic acid are exhaled, and from a concentrated solution only a small quantity of gallic acid and traces of sugar remain after a few days' exposure. It remains to be shown that the fungus, during development and life, splits up the tannin, and that the change is not the result of soluble principles secreted by the latter, capable of acting without the organism. To establish this it is only necessary to introduce into a solution of tannin some of the vegetation from an active fermentation, and exclude

the air as in the first experiment. The fungus developed is that known as the *Penicillium glaucum* or the *Aspergillus niger*.

The Austrian Professor, M. Lielegg, has made some observations with the flames coming from furnaces in which iron is worked solely by the Bessemer process. This flame is carbonic oxide in a state of incandescence. The appearance and disappearance of spectral lines mark the progress of the metallurgical operations. At the moment when the decarburisation of the iron commences, and when it has reached the proper limit, these lines suffer essential modifications. The appearance of a group of lines and of one distinct line at the violet end, marks an important stage during the formation of malleable iron; these lines disappear sooner than any of the others, this effect taking place within the last five minutes of the operation, so that they serve to denote the termination.

The action of the alkaline silicates on the animal economy has been studied by M. Husson. His experiments were made upon dogs. Solutions of silicate of sodium were administered to them; they were afterwards killed, and the organs submitted to chemical examination. These are some of the results M. Husson has arrived at. The alkaline silicates given in quantity so minute, that the contents of the stomach remain acid, are completely decomposed, the same is the case when they are in very dilute solution: the intestinal juices are unable to redissolve the silica. It follows that the alkaline silicates can only enter into the blood when administered in sufficient quantity to be alkaline in the small intestine. Traces only are found in the blood. No deposit forms in the brain, the liver, the bile, or the bones; but the muscles contain appreciable quantities of precipitated silica, as does the spleen. By far, however, the largest quantity of silica is precipitated in the urine as silicic acid and silicate of lime. M. Husson explains the precipitation in the muscles as being due to the acid developed during exertion, biphosphate of sodium playing the same part in causing the urinary deposit. The symptoms produced are—turbidity in the urine, difficulty in passing the same, and congestion of the kidneys.

At the *séance* of the Academy of Sciences held on Jan. 6, M. Guldeberg sent a very mathematical paper on the molecular theory of bodies. M. Becquerel presented a note by M. Le Roux on experiments relating to the employment of the electric light. M. Houzeau made known a method of estimating small quantities of peroxide of hydrogen.

In the presence of an acid peroxide of hydrogen decomposes either in the cold, or when heated, a solution of neutral iodide of potassium; iodine is set at liberty, and potash formed, which combines with the acid according to the equation—

$$HO_2 + KI + \text{acid} = HO + KO, \text{acid} + I.$$

As a consequence of this it is evident that by simply estimating the potash formed, the amount of peroxide of hydrogen is arrived at. The process is conducted as follows. Titrated acid is first added to the neutral oxygenated solution, and then a slight excess, usually a few drops, of neutral iodide of potassium. The mixture is heated to aid the reaction, and the iodine completely expelled by ebullition. Finally a titration is performed with an alkaline solution. Thus the amount of residual acid is estimated. The solution of iodide of potassium is made by dissolving three grammes of the salt in 100 grammes of water. When the sulphuric acid and neutral iodide of potassium are sufficiently diluted, they do not react upon each other, either in the cold or upon heating. Contrary to what has been observed with regard to ozone, oxygenated water does not seem to react with iodide of potassium when the solutions are neutral. But the vapour of peroxide of hydrogen blues, notwithstanding at the same time that ozone does the iodide and starch-paper. Neutral iodide of potassium can equally serve to detect oxygenated water when this has been previously acidulated. In most cases the yellow or pinkish colour given to the solution may be considered characteristic of peroxide of hydrogen; but the sensitiveness of the reaction is augmented by the employment of chloroform, which is rendered violet or rose-coloured by traces of iodine invisible in water. Nitrites, hypochlorites, and other analogous salts react on iodide of potassium in the same manner as oxygenated water. This source of error may be removed by operating as follows:—3 or 4 c.c. of liquor is rendered acid, if neutral or alkaline, by a sufficient quantity of very dilute sulphuric acid. The addition of a few drops of iodide of potassium is the next step. A yellow or red colouration produced indicates the presence of oxygenated water, or nitrites and analogous salts. The experiment is then repeated after previously boiling the acidulated solution for a few minutes to expel nitrous acid, &c. If upon the addition of the iodide it still produces a colouration, this indicates the presence of peroxide of hydrogen. If in the cold there is no colouration, the solution is heated; if the reaction takes place, oxygenated water is present. If no colouration is produced under this treatment, a drop of chloroform is added, and the mixture agitated for a few minutes at about 40° C.; a rose tint indicates the presence of oxygenated water; if no tint is produced, it may be concluded that the solution does not contain oxygenated water, or that quantity is too minute to be detected. A solution may be concentrated either in vacuo or over quick-lime. Concentration may also be effected by heat. A very dilute solution of oxygenated water containing sulphuric acid may be boiled for some minutes without suffering an appreciable decomposition.

A paper entitled "researches on the action of chloride of cyanogen on zinc ethyl," by M. Gal, was presented by M. Fremy. M. Gal recounted the experiments he had made. He finds by acting upon zinc ethyl by gaseous chloride of cyanogen, a liquid is obtained, boiling at 98°, identical with hydrocyanic ether; the reaction is the following—

$$C_2NCl + C_4H_5Zn = C_4H_5C_2N + ZnCl.$$

The author undertook the research in order to throw light upon the constitution of hydrocyanic acid, and to determine which of the two isomeric ethers of this acid should be considered as its homologue: he has not been able to decide. One of the ethers boils at about 82°, the other at 98° C. On the 13th of January, M. Becquerel made known the results of further experiments upon electro-capillary actions; this is the fourth memoir upon the subject. In decomposing mixed metallic solutions, he has observed that the metals are deposited separately: from a solution containing nitrate of silver and nitrate of copper, the metallic silver is alone deposited. M. Thénard presented a paper, by M. Romier, on the blue colouring matter of certain dead woods. M. Romier thought of the matter being applicable to dyeing purposes, and specimens of silk owing their tints to it were examined by the Academy. At the meeting on the 20th of January, Father Secchi contributed a second note on stellar spectra. There was also a note on the colouration effects which are produced when the sparks from an induction coil pass between the surface of a liquid and a platinum pole, from M. Becquerel. M. Dupré presented a memoir on molecular attractions and chemical operations. M. Miergnes addressed a communication describing a new pile composed of zinc and carbon. Having already covered the space accorded to him in these columns, your correspondent must content himself at present with simply announcing these interesting papers.

Cymol from Camphor.—Longuinin and Lippmann. Equal molecules of camphor and phosphoric perchloride are intimately mixed, and the mixture very slowly distilled from a retort. The distillate is washed with water, dried, and rectified over sodium. It is then quite pure, having its boiling point between 175° and 178° C.—(*Bull. Soc. Chim.* vii. 374).

DR. LETHEBY ON FOOD.

Lecture II.

(FROM OUR OWN REPORTER.)

Dr. Letheby delivered this, his second lecture "on Food," at the rooms of the Society of Arts, John Street, Adelphi, on Monday evening, the 27th instant, in the presence of a numerous audience. Dr. Letheby commenced by stating that the relative properties and digestive functions of food were purely physical, and chemical in their character. There was, he remarked, a greater number of solvents in the alimentary canal than one would suppose. The saline and gastric juices acted with great force, and the quantity of those foods poured in for the purposes of digestion amounted to rather more than 3 gallons in the twenty-four hours. The amount of saline was 3½ lbs., in which there was 231 grains of solid matter; 14$\frac{1}{10}$ lbs. was made up of the gastric juices containing 3,000 grains of solid matter, and 316 grains of pepsine; the pancreatic acid 8·82 lbs., in which was 6,000 grains of solid matter, and 784 of a peculiar principle of fat and starchy matter; bile 3½ lbs.; and the intestinal mucus ½ lb. Food in the alimentary canal is not only submitted to the action of solvents but also that of water, drenching the food rather than dissolving it. Saline matter is secreted by many of the glands, and is composed of foods all slightly alkaline at the time of digestion; though at this time the potato is strongly acid in the interval of passing from the stomach to the alimentary canal. The potato composed of liquid and solid matter, half of which is inulin, which is something like the peculiar principle found round the germ of all seeds, and which is of such a character that it will not pass through the membranes of the body, but is discharged through the alimentary canal. There were some articles which served artificially to promote digestion, such as Liebig's extract of malt, which proved to be a powerful solvent. The gastric juice which is secreted from the glands is a thin transparent fluid, and from possessing a large proportion of organic matter called pepsine this fluid has the power of changing albumen, which is known as pepsine or albuminous peptone, and which differs from ordinary albumen in this respect that it is not coagulated by heat. This ordinary albumen will not, however, pass through the membranes till acted upon by pepsine. The digestive power of the gastric juice is destroyed if the temperature gets above 120° Fahrenheit or below 100° Fahrenheit. At the latter point the digestion does not take place very rapidly. Dr. Letheby then described how this gastric juice was obtained from the stomachs of pigs and sheep, stating that it was when the animal had been kept unfed for some time, and when his whole nervous system was excited by the smell of some savoury dish that it was killed, and that the quantity of the pepsine produced from the stomach of the pig was better than that obtained from the sheep. He then proceeded to say after a few words respecting pancreatic acid, observing that it was a clear colourless fluid possessing the power of being either alkaline or acid whose functions were were not well known. Twenty years ago Bernard had thought it possessed the power of making soap, but lately Mr. Schweitzer, of Brighton, had carefully gone into the subject, and found that it made fat. It possessed a powerful solutive action; and it may—though this is generally denied—act on nitrogenous matters. Dr. Letheby then proceeded to devote his attention to what he said was a very complex subject, and on which he should say but little—the bile. It was of a yellow colour, and slightly alkaline, which came into the body daily to the quantity of 3½ lbs., of which about 14 per cent was solid matter, of which 12 are organic. Several hypotheses were presented to the meeting, and the lecturer, whilst not being able to pronounce positively upon the matter, seemed inclined to believe it to be a residuum from the liver in making blood to circulate through the body. After a few words bestowed upon the intestinal mucus, Dr. Letheby proceeded to apply the facts already gathered to an explanation of the phenomena of digestion, first taking protinaceous or nitrogenous matters digested by the gastric juices, of which there are several divisions, their order of easiness in digestion being as follows, viz.:—Liquid and soft albumen, then hard albumen, followed by fibrine, gelatine, cartilages, and appendages of the skin, in the order just named, showing, as regards the latter division, that all appendages, such as hair, feathers, and wool, were thoroughly insoluble; citing, as an example, the case of the boa at the Zoological Gardens, who some years ago, swallowed his blanket which, a few days afterwards, was cast out of the alimentary canal uninjured; that starch was converted into the low form of sugar known as glucose; that gums and all bodies of a similar character were indigestible, and were therefore not only worthless but did harm, causing other food to be hurried in its passage through the body, and that saline matters were digested by the acid of the stomach. Dr. Letheby then proceeded to state what were the most digestible kinds of food, quoting Dr. Bowman's observations made under highly advantageous circumstances. Dr. Bowman had a patient whose wound affected the stomach, and which required to be kept open. By these means Dr. Bowman was enabled to see the processes the food swallowed by the man underwent during the time of digestion; and also to learn the time required for the digestion of the food taken into the body. The result of his observations was that soused tripe was the most digestible of all foods, taking but one hour to digest; that venison ranked next, taking one hour and a half; then raw eggs or raw oysters, taking one hour and three-quarters; ox liver, two hours; poultry, lamb, and hard boiled eggs, two hours and a-half; beef and mutton two hours and three-quarters to three hours and a quarter; pork, three hours and a quarter; and lastly, salt beef, four hours.

The question, How digestion may be helped on? possesses the following answer. There must be a proper selection of foods as regards tenderness and flavour; a proper variation of the food from day to day, and by carefully watching how this food is cooked; and by the maintenance of a proper temperature of the body and of a cheerful temper.

The chief constituent of food is water, and the liquid makes its appearance in all parts of the body to the extent of 75 per cent. Thirty pounds per diem is wanted to properly carry on the process of digestion, and its importance may be fully appreciated when we see that it dissolves tissues, and carries the blood into circulation; that it carries out of the body waste; that it cools the body when unduly heated; and that it lubricates the whole system. Like everything else, however, it must be taken in a proper manner; least in the morning, more at noon, and most at night, when it is greatly required to carry off the accumulated wasted tissues, and leave the body clear for the next day's operation. Dr. Letheby then passed on to nitrogenous or plastic matter, and showed how gradually, from the belief that the nitrogen within us supplied our muscular force, it had come to be disputed, questioned, experimented upon; and that finally in the year 1866 two professors of Zurich, Fick and Wislecenus, had taken the trouble to put the matter to a practical test by ascending the Faulhorn, one of the Bernese Alps, and at an altitude of 6,417 feet above the level of the Lake of Brienz. During the day before, the time whilst they were at their work, and for a few hours after, they religiously abstained from eating any thing containing nitrogenous matter. The ascent of the mountain took six hours, and during that period, and for some time afterwards, they collected all the secreted nitrogenous matter, which by the most liberal computation but provided for half the strength requisite for these two gentlemen to reach the top of the mountain; and to prove that more nitrogen was evolved before and after the work was done, the quantity of nitrogen

sensibly increased after a meat meal. Dr. Letheby then proceeded to give some interesting statistics to prove the muscular force these quantities of nitrogen represented; showing that calculation should be made for the beating of the heart, respiration, and so forth, all of which went to prove the fallacy of the assertion that the burning of nitrogenous matter gave us our muscular force. Nothing of the sort could occur till the hydrocarbons in the blood were burnt, and then the nitrogenous matter could ignite; but it would be even then the hydrocarbons which were creating the muscular power; and if a test was required to prove that it was the carbon that was thrown off by exertion, there was one at hand. Take a man: during sleep he will but exhale 293 grains of carbon in the hour; let him be lying down in a state approaching sleep, and the rate increases to 355; let him sit up, to 448; let him walk two miles an hour, 1,088; let him walk three miles an hour, 1,552; and if you let him work at the treadmill at the rate of 28·65 feet per minute, 2,926. What better proof can be afforded than this, for here we have, speaking in round numbers, when all is calm and at rest, the man but breathes 300 grains of carbon per hour, whilst when he is working hard at the treadmill, 3,000. The result of the calculations must be that the chief agent of heat and force is hydrocarbon, and that nitrogenous matter goes to replace muscle; that muscles do not decay or oxidise during working, but afterwards; and that nitrogenous matter comes from food, and not from worn-out muscle. They nitrogen must be present there can be little doubt; our habits necessitate out eating meat, and the best example of this fact is that a party of Hindoos who commenced to make a line of rail were obliged to dispense with the laws of their caste and live like English navigators to enable them to complete the work. Nor can there be any doubt that too rich nitrogenous food produces force, and that a nation of meat eaters are more pugnacious than one of vegetable or carbon eaters. A brief review was then taken of the functions of fat, starchy, and saccharine matters, saline substances, and those beverages we are accustomed to indulge in at meal times, the lecture being concluded with a consideration of the question, what amount of work an average man can do in a day? The answer, based upon a comparison of the previous calculations undertaken to show the work done by the two professors whilst ascending the mountain, being that the average work a man is capable of performing, provided he is properly fed, is 10,000 lbs. lifted 1 foot high. In the inquiry, however, we learn this, that a man's external force is but $\frac{1}{5}$th portion of the whole force he possesses, and that an ordinary 10-horse steam engine will do the same amount of work at a cost of 5d. per hour, whereas the expense incurred by using a man would be £2 sterling.

On some Sources of Coal in the Eastern Hemisphere.—By Cuthbert Collingwood, M.B., F.L.S. 1. *Kelung, Formosa.*—The coal is found in depressions in red sandstone, and is of comparatively recent origin. It is light, burns very rapidly, gives out great heat, produces 50 per cent of ash, and forms considerable quantities of clinker. 2. *Labuan, Borneo.*—Several seams of coal crop out conspicuously near the coast, the lowest being 11 feet 4 inches in thickness. It is heavy, close-grained, fast-burning, and gives out considerable heat; it is of very recent date, dammara resin, and leaves of recent trees being found associated with it. 3. *Diu, Saghalien.*—Coal excellent, burns quickly, with little ash. Presents a fracture similar to Welsh coal. 4. *Japan.*—The author describes coal from several localities in Japan as bright, clean, and resembling Sydney coal, but having a tendency to form clinker. He concludes with a description of some coal from Ivanai, Niphon, which is very blean, highly bituminous, burns with a flame in the flame of a candle, and would probably be valuable as a gas producing material.—*Abstract of a paper read before the Geological Society.*

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Conversion of Gallic Acid into Tannin—T. Löwe finds that gallic acid in aqueous solution is converted into tannic acid by the oxidising influence of argentic nitrate. The oxidation is more complete if a salt of gallic acid is employed.—(*Journ. pr. Chem.* cii. 111).

Acetylene.—R. Rieth. The imperfect combustion of coal gas which takes place when the flame of a Bunsen's burner has gone down, so as to burn within the tube, has been found to be a rich source of acetylene. The escaping gases are collected by means of a funnel placed over the burner, and connected with an aspirator. The quantity of the silver compound of acetylene obtained from one burner in twelve hours amounted to 100 grammes.—(*Zeitschr. f. Chem.* N.F. iii., 598).

Oxidation of Potassium and Sodium.—The oxidation of potassium and sodium, when exposed with a clean surface to the air, is accompanied, according to H. Baumhauer, with evolution of light.—(*Journ. pr. Chem.* cii. 123).

Viridinic Acid.—O. Cech. This acid may be obtained direct from coffee by pulverising the beans, extracting them with ether alcohol, to remove fat, and exposing them in moist condition to the air. After a few days the mass, which has assumed a green colour, is exhausted with acetic acid and alcohol, which takes up the viridinic acid formed.—(*Ann. Chem. Pharm.* cxliii. 366.)

Preparation of Iodhydric Acid.—C. Winkler. Instead of preparing this acid by passing a current of sulphuretted hydrogen through water, containing iodine in suspension, the author proposes the following plan of working. Iodine is dissolved in carbonic disulphide, water placed on the top of this, and the sulphuretted hydrogen passed to the bottom of the vessel into the iodine solution. The dark colour of the latter gradually becomes lighter, while the iodhydric acid formed is completely absorbed by the water. The sulphur which separates remains dissolved in the carbonic disulphide.—(*Journ. pr. Chem.* cii. 33.)

Derivatives of Xylol and Dimethylbenzol.—R. Fittig. A careful comparison of methyltoluol, or dimethylbenzol $\mathrm{C_6H_5(CH_3)_2}$ (obtained by replacing in toluol one atom of hydrogen by one of methyl) with xylol from coal-tar, has shown that these hydrocarbons are not identical. Both, however, are converted by oxidation with diluted nitric or chromic acid into the same derivatives, *i.e.*, toluylic and terephtalic acid. Amongst the compounds prepared and examined were nitroamidoxylol, nitroamidomethyltoluol, diamidoxylol, dibromxylol, dibrommethyltoluol, parabromtoluylic acid, nitroparabromtoluylic acid, paradibromtoluylic acid, monobromnitroxylol, dixylyle.—(*Zeitschr. Chem.*, N.F. iii., 523.)

Derivatives of Sulphurous Chloride.—Fr. Grauhe. Sulphurous chloride is prepared by passing a current of sulphurous acid into phosphoric perchloride, and subjecting the products of the reaction to fractional distillation. The pure chloride boils between 78° and 79° C. Argentic cyanide converts it into sulphurous cyanide $\mathrm{S_2O_2(C_2N)_2}$, which is insoluble in water, soluble in alcohol and ether. From the latter it crystallises in long needles. Zinzic ethide decomposes sulphurous chloride with formation of ethylic sulphide according to the equation—

$$\mathrm{S_2O_2Cl_2 + 3ZnC_4H_5 = S_2\begin{cases} C_4H_5 \\ C_4H_5 \end{cases} + 2ZnCl + ZnO + C_4H_5O}$$

An experiment in which sulphurous chloride and benzol were made to act upon each other with a view of obtaining phenylsulphurous acid, was unsuccessful.—(*Ann. Chem. Pharm.* cxliii. 263.

Double-chlorides of Platinum.—K. Birnbaum. Plumbic chloride dissolves readily in a concentrated neutral solution of platinic chloride. On evaporation crystals of plumbo-platinic chloride $Pb\,Pt\,Cl_6 + 4H_2O$ separate. An ammoniacal solution of argentic chloride added to ammoniacal platinic chloride, causes the formation of a yellow crystalline precipitate, which after desiccation over sulphuric acid had the composition—

$$2NH_3 + 2AgCl + PtCl_4 + H_2O.$$

No definite compound could be obtained with mercuric chloride.—(*Zeitschr. f. Chem.* N.F. iii. 520).

CORRESPONDENCE.

WATER ANALYSIS.

To the Editor of the Chemical News.

SIR,—As is mentioned in your report of the proceedings at the last meeting of the Chemical Society, I asked Dr. Frankland whether the comparative experiments on our method of water analysis as contrasted with his own method had been carried out on natural or on artificial waters. Your report, however, omits to give his reply.

It was, that *natural*—not artificial waters had been used. Inasmuch, therefore, as the amount of nitrogenous organic matter present in these natural waters is an unknown quantity, the fact that Dr. Frankland's numbers are not parallel with our own, leaves the question of correctness entirely untouched.

Had artificial waters (*i.e.*, waters into which known quantities of organic matter had been put) been taken, the contrast would have borne a different construction.—I am, &c.,

J. ALFRED WANKLYN.

London Institution, January 25th, 1868.

To the Editor of the Chemical News.

SIR,—In reference to the report of the last meeting of the Chemical Society, which appeared in the CHEMICAL NEWS of the 24th instant, I beg to state that I did not say that in employing Messrs. Wanklyn, Chapman, and Smith's method I calculated the nitrogen as albumen, or used their process as a means of control.

What I did say was in effect that the process was found to give valuable information as to the character of waters, and that the results were in accordance with their known history.—I am, &c.,

WILLIAM THORP, JUN.

THE LATE LUNAR ECLIPSE.

To the Editor of the Chemical News.

SIR,—In a recent number of your valuable journal you inserted a notice of the *Proceedings* of the Manchester Philosophical Society. Among other matters, this contained a paper by Mr. A. Brothers, on the late lunar eclipse. In his paper Mr. Brothers expresses his surprise that I had stated in a letter I sent to the *Astronomical Register* that I saw no colour on the darkened part of the limb of the moon, while he himself distinctly saw colour with a refracting telescope.

In a paper I have read at the Royal Astronomical Society I have furnished an explanation of these apparently contradictory observations.

As this explanation can scarcely fail to be of interest to many of your readers I should feel greatly obliged by your inserting the paper in your journal.*—I am, &c.,

JOHN BROWNING.

Upper Holloway, Jan. 1st, 1868.

* See page 55.

OZONE.

To the Editor of the Chemical News.

SIR,—The following is an account of the development of atmospheric ozone in October, November, and December.

In October there were large amounts of ozone on the 2nd, aft. of 16th, and morn. of 17th; considerable amounts on 29th and 30th; very little on 5th, 9th, 11th, morn. of 12th, 19th, and morn. of 28th.; no ozone on aft. of 1st, aft of 10th, aft. of 12th, 13th, aft. of 15th, aft. of 18th, 20th, 22nd, morn. of 23rd, and aft. of 25th.

In November, large amounts on 16th, 17th, and 18th; considerable quantities on 13th and 14th; very little on aft. of 1st., morn. of 9th, 11th, 15th, 21st, 23rd, 24th, 26th, and 27th. No ozone on aft. 2nd, 3rd, aft. of 7th, 8th, aft. of 9th, 19th, 20th, 22nd, 25th, and 28th.

In December, large quantities on 15th and 17th; considerable amounts on 3rd and 9th; very little on 5th, 6th, 11th, 12th, 14th, 23rd, 25th, 30th, and 31st. No ozone on 4th, 8th, 10th, 20th, 26th—29th.

During November and December the development of ozone has been very scanty.

During the past year the greatest development of ozone in January occurred on the 5th, 6th, 7th, 20th, 22nd, 23rd, and 29th; in February, on the 5th, 6th, 16th, and 26th: in March on the 25th, morn of 27th, and aft. of 30th; in April, on the 4th, aft. of 8th, morn. of 9th, aft. of 10th, and morn. of 11th; in May, on the morn. of 14th, 15th, aft. of 21st and 25th; in June, on 5th, 6th, 7th, aft. of 8th, 24th, aft. of 25th, 26th, aft. of 27th and aft. of 28th; in July, on aft. of 3rd, 14th, 15th, and morn. of 16th; in August, on aft. of 5th, morn. of 6th, morn. of 7th, 12th, 13th, 17th, morn. of 18th, and aft. of 20th; in September, on aft. of 2nd, 4th, 5th, 6th, morn. of 7th, aft. of 14th, aft. of 17th and 18th; in October, on 2nd, aft. of 7th, aft. of 16th, morn. of 17th, and 29th; in November, on 13th, 14th, 16th, 17th, and 18th; in December, on 3rd, 9th, 15th, and 17th.

I should state that in almost every instance the test-papers were freshly prepared immediately before exposing them to the atmosphere,—I am, &c.,

R. C. C. LIPPINCOTT.

Bournemouth.

MISCELLANEOUS.

Dr. Jelf.—In consequence of the meditated retirement of Rev. Dr. Jelf from the Principalship of King's College, London, a subscription is being organised by his admirers, including past and present students of all departments of the College, for the purpose of presenting him with a testimonial, which we hope will be worthy of the dignity of this vast institution in its extent and aims. It is computed that between 10,000 and 20,000 living men, mostly engaged in professions, have here received their education. Subscriptions will be received by the Hon. Treasurer, Henry Worms, Esq., Captain of the King's College Rifle Volunteer Corps, 15, St. George's Place, S.W.; or at the London and Westminster Bank.

The Influence of Chemical Knowledge on Sugar Manufacture.—*The Produce Markets Review* says:—Of all countries England is the most interested in sugar, not only as the greatest consumer, but as owner of some of the richest producing countries in the world, yet no nation displays greater ignorance or apathy with regard to this subject. Like the Lotos eaters, we are content to listen to the distant waves of progress, confident that the protective system of sugar duties will keep the boundaries of our fool's paradise inviolate. But the old proverb, "Where ignorance is bliss 'tis folly to be wise," has certainly no application to commercial matters, for the country that remains in ignorance, whether it be from choice or from indifference, is sure to fall into the rear. In no part of the world is scientific knowledge on mechanical

subjects turned to such practical account as in England, and many of our greatest men have made science the handmaid of commerce by applying scientific discoveries to the purposes of every-day life. The telegraph, and more recently the aniline dyes, and Bessemer's iron-working process, are a few instances among many; but sugar, of which the manufacture is completely a chemical process, is entirely overlooked by our *savans*—and yet there is a wide and almost unlimited field for chemical science in perfecting sugar manufacture, which has hardly advanced from its barbarous infancy of crushing mills, windmills, and open pans. The problem of sugar making, which has yet to be solved, is this:—To extract all the saccharine matter as it exists in the cells—that is in a pure condition, and white in colour—without extracting the injurious salts or acids which co-exist side by side with the sugar, and to do this at as small an expense as possible. A problem scarcely less important is the power of detecting by chemical analysis the exact proportion of extractable saccharine matter in any sample of sugar, for it must be observed that the per-centage of extractable saccharine matter is a very different thing from the saccharine strength shown by the polarising saccharometer. We do not hesitate to say, that any chemist who would solve these two problems would render a service to the sugar world of similar importance to that rendered to the world at large by the discovery of the steam engine. While our English chemists are mute upon the subject, the ablest chemists of France and Germany have for the last eighty years been employed in solving the delicate problem of the crystallisation of sugar, and the result of their labours, so far, may be seen in the vast continential beet-sugar crops, which are entirely due to the labours of a generation of chemists which has hardly yet passed away."

CONTEMPORARY SCIENTIFIC PRESS.

Under this heading it is intended to give the titles of all the chemical papers which are published in the principal scientific periodicals of the Continent. Articles which are merely reprints or abstracts of papers already noticed will be omitted. Abstracts of the more important papers here announced will appear in future numbers of the "Chemical News.")

Bulletin de la Société Industrielle de Mulhouse.
September, 1867.

J. KOLB, "On the Absorption of Carbonic Acid by some Oxides." JUNDT, "Report on Braun's Carbon Process for obtaining Photographie Facsimiles of Sketches and Cartoons." A. DOLLFUS, "On Cadmium and its Industrial Uses." DOLLFUS, "On the Use of Cadmium as a Substitute for Bismuth in the Metal Plates for printing Fabrics." G. SCHAFFER, "Report on Pernod's new Extract of Garancine." KUHLMANN, "On some Methods of Fixing the Gases of Stables and of using them as Manure." SACC, "On the Use of Benzine for Extracting the Colouring Matter of Garancine and other Dyes." "On the Use of German Silver for the Manufacture of Watch Cases in Switzerland." KUHLMANN, "On the Action of Water on Lead."

Génie Industriel.
September, 1867.

DUPUY and TURPIN, "An Improved Retort for Distilling Resins, and for Preparing the Oil so obtained for Lubricating Purposes."
October.

D. SAVALLE and Co., "Apparatus for Distilling and Rectifying Alcohol." DE LAPPARENT, "Note on a New Process for Charring Timber." WEINBERGER and LAFFOURCADE, "A Portable Apparatus for the Manufacture of Alcohol from Refuse Grapes." A. E. RUDBERG, "An Improved Method of Manufacturing Nitroglycerine and of Exploding the same in Blasting Operations." CHOLET, "A Solder for Aluminium Bronze."

Comptes Rendus.
October 28, 1867.

PAYEN, "On the Use of Osmose in the Manufacture of Sugar." E. CHEVREUL, "A Comparative Examination into the relative Facility with which French and Japanese Silks take the Dye." A. POEY, "Remarks on the Ozonoscopic Colourations obtained with the Jame Test, and on Berigny's Ozonometric Scale." LE VERRIER, on the same subject. CHEVREUL, "Observations on the Discrimination of Colour *apropos* of Poëy's Paper."

Sitzungsberichte der Kaiserlichen Akademie der Wissenschaften zu Wien. (Mathematisch-Natur-wissenschaftliche Classe.)
May, 1867.

A. F. REIBENSCHUH, "On Crystallised Ankerite from Erzberg, Upper Styria." F. ULLIK, "Researches on Molybdic Acid and its Salts." GOTTLIEB, "Analysis of the Emma Spring at Gleichenberg, Styria." W. F. GINTL, "On the Volumetric Estimation of Soluble Ferrocyanides and Ferricyanides by Means of Chamælcon Mineral." A. BRIO, "Researches on the Optical Properties of Oxalate of Ammonia, Bitartrate of Soda, and of Formiate of Copper and Strontia." E. BRUCKE, "On the Behaviour of some Albuminoid Substances towards Boracic Acid."
June-July.

A. LIELEGG, "On the Spectrum of the Flame of Bessemer Converters." F. ROCHLEDER, "On Æscigenine, and on some Allied Substances—Caincine and Chinovine." H. ALLEMANN, "Chemical Analysis of the Waters of the Mineral Spring at Ebriach, Carinthia." J. WOLFF, "Chemical Analysis of the Mineral Spring at Sztojka, Transylvania." M. EROFEJEFF, "Researches on the Optical Properties of Sulphate of Iron." S. KONYA, "Chemical Analysis of the Spring at Baden, near Vienna." E. BRUCKE, "On the Structure of the Red Corpuscles of the Blood." F. ROCHLEDER, "On Saponine." S. MAYER, "On the Quantity of Fibrine separated from Blood during Coagulation." L. PFAUNDLER, "On the Capacity for Heat of the Hydrates of Sulphuric Acid." F. ULLIK, "On some Compounds of Tungstic Acid." H. ALLEMANN, "On the Chemical Composition of Maize Oil." W. BAXT, "On the Physiological Action of some Alkaloids contained in Opium." E. LUDWIG, "On the Presence of Triethylamine in Wine."

NOTES AND QUERIES.

Dyeing Black.—Could you kindly inform me of the best method for dyeing a good black for polishing, and the best means to dye the yarn through.—R. B. C.

Yellow Chromate of Lead.—Can any of your correspondents give me a process for preserving, with its original lemon tint, yellow chromate of lead in paste. The method of precipitating with excess of sulphate of lead is unsatisfactory.—A. C. B.

Oxychloride of Magnesia.—I am engaged in a business connected with the use and preparation of cements in the building trade, and would thank any of your numerous scientific friends to inform me where I might find cheaply a crude carbonate of magnesia suitable for making oxychloride of magnesia for siliceous cementation, and a few hints as to the best mode of its preparation and use.—J. E. HAMILTON.

Free Sulphuric Acid.—A corespondent in your last issue desires a process for the estimation of "Free Sulphuric Acid in Superphosphates." I have much pleasure in assisting him. The following is a method which I have frequently tried, and is satisfactory for the purpose. A water solution of the manure being made, evaporate slowly until a small quantity only is left: add about seven volumes of concentrated alcohol, and allow to settle in the cold for some hours. This precipitates all sulphates, and leaves in solution, besides phosphates, the free sulphuric acid. Filter, wash with alcohol, add a large amount of water to the solution, and carefully evaporate off the spirit, and estimate the acid by baric chloride, &c. The soluble phosphates do not in any way interfere.—R. CARTER MOFFAT, Ph. D., Glasgow.

Weights and Measures.—Now that foreign weights and measures are so much used in scientific books, it would be be a great advantage to those who like myself are not very conversant with the value of these figures, if one of your clever mathematical correspondents would calculate and publish in your columns some simple factors whereby kilogrammes could be converted into cwts. and tons, francs into shillings and pounds sterling, grammes into ounces, metres into feet and yards, &c., &c., by a simple process of multiplication.—IGNORAMUS.

TO CORRESPONDENTS.

Enquirer asks for the best work treating on the combustion of coal in furnaces.

Student.—See Dr. Miller's lectures on spectrum analysis, reported two years ago in this journal. No special book on this subject has yet been published in England. A very good one in Dutch was written some years ago by M. Dibbits, and reviewed in the CHEMICAL NEWS.

J. T.—Mix the crude paraffin with paraffin oil, benzol, or coal naphtha.

T. D., an Old Subscriber.—Toluidine is a regular article of commerce now; almost any aniline maker, or dealer in coal tar products will supply it. It is made by mixing toluol with nitro-sulphuric acid, and reducing the resulting nitro-toluol with sulphide of ammonium or iron and acetic acid.

Rusticus.—1. We believe Dr. Crum Brown first brought his system of graphic notation before the Royal Society of Edinburgh. 2. The researches are given in the Journal of the Chemical Society. They are not thought much of by chemists. 3. Not except through a member.

W. P. B.—A letter is waiting for you at our office. Please forward your address.

Communications have been received from J. Horsley (with enclosure); M. Jannsen; Dr. E. Röhrig (with enclosure); Messrs. Townsend and Adams, New York (with enclosures); W. Ercot Smith; F. A. Aramayo; O. Coke; J. Dalmeira; H. P. Dobson (with enclosure); Messrs. Johnson and Matthey; J. E. Hamilton; A. Hochstetter; E. Jones; J. Cliff; T. R. Fraser, M.D.; Dr. Sansom; T. W. Lovibond; H. Lowe; Dr. H. Sprengel; K. Rumball (with enclosure); Karl Hoffman; W. Wyatt (with enclosure); Captain W. A. Ross; Dr. F. C. Calvert, F.R.S.; D. Marples (with enclosure); W. Hall (with enclosure); J. A. Wanklyn; A. P. Price; W. Thorp, junior; W. E. Drysdale; Dr. Watts (with enclosure); W. J. Day; J. Muspratt and Sons (with enclosure); F. W. Hart; N. Earle; Professor Tyndall, F.R.S.; J. E. Thorpe; H. Bower; Dr. R. C. Moffat; G. Gore, F.R.S. (with enclosure); Rev. B. W. Gibsone, M.A. (with enclosure).

MEETINGS FOR THE WEEK.

MONDAY.—Royal Institution, 2. General Monthly Meeting.
—— Medical, 8.
—— Society of Arts, 8. Cantor Lectures. "On Food," by Dr. Letheby.
TUESDAY.—Royal Institution, 3. Professor Tyndall, "On the Discoveries of Faraday."
WEDNESDAY.—Society of Arts, 8.
—— Geological.
—— Pharmaceutical, 8.
THURSDAY.—Royal Institution 3. Professor Tyndall, "On the Discoveries of Faraday."
—— Royal, 8½.
—— Chemical, 8. "On Gas Analysis," by Dr. W. J. Russell.
—— Royal Society Club, 6.
FRIDAY.—Royal Institution, 8. Professor Huxley, "On Animals intermediate between Birds and Reptiles."
Geologists' Association, 8.
SATURDAY.—Royal Institution, 3. Professor Roscoe, "On the Non-Metallic Elements."

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Mr. Henry Matthews, F.C.S., is prepared to give Instruction in all branches of PRACTICAL CHEMISTRY, particularly in its Application to MEDICINE, AGRICULTURE, and COMMERCE.

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For Particulars and Prospectuses apply to Mr. Henry Matthews, at the Laboratory, 60, Gower-street, Bedford Square, W.C.

PARIS EXHIBITION TWO GOLD MEDALS.

Liebig's Company's Extract of Meat, as distinguished from "Liebig's Extract of Meat," which name is daily more used for all sorts of extracts. Warranted genuine and of perfect flavour by Baron Liebig, whose signature is on every genuine jar. Cheapest and purest stock for Soups, Entrees and Sauces, highly strengthening for Children and Invalids. 1 lb. 14s, ½-lb. 7s 6d, ¼-lb. 4s, 2-oz. 2s, equivalent to 1d. half-a-pint of best beef-tea. Retail, of all Italian Warehousemen, Chemists and Grocers. Wholesale, of Crosse and Blackwell; E. Lazenby and Sons; John Burgess and Son; Barron, Harveys, and Co.; Barclay and Sons; Burgoyne, Burbidges, and Squire; Wm. Edwards; M. E. Foster; Langtons, Scott, and Edden; S. Maw and Son; G. S. Pedler; T. and H. Smith and Co., London; Duncan, Flockhart, and Co.; John Mackay; H. C. Baildon, Edinburgh; Southall, Son, and Dymond, Birmingham; Wm. Smeeton, Leeds; Raimes and Co., and B. Westworth, Liverpool; Mottershead and Co., Manchester; W. Proctor and Son, Newcastle-upon-Tyne; all Wholesale Houses, and of Liebig's Extract of Meat Company (Limited), 43, Mark Lane, E.C.

Methylated Spirits.—David Smith Kidd, Licensed Maker, Commercial Street, Shoreditch, N.E. Also, FINISH, FUSEL OIL, and RECT. NAPHTHA.

CANDLES, GLYCERINE AND SOAP. A Gold Medal was awarded at the Paris Exhibition to Price's Patent Candle Company, Limited, for "Candles, Glycerine, and Soap"—the only one to any British Exhibitor for these three things combined. The chief Candles of the Company are their "BELMONTINE" and "PRICE'S PARAFFINE" for those who must have the extreme transparency of pure Paraffine; their GOLD MEDAL PALMITINE" and "SHERWOOD PALMITINE" for those who, while desiring candles of great beauty, require also steady brilliancy of light and freedom from smoke and smell; their good old-fashioned "BELMONT SPERM AND WAX," and "BEST," "No. 2," "No. 3," and "BATTERSEA" COMPOSITES for those who require only perfect burning without caring for transparency; and their "CHAMBER" Candles, hard, and of small diameter to avoid the dropping of grease when carried. Their new toilet soap, "PRICE'S SOLIDIFIED GLYCERINE" contains half its weight of their distilled Glycerine, and should be the one toilet soap in use, especially in winter, because of its admirable effects in preventing chapping of the hands and face. There ought also to be in every house one of the sealed bottles of their patent distilled Glycerine, known everywhere as "PRICE'S GLYCERINE," two or three drops of which, mixed with three or four times as much water, will in a day or two remove chapping and roughness of skin, whether of adults or children; and when this is effected, a single drop of the undiluted Glycerine applied once a day will prevent the recurrence of the chapping and roughness. Insist on having "Price's Glycerine" in the Company's own sealed bottles, quantities of cheap impure Glycerine being now sold in the shops because of the low rate at which the dealers can buy it in comparison with Price's. All the good medical authorities abroad as well as at home order "PRICE'S" as the one only Glycerine to be used.

"PRICE'S NEW PATENT NIGHT LIGHTS" for burning in the wide glasses, are believed to be the very best Night Lights made. "PRICE'S CHILD'S NIGHT LIGHTS" are known everywhere and are excellent for burning without a glass.

Water-glass, or Soluble Silicates of Soda and Potash, in large or small quantities, and either solid or in solution, at ROBERT RUMNEY'S, Ardwick Chemical Works, Manchester.

THE OXYGEN TREATMENT.
Three Great Exhibition Prizes;—
The Medal, London, 1862; Silver Medal and Hon. Men., Paris, 1867; Medal of "International Society for Aid to the Wounded in War."
CONDY'S PATENT FLUID,
For Remedial Purposes (under Government Medicine Stamp),
Is used in all the Principal Hospitals,
For Wounds, Sores, Ulcers, Burns, Sore Throats, Discharges, &c.
FROM MANY HUNDREDS OF MEDICAL REPORTS:—
"Almost in despair of saving my patient, I prescribed a gargle containing CONDY'S FLUID. The improvement was so rapid that it could be due to the gargle only."—On Diphtheria, by N. EVANS, M.D.; *Medical Times & Gazette, Oct. 27*, 1866.
Sold by J. Bell and Co., 338, Oxford Street, and most Chemists. Prices:—1s. 1½d., 2s. 9d., and 4s. 6d. Must be of crimson colour with Prize Medal on label, and Gov. Med. Stamp. Wholesale at the Chemical Works, Battersea, London.

MURRAY AND HEATH,
OPTICIANS, &c., TO HER MAJESTY,
69, JERMYN STREET, LONDON, S.W.
(Four doors from St. James's Street), Late of 43, Piccadilly.
MURRAY AND HEATH'S IMPROVED SEA-SIDE POCKET MICROSCOPE, with or without Tripod Stand.
The NEW FIVE GUINEA STUDENT'S MICROSCOPE, with Eye-piece and Two Acromatic Object-glasses.
BINOCULAR and other MICROSCOPES, MICRO-SPECTROSCOPES, &c., &c.
Catalogues on application or forwarded for three stamps.

Scholl's Patent Platinum Gas Light Perfecter.
—Extract from Report by Dr. Letheby:—
"The results have been very remarkable, for they show an average increase of 63 per cent on the illuminating power of the gas. I am of opinion, therefore, that the invention is of great practical value." Price One Shilling each for Fish-tail Burners. To be had retail of Gas-fitters and Ironmongers, and (wholesale only) of JOHN SCHOLL, 41 and 42, Berwick Street, Oxford Street, London, W. Terms on appcation. N.B.—A specimen sent free on receipt of 12 stamps.

Instruction in Practical Chemistry and Evening Classes for the Study of Chemistry, Botany, Materia Medica, &c.
TO PHARMACEUTICAL AND OTHER STUDENTS.

Mr. J. C. BRAITHWAITE, for thirteen years Principal Instructor in the Laboratories of the Pharmaceutical Society of Great Britain, and Demonstrator of Practical Pharmacy, Pharmaceutical Latin, &c., wishes to inform his old Pupils and others that he continues to devote his whole attention to Education. The Session 1867—1868 will commence on the 1st of October, when

Mr. BRAITHWAITE'S Laboratory, which has been enlarged during the recess, will be re-opened at 10 a.m. for Instruction in Practical Chemistry as applied to Pharmacy, Medicine, Analysis, &c. Pupils can enter at any period. Terms moderate.

THE CHEMICAL and TOXICOLOGICAL CLASS meets as usual every Monday and Thursday Evening, at 8 p.m., commencing October 3rd.

The LATIN CLASS for the reading of the Pharmacopœia, Physicians' Prescriptions, &c., every Tuesday and Friday Evening, at 8 p.m., commencing October 1st.

The BOTANICAL and MATERIA MEDICA CLASS, every Wednesday and Saturday Evening, at 8 p.m. The usual EXCURSIONS for the STUDY of PRACTICAL BOTANY will commence at 10 a.m., October 2nd.

Fee to either of the above Classes Half-a-Guinea per Month; to the Botanical Excursions only. Half-a-Guinea per Eight Lessons, payable in advance. Pupils can enter at any period.

Address, 54, Kentish Town Road, N.W.
Mr. Braithwaite receives a few Pupils to board in his house.

Mr. J. Tennant, Geologist, 149, Strand, London, W.C., can supply Elementary Collections of Minerals, Rocks, and Fossils, to illustrate the Works of Ansted, Lyell, Jukes, and others, on the following terms:—

100 Small Specimens, in Cabinet with Three Trays	£2	2	0
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More extensive Collections, either to illustrate Mineralogy or Geology, at 50 to 500 Guineas each, with every requisite to assist those commencing the study of these interesting branches of Science, a knowledge of which affords so much pleasure to the traveller in all parts of the world.

In the more expensive Collections some of the specimens are rare, and all more select.

THE CHEMICAL NEWS.

Vol. XVII. No. 427.

CRYSTALLOGRAPHY AND THE BLOWPIPE.

II.

By CAPTAIN W. A. ROSS, R.A.

As Professor Richter and several other operators have found some difficulty at first in blowing the vesicles described by me, I ask your permission to commence this paper with an explanation of their formation.

Method of Blowing Vesicles of Borax, Soda, or Phosphorus Salt.

a. Borax.—This being the most cohesive and least deliquescent of the three fluxes, requires no addition to enable a strong vesicle to be blown, which will last for weeks or months. The platinum wire should be twisted into a ring over one of the legs of a pair of the *round* pliers used by bird-cage makers; thering is then nearly perfect, and should have the diameter of a largish pin's head. The other end is then placed in a holder, the wire heated, and a bead of borax taken up, which should be *perfectly clear* on cooling. This bead is then heated again, and charged with the substance. If a silicate, the bead will be observed to become much less fluid, and to move heavily round under the influence of the O.F.* like a thick jelly. After a little practice the operator will find it the best way to hold the ring of the platinum wire nearly horizontal to the table, so that the greater part of the fluid bead hangs downward, because by blowing *upward* through this there is not only less chance of bursting the vesicle, but the colouring matter (if any) will accumulate better round the ring, where the borax is generally thickest. I always now use the *geblase*, or caoutchouc bellows,† with the aid of which, in heating the beads, I can easily blow thirty vesicles in a couple of hours, and could make them in one if the minerals or oxides were ready and powdered. The bead should be held in a strong O.F. or R.F., according to the condition in which the substance is required. It should be allowed to cool down to red heat, and then the jet of the blowpipe applied close to, but not touching it, and square to the ring of the wire. Siliceous vesicles (which are, in fact, glass) are easily made, but they require to be annealed by being held near the flame for a short time after, for if suddenly withdrawn, large pieces will crack out of them. As the strength of the blast from a mouth blowpipe does not vary much, the *size* of the vesicle is not under the operator's control, and can only be partially regulated by the quantity and density of matter in the bead. Two beads of apparently the same size and density will, however, sometimes give vesicles of different dimensions, in which case the smaller will always be found to have a greater quantity of the flux round the ring of the wire.

The bead may be charged with the substance until perfectly opaque; for however saturated it may be, the vesicle will always be blown out clear. Even cobalt and manganese only give faint coloured lines of blue and pink over the *vesicle*, however much the *bead* may have been charged, but the exposition of undissolved matter is so delicate in the former that what may have seemed merely a thick opaque solution in the latter, appears in the vesicle as a number of spots or particles of extraneous matter, some of which look formidably large under the microscope. The merest particle of reduced metal is so discernible in this way, that I have amused myself by holding a green bead of the oxide of copper in the R.F. until it was apparently quite clear, and then, blowing it into a vesicle, I invariably found, with a microscope, a particle of metallic copper. I have now a vesicle of chromate of iron two inches long by one wide, made from an "opaque" bead, covered in this way with spots of the undissolved ore.

The vesicles, as made, should be placed in a tray on cotton, and a record *immediately* written of each of them, numbering from the right. If this is omitted, or a vesicle is misplaced, its contents are forgotten, and the only resource is to shake off the re-heated bead, and make a new vesicle.

b. Soda and Phosphorus Salt vesicles are made in exactly the same way as those of borax; but a small proportion of silicic acid must be added, without which the soda vesicle cannot be blown at all, and, even then, both of them deliquescing, will not last more than a short time.

I now proceed to record a few observations on the borax vesicles, which, I think, will be found to be based on certain *fixed* principles.

1. A vesicle clouding over with an unctuous-looking white film within an hour or so of being made, and showing (under the microscope) small deliquescent drops outside, may be set down as containing an alkali in considerable proportion, combined with little or no silicic acid. (N.B. I hope to be able soon to distinguish between soda and potash, the former appearing to crystallise in flowers or leaflets, the latter in stars.)

2. A vesicle clouding over with a *dry* white film after a few hours, and not deliquescing at all, or not for several days, contains one of the alkaline earths. Of these, *baryta* may be at once recognised by the peculiar blue-white appearance of the new film, which has much the colour of a solution of sulphate of quinine.

3. These films, however slight they may appear at first, are evidently due to the aggregation of minute crystals, which are, in the first instance, not distinguishable by the most powerful pocket lens.

4. After the first day or two, an immense number of similar crystals generally cover the surface of the vesicle, those apparently containing metallic acid salts, having a curious resemblance to the annular rings of a section of exogenous wood, but in no two differing vesicles are these crystals exactly alike. Over these, after the lapse of another day or two, a fresh kind of crystal sometimes appears, smaller and much fewer in number than the first. If I might venture a surmise regarding this phenomenon, it would be that the *first* are crystals of the double borates of the metallic oxides, the *second*, some combination of the latter with CO_2 derived from the atmosphere.

5. A vesicle of *boro-silicate of soda* remains quite clear for several days, and as far as I know yet, does not crystallise.* It is therefore the best vehicle I know for the vesicular exhibition of crystals of substances contained in it.

6. Vesicles of *silicate of soda* deliquesce a few minutes after formation, and those of P. salt in a little longer time, but the most curious phenomena are those exhibited under the magnifying glass by vesicles of silicate of potash, which cloud over and deliquesce as soon as formed, the crystals, scarcely discernible, appearing like small white rings with a black centre; the deliquescent moisture at the edges shrivelling up the vesicle, and advancing on all sides *towards the centre of gravity* like a miniature wave. The crystals of soda silicate appear formed like small white flowers with four petals.

The above may, I think, be depended on as a ground work for careful examination, but when I came to attempt to reduce my observation to system, making sketches of

* Oxidating flame.—Initials will be used to save space.

† This ingenious and portable bellows is the invention of an American student at Freiberg. It is described and figured in Richter's last edition of Plattner's work.—Leipsic, 1867.

* This vesicle, made of a mixture of borax with one-third of silicic acid, eventually crystallised after a period of three weeks. The crystallisation is interesting as a type of SiO_2. I apply the term *grammate* to it from its similarity to a series of lines or letters.

the crystals as I proceeded, I found, that independently of requiring the pencil of a Redgrave or Millais to copy the beautiful forms examined, I might as well begin to write a perfectly new work on crystallography, every second page of which would require to contain elaborate illustrations! The field is immense, and requires many and careful observers, for although the whole effects are evidently due to the operation of definite laws—

"A mighty maze, but not without a plan,"

the clues cannot be followed successfully by one or even by few observers. Every metal with its salts appears like a kind of mineralogical kaleidoscope, throwing its crystallisations apparently at random into the most elegant shapes, each of which must be made to yield its atom of information as to the source of all. But when I proceeded to examine crystallised vesicles of the alkaline and earthy silicates, as *albite*, *adular*, *calcite*, *heavy spar*, &c., I could really, with little imaginative aid, fancy myself beholding scenes in fairy land. The beautiful snow crystals pictured in the Chemical News in illustration of Professor Tyndall's lecture, are tame compared to these. Given a candle, a powerful pocket lens, and these vesicles, and you have objects of exquisite beauty, before which the most brilliant gems in the fairest setting of silver or gold must "pale their ineffectual fires." Taking the platinum wire carefully in a pair of fixing pliers and holding the vesicle between your eye (applied to the magnifying glass) and the light, you behold the most delicate tracery of fronds, flowers, ferns, or winter trees, standing out in frosted silver against a flood of golden light. Sometimes the appearance is that of a Cashmere shawl elaborately worked in silver (*calcite*), but the mineral *cerite* seems to afford forms even more exquisitely beautiful than these. I cannot attempt to describe the appearance of the *cerite* crystals, unless sprigs of the maiden hair fern elegantly posed together and covered with frosted silver on a ground of clear glass, can afford some idea of them.

It may be said these crystals are possibly pretty, but what is the use of them? I answer, that if friendly *collaborateurs* will assist me, I hope to turn them to a very distinct use. Already I can distinguish by means of them with tolerable certainty, an alkali or alkaline earth, isolating one—baryta,—and this you will recollect is at present the weakest part of blowpipe analysis. If I had space, I should much like to inform you of some remarkable vesicular reactions afforded by *molybdenite*, which, with the result of ulterior experiments, would appear to place that metal in close relation to the "earths," but I must reserve such remarks for another paper. In the meantime, why should not balls of fluid glass, containing substances in solution, be blown into globes of sufficient tenuity to favour crystallisation, and thus form permanent and beautiful models in illustration of one of the queens of earthly science—crystallography?

THE MICROSCOPE IN GEOLOGY.

By DAVID FORBES, F.R.S.

An interesting paper on this subject, by Mr. David Forbes, F.R.S., appeared in the *Popular Science Review* for October last; from it we condense the following. The original article is illustrated with numerous coloured diagrams of rock sections, as seen under the microscope.

The more searching and exact method of investigation now demanded by the advancing state of geological inquiry, necessitates that the student of that science shall in his researches avail himself of all possible means which the collateral sciences place at his disposal; and, amongst others, of those which can enable him to extend his powers of observation beyond the limits to which his unassisted eyesight can convey him.

The application of the microscope in geological inquiries is as yet, however, quite in its infancy, for with the exception of Sorby's invaluable memoirs on some special points of inquiry, literally nothing has as yet been made public which could even serve as an introductory guide to the geologist who might wish to commence the study of the subject.

In the present communication it is intended, as far as the space at disposal will allow, to attempt a short sketch of some of the results already obtained, in order thereby to illustrate the use of the microscope in similar inquiries.

When applying the microscope to the examination of rock structure and composition, it is necessary to prepare the specimens previously, in order to be enabled to make full use of transmitted light in their investigation.

When in sufficiently thin splinters or laminæ, by far the larger proportion of mineral compounds allow light to pass through them with more or less facility, and amongst these, most silicates, chlorides, fluorides, carbonates, sulphates, borates, and other salts; as well as many oxides, and some few sulphides, sulph-arsenides, &c. On the other hand, all native metals, alloys, and most of their combinations with sulphur, arsenic, antimony, &c., along with some few oxides, and other compounds, are opaque, even when in the thinnest laminæ, and consequently when present, as they often are, in minute quantity in rocks, although sometimes recognisable by their external crystalline form, are not to be distinguished by their optical properties, as in the case of those bodies which, as before-mentioned, are translucent.

When a mineral or rock under examination is entirely in the vitreous state, as, for example, obsidian, it appears when viewed under the microscope, merely as a more or less transparent or coloured glass, presenting, if perfectly in the vitreous condition, no evidence of crystalline or other structure, except, perhaps, traces of the striæ of viscid fusion. It is usually found, on inspection, however, that some part of the mass is sufficiently devitrified to allow of its structure and mineral composition being recognised. In some cases, when the glassy appearance presented to the eye would discourage any hopes of structure being discovered, the microscope proved the reverse most conclusively.

In many cases, however, where the specimens are so perfectly in the vitreous state as to show no trace of structure whatsoever, this may be developed by carefully acting upon the surface by gaseous or liquid hydrofluoric acid.

The rock sections may be prepared for the microscope as follows:—A fragment, from one-quarter to three-quarters of an inch square, and of convenient thickness, is chipped off the rock specimen in the direction of the required section, and ground down upon an iron or pewter plate in a lapidary's lathe with emery, until a perfectly flat surface is obtained. This surface is then worked down still finer by hand on a slab of black marble, with less coarse emery, then upon a Water of Ayr stone with water alone, and, lastly, finished by hand with water on a slab of black marble. This side of the rock is now cemented by Canada balsam on to a small piece of plate glass about 1½ inch square and ⅜ thick, which serves as a handle when grinding the other side on the emery plate as before; this grinding is continued until the section is so thin as to be in danger of breaking up from the roughness of the motion, upon which it is completed, by further grinding with emery by hand on marble, and finished first upon Water of Ayr stone with water, and afterwards upon black marble, as before described. The section is now removed from the plate-glass, and mounted in Canada balsam on a slide, covering its upper surface with a thin glass as usual.

The thickness to which such sections need be reduced is, of course, entirely dependent upon the transparency of the rock constituents, and is commonly from 1-100 to 1-1,000 of an inch.

Thin splinters of rocks and powdered fragments, mounted in Canada balsam, may also be examined with advantage, but cannot replace the above-described sections.

The examination of such a rock section enables a mineralogical analysis to be made, even of the most compact and apparently homogeneous rock, and generally leads to the discovery of other mineral constituents previously unsuspected, from their being invisible to the eye, and also, as Sorby has observed, allows those minerals, formed at the time of solidification of the rock, to be distinguished from such as are the products of subsequent alteration.

Arranging rock species according to their structure, it will be found that most rocks fall naturally into one or other of two great classes—

I. Primary or eruptive rocks;
II. Secondary or sedimentary rocks;

and it will be seen that the microscope is of special value when applied in cases where the external appearance renders it doubtful as to which of these classes a rock may pertain.

The terms *primary* and *secondary* are here used quite independently of geological chronology.

I. Primary or Eruptive Rocks.

This class includes rocks which have made their appearance in many, if not in all epochs, from the most ancient to the most recent, from the old granitic outbursts to the eruptions of the now active volcanoes; and if, as is now generally admitted, the earth be regarded as having been once a molten sphere, the consolidated original crust of the globe would pertain to this class of rocks.

Mineralogically they consist of crystallised silicates, with or without free quartz, and usually containing many other minerals in minor quantities, especially metallic compounds, as magnetite, titanoferrite, iron pyrites, &c., which last are frequently present in so minute a quantity as only to be detected by the microscope.

Whatever be their geological age, or from whatever part of the earth's surface they be taken, the microscopical inspection of such rocks shows immediately that they possess certain general and definite structural characters, distinguishing them at once from all other rocks.

The mineral constituents of such rocks are seen to be developed as more or less perfect crystals, at all angles to one another, thereby indicating that the entire mass must have been at one time in a state of liquidity or solution (aqueous or igneous), sufficient to allow of that freedom of motion absolutely essential to such an arrangement of the particles.

The microscopic examination already made of many hundred sections of eruptive rocks, differing widely in geological age and geographical distribution, shows that in all rocks of this class, whether of the most compact, hard, and homogeneous appearance, or occurring in the softest and finest powder, like the ashes and dust frequently thrown out by volcanoes; a similar crystallised arrangement and structure is present and common to them all. Lavas, trachytes, dolerites, diorites, porphyrites, syenites, granites, &c., all possess the same general structural features, serving to distinguish the eruptive rocks as a class from all others.

In the examination and discrimination of the minerals which compose these rocks, especially when close-grained, the microscope is quite indispensable, since without it no such inquiry could be attempted. In these examinations, the assistance of polarised light is most valuable; but the space, unfortunately, only allows of a mere mention of its application. In distinguishing dolerites from diorites, when fine-grained, as is often of considerable geological importance, the fibrous structure of the hornblende of the latter is generally so well developed, even when present in very minute quantity, as to distinguish it readily from the augite of the former, which possesses no such structure. Even in the case of Uralite, a mineral characteristic of certain porphyritic rocks, which has the external form of augite, although its chemical composition is that of hornblende, the fibrous structure characteristic of hornblende is distinctly visible. The microscopic structure of some minerals, however, varies with their origin; thus Sorby has shown that the structure of augite, and some other minerals in meteorites, is quite distinct from that of the same minerals occurrring in eruptive rocks, and demonstrates, in a very striking manner, how the study of such peculiaritics is likely to clear up the mystery in which the origin of these bodies is involved.

When, as is often the case, especially with translucent, colourless minerals like quartz, leucite, calcite, felspar, &c., the appearance presented under the microscope is alike, their optical properties and the use of polarised light afford the means of distinguishing between them with certainty; as, also, in the event of one substance being present under two forms, as calcite from aragonite, monoclinic from triclinic felspars, &c. In a similar manner, the structure, whether crystalline or vitreous, is determined, and valuable information obtained, elucidating the mode of formation and origin of the rocks themselves.

The alterations produced in eruptive rocks, subsequent to their solidification, by the action of water, atmospheric, or other agencies, are studied with advantage under the microscope.

Before proceeding to the next class of rocks, the discovery by Sorby of the numerous minute fluid cavities in the quartz of granites should be alluded to, as proving the great value of the microscope in the study of these rocks. The result of this gentleman's researches* proves that granites have solidified at a heat far below the fusing points of their constituent minerals, and at such a pressure as to enable it to entangle and retain a small amount ($\frac{1}{4}$ to $\frac{1}{2}$ per cent) of aqueous vapour, which naturally must have been present during its liquefaction. The presence of these fluid cavities in the quartz of granite was immediately blazoned forth as proof positive of the non-igneous origin of granite; whereas if Mr. Sorby's memoir had actually been read, it would have been seen that he had found fluid cavities, perfectly identical with those in granite, not only in the quartz of volcanic rocks, but also in the felspar and nepheline ejected from the crater of Vesuvius; and that the presence of fluid, vapour, gas, and stone cavities, are common both to the volcanic quartz-trachytes and to the oldest granites; and the inference drawn by Mr. Sorby from the results of his researches is that both these rocks were formed by identical agencies. He, therefore, classes them together under one head as rocks of similar origin.†

(To be continued.)

Amido-acids from Chlordracylic and Chlorsalylic Acid.—H. Hübner and R. Biedermann. Chlordracylic acid is converted into nitro-compound, and reduced by means of tin and chlorhydric acid to chloramidodracylic acid $C_7H_3Cl(NH_2)O(OH)$. The latter is treated with sodium amalgam, which removes the whole of the chlorine, being thus converted into amido-acid isomeric with amidodracylic acid. Similar experiments made with chlorsalylic acid have shown that chloramidosalylic acid is distinguished from chloramidodracylic acid, although they both have the same fusing point, *i.e.*, 212°.—(*Zeitschr. Chem.*, N.F. iii. 567.)

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* Quart. Jour. Geol. Soc. vol. xiv. pp. 453—500.

† These researches tend to confirm the theory of the igneous origin of granite and eruptive rocks in general. It must not be forgotten that by *igneous action*, as used by the Plutonist, was always understood *the action of heat as developed in volcanoes* (the study of which was the basis of the theory itself), in which the agency of water was always recognised. Nearly half a century ago, Scrope not only insisted on the important part played by water in volcanic action, but specially pointed out the difference between such volcanic fusion and ordinary melting. The term hydro-igneous action might not be inappropriate for such, but hydro-thermalism does not at all express what is intended. The idea of a true dry fusion in nature exists only in the brains of the ultra-Neptunist or lukewarm hydrothermalist.

ON

HEAT AND COLD;

A COURSE OF

SIX LECTURES*

(*Adapted to a Juvenile auditory*),

DELIVERED AT

THE ROYAL INSTITUTION OF GREAT BRITAIN,

(*CHRISTMAS*, 1867—8),

BY

JOHN TYNDALL, Esq., LL.D., F.R.S.

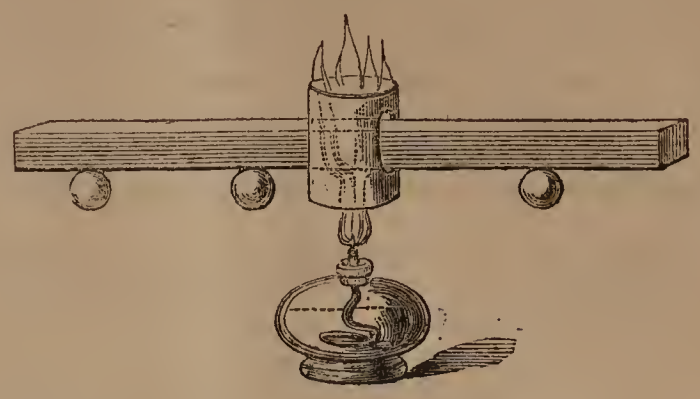
Lecture IV.

(Continued from page 54.)

Propagation of Heat.

I have now to say a few words upon another subject—the propagation of this thing we call heat—this curious quivering motion of the atoms of bodies; and in order to make this evident to you, I will, first of all, make an experiment or two on liquid bodies, or on gases. I want you to understand the manner in which heat distributes itself in gases, and, for that purpose, I have here placed a little piece of platinum wire—that metal which we raised to a bright white heat in our first lecture. It is a refractory metal, and bears a very large amount of heat. Now, we will have the room made dark, and Mr. Chapman will excite our electric lamp, and I will ask you to look at the shadow caused by this little platinum wire on the screen. I trust that even the most distant young philosopher now sees that shadow. We will heat the platinum wire by an electric current, and you will observe two things. You see, first of all, that the platinum wire gets longer—swags, sinks down—when I heat it. Observe also the air rising up from the surface of the heated wire. That wave-like motion is due to currents of heated air rising from the wire. The air when heated, rises in that way. The same is true of liquids: I have here a glass cell containing cold water, which will enable you to see this. I will place it in front of the lamp, and cast an image of it upon the screen. There is a means of warming this spiral of platinum wire within the water, and I want you to observe that the same thing occurs in water as you saw taking place with the air just now. Mr. Cottrell will now make the circuit for the electric current to pass; and then the moment the circuit is made you will find that the water will be heated by this spiral of platinum wire, and the heated particles of water will rise to the surface of the liquid. There, on the screen, you see the action of the hot wire upon the water, causing the water to rise in these *striæ*. The water goes up from the heated surface, and in time the heated particles will distribute themselves through the entire mass of the water. I make this experiment in order to fix upon your minds the difference between this action and another which resembles it at first sight. The action which I have shown you receives the name of *convection*, which I should like the elder boys to remember, and I want you to distinguish between this and another process, which is a very different one, and which is called *conduction*. In order to illustrate this subject of conduction, I have placed here before you an iron bar, and a copper bar (Fig. 6.), and I want to ask them which conducts heat best. Mr. Cottrell

FIG. 6.



will now light a lamp, and place it underneath the bars, so as to heat the ends of them at the same time; and as they become hot they will liberate these little balls, which are fixed on with wax; and I think you will find that the heat will travel along the copper better than along the iron. Here is a similar apparatus, with bits of tallow candle fixed to it. The greater the number of these pieces of candle that drop away from either bar, the farther and better the heat has travelled through that body. This is almost a better experiment than the more elaborate one, and it is one which you can make at home for yourselves. The copper will be able to melt away all its candles, while the iron will not be able to do so. The whole philosophy of the clothes you wear is, that they are bad conductors of heat. Your bodies are sources of heat. Through the burning up of the food you eat, within your bodies, warmth is produced; and the object of the woollen clothes which you wear at the present cold season of the year, is simply to prevent the passage of heat from the body to the air. For this reason we clothe the body with woollen cloth, that being one of the worst conductors of heat in nature. But the cloth has no warmth in itself; if I want to keep ice cool, as I did in a former lecture, I wrap my ice in flannel, which prevents the heat from without coming to the ice. Thus the woollen cloth simply prevents the transfer of heat in either direction, and hence the value of these non-conductors as articles of clothing.

The experiment with the pieces of candle sufficiently illustrates the fact that different materials differ in their power of conducting heat. I might also show you this in another way. If I warm this piece of iron by putting it into warm water, and then place it upon a cylinder of glass which stands on the face of the thermo-electric pile, that glass does not allow the heat to pass through to the pile, and the needle still remains on the side of cold. It would be a long time before the heat of this iron passed through the glass and reached the face of the pile. I will now remove the glass and place a cylinder of copper on the face of the pile, and then put the warm iron on the copper. I suppose that not more than two or three seconds will elapse before the heat will pass by the conduction of the copper to the face of the pile, and the moment it does so you will see that the needle will come to the other side of the middle line, showing heat. Now, in this case, instead of having the heat transferred, as in liquids or gases, by the passage of hot masses through the remaining bulk, we have a transmission of heat from atom to atom of the copper; and this process, as I have said, is called *conduction* of heat, in contradistinction to the other process, which is called *convection*.

And now I have to go on to another subject of a somewhat different character; but in passing I must say a word upon a very useful piece of apparatus, the safety lamp, which, unfortunately, is not always wisely used. I will state the problem which the inventor of this simple, but very wonderful apparatus placed before him. You must know that in our coal mines the miners are prevented from using a candle to light them while at their work, in consequence of the quantity of gas which is in the air of the mines. In former times they used to employ a flint and steel, and work by the feeble light

* Reported verbatim, by permission of the Author, for this journal.

of the sparks. The problem which Sir Humphry Davy, the inventor of the safety lamp, set before him was this:—"How can I give the miner light, and still preserve him from this explosive gas?" and he thought, "Can I put a light in any way within an apparatus so that, although the light shall shine through the apparatus, the gas outside will be prevented from exploding?" He found out that a flame could not pass through a piece of ordinary iron gauze. In fact, the flame is so much cooled by the wire gauze, in consequence of iron being a good conductor of heat and carrying the heat away from the flame, that the flame cannot get through. You see that when this

FIG. 7.

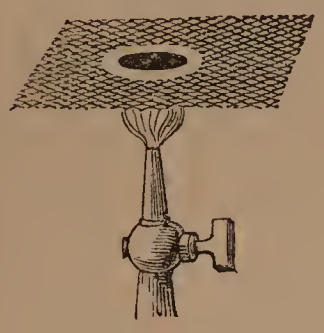
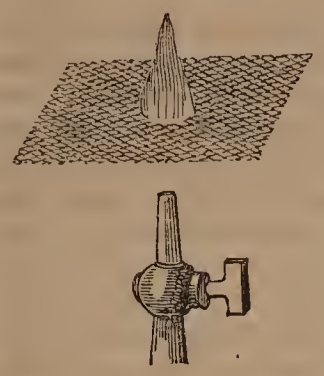


FIG. 8.



iron gauze (Fig. 7) is placed over the flame, the flame is entirely cut off, and cannot pass through; and if we light the gas above the gauze it will burn there, but the flame is prevented from reaching the gas below the gauze. (See Fig. 8.) Now, Sir Humphry Davy, when he made the miner's safety lamp, surrounded the candle wick or the oil wick with a wire gauze; and, although the light can pass through the meshes of the gauze, you might have an explosive mixture within and without the lamp, but the flame inside could not propagate itself to the gas outside, being unable to pass through the gauze.

I come now to another subject, and a very interesting one. I will ask Mr. Cottrell to heat a silver crucible, or dish, almost to redness; and supposing I then pour water into it, what do you think will occur? You might at first say, "Well, the water will be converted into steam." That is not quite the case. You will find when I pour the water into the vessel that the heat of the vessel produces such an amount of vapour from the water, that the water is supported upon a spring or elastic cushion of its own vapour, and is thrown into the form of a sphere, and the water rolls about in its own vapour. In order to show you this effect, we will cause a beam of light to fall right into the silver basin, and that beam of light will illuminate the drop of water which we pour into the basin. The image of the interior will be then thrown upon the screen. We now blow in a little water.

Now you see represented on the screen the globules of water rolling about—rolling about upon a cushion of their own vapour. Sometimes in this experiment we get a most beautiful figure produced by the water. We get a rosette form of globule. The vapour breaks away from the water in a kind of musical way. We will see if we cannot get the rosette form—a crimping of the edge of the drop of water. [After a few seconds the rosette form occurred. See Fig. 9]. When the basin is not very hot, at first these little crimpings arise, and then, when the vapour is not sufficiently strong to lift the water out of contact with the basin, the water will come into contact with the basin, and will suddenly boil. There it is. [At this moment the spherical form ceased, and the water boiled up and immediately disappeared with a hissing sound.]

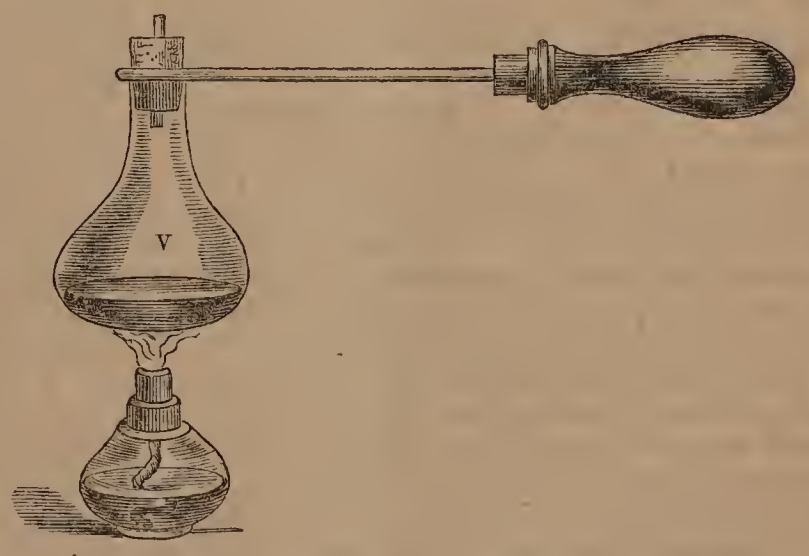
I must now send Mr. Cottrell down stairs to prepare something of very great interest and beauty; but as I do not know whether the experiment will succeed or not, I do not wish to raise your expectation. If, however, it succeeds, the experiment will be a very useful and a very important one.

In the meantime I want to show you what may occur in consequence of this spheroidal condition of water on a

FIG. 9.

hot surface. I have here a little copper boiler (Fig. 10). I will cork this boiler up, but I intend first of all to heat it very highly indeed, and then I will place a little drop of water into the boiler. I now heat the boiler, and Mr. Chapman will hand me some hot water, and when the boiler is heated I will pour a little into it, and that water will roll about as a spheroid. Vapour will be given off, but being small in amount, while the water is rolling about it will escape through a small hole in the cork. I will then withdraw the boiler from the source of heat, and the drop of water will then come into contact with the hot boiler; steam will be generated, and I think that that steam will be sufficient to expel the cork into the atmosphere. [The experiment was performed with the result anticipated.] There you see the steam drives out the cork the moment the water becomes changed into vapour by contact with the hot surface of the boiler. In this way we may have very serious explosions, but that is a subject into which I cannot go at present.

FIG. 10.



I want now to make an experiment or two which shall illustrate the character of a certain substance with which I am now going to operate. I have had occasion to mention gases several times in these lectures. Now, gases, and, in fact, the very air we breathe, are nothing more than the vapours of substances possessing very low boiling points. For instance, Mr. Faraday, to whom we are indebted for the very finest investigations upon this subject, succeeded in squeezing together the particles of the gas which is contained in this vessel, and forming it into a liquid; and there are other gases which have been liquefied by Mr. Faraday. One of them is a gas called carbonic acid, which we breathe out of our lungs. I want to generate a quantity of carbonic acid gas in this large round glass vessel. We have at the bottom of the vessel some

bicarbonate of soda, and I have here an acid. If I pour the acid into the vessel it attacks the bicarbonate of soda, and we get this carbonic acid gas liberated. I dare say we shall presently have accumulated enough for our purpose. [After an interval]—Now let me see whether the gas which has been liberated has not the power of putting out a candle. This will show whether the gas exists in this vessel or not. [A lighted taper was lowered into the vessel, and was immediately extinguished by the carbonic acid gas therein contained.] Yes: there is the gas. You see it is incompetent to support the combustion of the candle. The vessel is very nearly full. Now I will show you that this gas is very much heavier than ordinary air. I might ladle it out or dip it out in a bucket, and if I did so in front of the screen you would see it fall like water from a vessel, although under ordinary circumstances it is quite invisible. But I want to show you its heaviness by means of a soap bubble. I will blow a bubble from this clay pipe, and allow that bubble to fall upon this invisible gas. You will find that the bubble will float about upon the surface of the gas as if it were floating upon the surface of a visible liquid. [Successive soap bubbles were then produced, and on being detached from the tobacco pipe, were gently dropped on the surface of the carbonic acid gas, and, while floating there, were illuminated with electric light.]

Let me now tell you what I have sent Mr. Cottrell to do. Downstairs in the laboratory we have two very strong iron bottles, and these two bottles are filled with this carbonic acid. The gas in those bottles has been liquefied, and at the present moment he is turning a cock and allowing the liquid carbonic acid to turn into gas. What I want you to understand is that when the liquid carbonic acid turns into vapour it generates enormous cold, just as our vapour of water did on its production, only the cold generated by the carbonic acid is far greater. The consequence is, that when this liquid is turned into a gas and generates this cold, a portion of the vapour is turned into snow, and we thus obtain carbonic acid snow. I am almost afraid to speak to you about this matter, lest we should fail to get this wonderful substance. If I do get it I intend to put it into this vessel and make a few experiments with it which will both delight and surprise you. If we get the solid carbonic acid we shall be able to freeze water and produce ice in a crucible when it is actually heated to redness. First of all the carbonic acid snow is itself very cold, but in order to make it still colder I pour a little ether upon it. This turns it into a paste; and this mixture of carbonic acid and ether gives us nearly the greatest cold which has ever yet been produced. If we put that paste of carbonic acid and ether into the hot crucible, what occurs? The carbonic acid and the ether evaporate, and they so evaporate as to produce a protecting coating of vapour of carbonic acid between the red hot crucible and the pasty mass within it. In point of fact, the pasty mass does not touch the crucible at all. It remains intensely cold within the crucible. If we are successful in getting the solid carbonic acid, I shall dip this small brass sphere containing water into the mixture of ether and carbonic acid in the hot crucible; and I have no doubt that the water will freeze and will burst the brass sphere, and we shall then be able to take from the red hot crucible a sphere of solid ice. Mr. Cottrell is a long time bringing the solid carbonic acid. I am afraid he is not successful. Allow me simply to walk downstairs and see that the matter is going on rightly. [The lecturer then went in quest of the carbonic acid. On returning to the theatre he resumed as follows]—I am sorry to say that my worst anticipations have been realised. The experiment below has not succeeded. Here, however, is a little of this wonderful carbonic acid snow—solid carbonic acid. I will put a little in my mouth, and breathe against a candle. If I inhaled it I should kill myself; but I do not intend to inhale it. I intend simply to *exhale*. [The candle flame was then extinguished by the gas exhaled from the lecturer's mouth].

PROCEEDINGS OF SOCIETIES.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, January 7th, 1868.

EDWARD SCHUNCK, F.R.S., &c., President, in the Chair.

"*Variable Spot on the Moon's Surface*," by W. R. BIRT, F.R.A.S., communicated by J. BAXENDELL, F.R.A.S.

The interest attaching to the phenomena presented by the lunar spot Linné is my apology for communicating a few observations on another spot which exhibits similar phenomena. It will be seen that both spots manifest phenomena which appear to be referable to the presence of a *covering* by which the craters are at times *concealed*. We are not cognisant of any agency such as libration, angle of illumination, or variation of distance which affects the forms and appearances of lunar objects, being capable of rendering a crater *invisible* while its place is occupied by a white *cloud-like* spot of light; nor are any of these agencies capable of rendering an object on the moon's surface *indistinct* while others in its immediate neighbourhood are exceedingly sharp and well defined. With the hope of directing the attention of astronomers to this curious class of lunar objects, may I be permitted to lay the following observations before the Society? They have been made principally by the Rev. W. O. Williams, of Pwllheli, who has undertaken the examination of a zone on the moon's surface of 2° of latitude, viz. from 4° to 6° south.

The spot in question is marked IV Aα 17, IV Aζ 39 on the areas of the British Association Lunar Map IV Aα and IV Aζ, and is situated in 2° W. long., and 5° S. lat. It is also situated on the S.W. side of the ridge forming the N.E. boundary of Hipparchus, and has been described as a bright spot S.S.W. of IV Aα 7 (Beer and Mädler's Hipparchus F). Its diameter is 5″·94 and magnitude 0°·37, the diameter of Dionysius being regarded as unity. On De La Rue's photopraph 1858, February 22, it appears as a spot of about 4° of brightness. It is not so bright as Linné, which is about 5°. In this photograph it is seen to stand upon the east edge of a large *depression* running nearly S. by W.—N. by E. This edge, which forms a low ridge, connects the mountainous boundary of Hipparchus with the mountain IV Aζ 37. IV Aα 7, a bright spot smaller than IV Aα 17, IV Aζ 39, stands upon the west edge of this depression, which also meets the mountain IV Aζ 37.

On Rutherford's photograph 1865, March 6, this spot appears brighter that in De La Rue's, viz. 5°. Linné in this photograph is 6°. The observations that have been made of this spot are as under—

Year.	Date.	Authority.	Character.	Brightness.
1858	Feb. 22	De La Rue, Ph.	A bright spot.	4°
1865	Mar. 6	Rutherford, Ph.	A bright spot.	5°
1867	May 11 8½.....	Birt, Obs.	A shallow crater.	
1867	Oct. 7 8½ to 10	Williams, ,,	A very bright spot.	
1867	,, 17	Ingall, ,,	A faint shallow crater.	
1867	,, 17 13½....	Ingall, ,,	Drawn as a crater.	
1867	,, 17 13 to 15	Williams, ,,	A very conspicuous crater.*	
1867	,, 18 17 to 19	Williams, ,,	Crater very conspicuous, with a small central cone casting a shadow.	
1867	Nov. 5 9 to 10	Williams, ,,	Very bright, a streak of interior shadow on the west.	7°
1867	,, 6 8 to 10	Williams, ,,	A bright patch of light, streak of shadow scarcely discernible.	6°
1867	,, 15 18 to 20	Williams, ,,	Very bright.*	10°
1867	Dec. 5 6 to 8	Williams, ,,	A whitish spot, no trace of a crater.	5°
1867	,, 6 9 to 10	Williams, ,,	A whitish spot no crater.	5°

* On these occasions Mr. Williams saw a small bright point to the E., which he considered to be the *highest* point of the ridge.

Mr. Baxendell stated that on the night of the 3rd instant he had an opportunity of examining the spot referred to by Mr. Birt with Mr. Gladstone's equatorially mounted achromatic of 7¼ inches aperture, using powers from 60 to 250. It was then a well-marked though shallow crater, having a diameter about three-fourths of that of Beer and Mädler's Hipparchus F. The shadow of the western wall was very conspicuous on the floor of the crater.

Mr. Baxendell also read the following extract of a letter dated November 27th, 1867, which he had received from Mr. C. Ragoonatha Chary, the first native assistant at the Royal Observatory, Madras :—

"I have prepared the necessary calculations connected with the total solar eclipse to take place in the Indian Peninsula on the 18th of August, 1868, and these, with appropriate description and remarks on the eclipse by N. R. Pogson, Esq., are now in the press and will be published in the leading Madras Almanac. In these calculations I find that a slide-rule constructed for trigonometrical purposes may most advantageously be used even in such intricate cases as the solar eclipse. It saves more than three-fourths of the time and labour; and having calculated independently with the slide-rule as well as by means of logarithms for several places, I found the difference rarely to amount to half a minute in time, which is no great matter in predicting for amateurs, and even for intending observers. Mr. Woolhouse's method is followed, I believe, in the Nautical Almanac. The skeleton forms of this method, which are printed in great detail for logarithmic calculations, may be greatly simplified and facilitated by the use of a slide-rule accurately divided. The one I used was not very accurately divided, and was only two feet in length.

"*On the Examination of Water for Organic Matter*," Part II., by Dr. R. Angus Smith, F.R.S.

At present the conclusion only is given, as no abstract was prepared.

The following may be considered as a summary of the results required for sanitary purposes.

1. Quality of the organic matter, *i.e.* what is produced by standing under favourable circumstances for developing vegetable or other life ?

2 and 3. Condition of the organic matter. Products of decomposition. Easily decomposed organic matter. These two can be estimated for sanitary purposes sufficiently by permanganate of potash.

4. Nitrates as remnants of organic matter.

5. Nitrites as remnants of organic matter.

6. Chlorides as indicating animal sources.

7. Oxygen as indicating activity of decomposition or destruction.

8. Total organic matter and ammonia, by weighing and other methods.

PHIARMACEUTICAL SOCIETY.

Wednesday Evening, February 5th, 1868.

G. W. Sandford, Esq., President, in the Chair.

The minutes of the preceding meeting were read and confirmed. The thanks of the meeting were given for several donations to the library, and the President directed attention to a fine collection of drugs from North America, which had been presented to the Society by Mr. William Procter, jun., of Philadelphia, who is an honorary member of the Society.

Professor Bentley said the collection was a very interesting one, especially as American remedies had lately been brought so prominently under our notice. The specimens would be placed in the museum for examination.

Mr. H. S. Waddington read a valuable paper on "*Micro-Sublimation*," in which he gave the results of his experiments with a number of the alkaloids, such as santonine, salicine, narceine, papaverine, cinchonine, narcotine, strychnine, iodine, &c. Some very interesting slides were on the table illustrating the results of Mr. Waddington's researches, which were examined under the microscope by the members before and after the meeting.

The President, in thanking Mr. Waddington for his excellent paper, expressed his pleasure at seeing Dr. Guy present, who had devoted so much time and attention to the subject of sublimation.

Dr. Guy said the Society was under great obligations to Mr. Waddington for his paper, and referred to the beautiful specimens which he had seen before the commencement of the meeting. He had obtained some very fine ones himself, but only after thousands of experiments. He attached the greatest importance to the subject, and believed that greater results would be obtained by pursuing it still further.

Dr. Attfield made some remarks on the subject, and said that their warmest thanks were due to Dr. Guy and Mr. Waddington, for the fresh facts they had brought before them ; several bodies which were believed to be fixed, were now found to be volatile.

Professor Bentley read a paper contributed by Mr. Broughton, B.Sc., F.C.S., on a "*False Cinchona Bark of India*," at the conclusion of which

Dr. Attfield read a paper on the "*Preservation of Syrup of Iodide of Iron*," by Mr. T. B. Groves, F.C.S., who has for some time been engaged in devising means for preserving the syrup. He had found that it kept better when made with iron filings instead of pure iron in the form of wire, which he attributed to the presence of impurities in the filings. He had added dilute sulphuric, and phosphoric acids as preservative agents, and had obtained successful results with them. Mr. Groves prepared a number of specimens of the syrup, and to one he added 1 minim of dilute sulphuric acid to the oz.; to another, 2 minims of dilute phosphoric acid to the oz.; to a third 2 minims of dilute phosphoric and 1 minim of dilute sulphuric acid to the oz.; and to another specimen 8 drops of phosphoric acid. He had found that phosphoric acid was the only acid to be relied on, and it was very necessary not to add the acid before the syrup had cooled.

The President said that as Dr. Redwood had assisted in compiling the present Pharmacopœia, he would, perhaps, give them his opinion respecting the method proposed by Mr. Groves.

Dr. Redwood said that he was not at all prepared to admit there was any occasion to make the alteration ; the syrup of the British Pharmacopœia would keep for any reasonable time if properly prepared.

Mr. Ince greatly disapproved of such an addition, and thought it quite unnecessary. He had for a long time, before the Pharmacopœia was issued, made it according to that form with the most satisfactory results. The Pharmacopœia form was the same as that of the French Codex, which had always given good results.

Mr. Gale had adopted the form given in the present Pharmacopœia for ten years, and he had always found it successful ; the syrup would keep well for six months.

Mr. Wood, of New York, could not agree with all the remarks he had heard from Mr. Ince and Mr. Gale; he had found that if the syrup of iodide of iron was kept longer than three months, a layer was formed on the surface.

Mr. Umney had also found a layer on the surface after three months ; the syrup would keep very well for that time by putting it into bottles while hot.

Dr. Attfield then explained a simple mould for suppositories which had been forwarded by Mr. Laird, of Dundee. The idea suggested itself to him when witnessing the preparation of gelatine pastilles at Keiller's marmalade manufactory.

The President said that as the hour was late, the reading of the other papers must be deferred till the next meeting, which would be held on the 4th of March.

FOREIGN SCIENCE.

Paris, Feb 4, 1868.

Ozone and the cholera.—The nature of the troïlite.—Method of distinguishing the protosulphide of iron from the magnetic sulphide.—A new material for hats.—Ulmic and humic acids.—Reaction by which phenic acid may be distinguished from creosote.—Important products extracted from the olive, and from the Australian myrtle.—Academy of Sciences.—Respiration of cattle.—Study of a disease which attacks ruminants.—Production of nitrous gas during the fermentation of beet-root juice.—Niobium and tantalum.—On dissociation.—Phenomena intimately connected with muscular contraction.

During the autumn of last year, when the cholera was felt severely in Turin, Father Denza studied the meteorological condition of the atmosphere; he studied especially the connection between the prevalence of the disease and the absence of ozone. His observations were made at Moncalieri, rather more than half a mile from the town: the electricity was measured as well as the ozone. During the days in August and September, when the cholera was at about its height, the amount of ozone present was variable, but considerable—perhaps about the average. The electricity, however, during these days almost entirely disappeared; it is an interesting observation.

M. S. Meunier has recently published some facts concerning certain compounds occuring in meteorites, pyrrhotine Fe_7S_8, and troïlite. Troïlite has been considered by several mineralogists as a protosulphide of iron; the results M. Meunier has obtained in analysing many samples of troïlite, separated from meteoric iron, lead him to believe that the composition is much nearer that of magnetic pyrites. He indicates also a method of distinguishing these two substances so nearly alike in constitution, troïlite and pyrrhotine. The reaction consists in the precipitation of copper from its solutions by the one and not by the other. A number of experiments were made with artificial protosulphide and pyrrhotine; it was found that the protosulphide precipitated a solution of copper exactly like iron itself, while the magnetic sulphide gave place to no such reduction. The proto-sulphide obtained in the dry way exhibits the reaction even better than that obtained in the wet way, since the copper is not deposited in such fine particles. By melting iron and sulphur together, sulphides containing a little more sulphur than the protosulphide are obtained. In experimenting with these compounds as soon as the proportion of sulphur approached that of the magnetic sulphide, the precipitation ceased to be possible. Certain phosphides of iron, like the protosulphide, give rise to a precipitate. We may hope for further details.

Your correspondent hears, on good authority, that an entirely new kind of hat will be introduced in the summer. It will be made of paper in imitation of straw. The process of manufacture is curious, and probably quite new. A straw hat of the required size is covered with plumbago and electrotyped, the straw is burnt out of the mould, and manilla paper pulp pressed in. The invention is said to be that of an American. Many advantages, such as being waterproof and light, are claimed for the material.

M. Lefort has separated from among other substances contained in the trunks of old trees, an acid to which he gives the name xylic acid. This acid possesses the formula $C_{24}H_{14}O_{16} + HO$; it presents itself in the form of a vitreous black hard substance. Apparently this compound is the basis of all the compounds studied up to the present time, under the names of ulmic and humic acids.

M. Rust has made known a reaction by which phenic alcohol may be distinguished from the creosote separated from beech-wood tar. A mixture of 10 parts of collodion and 15 parts of phenic acid, forms a gelatinous mass, while the creosote from beech-wood tar mixed with collodion gives a clear solution.

M. de Luca, professor of chemistry to the Faculty of Science in the University of Naples, contributed at one of the meetings of the Société d'Encouragement, a memoir on some important products extracted from the olive and from the Australian myrtle. When the leaves of the olive are kept in strong alcohol they lose water, and at several points upon their surface radiated silky needle-shaped crystals make their appearance. If the leaves are treated with boiling alcohol, the liquid on cooling deposits the same crystalline matter; in this case, however, contaminated of course with all the other principles soluble in hot alcohol. The crystals have a faint sweet taste. The substance is not very soluble in alcohol, and its point of fusion is 164° to 165° C. Its composition is expressed by the formula $C_6H_7O_6$; the physical properties resemble those of mannite extracted from manna. The principle is present in the leaves during development, in small quantity, increasing with their growth; the amount diminishes at the flowering and when the leaves begin to lose their green tint. The process of extraction is easy; the leaves are macerated in water, and the liquid evaporated. The mannite does not undergo fermentation under the conditions, and is found in the residue. The flowers of the olive contain abundance of mannite; taken in the month of June and placed in alcohol, a solution is obtained, which when the winter arrives (by the fall of 10 or 15 degrees) deposits mannite. The juice obtained from the fruit of the Australian myrtle, by simple expression, is of a fine violet red colour, its taste is slightly acid and very agreeable. This juice, which contains glucose, cream of tartar, and free tartaric acid, undergoes fermentation at the ordinary temperature with disengagement of carbonic acid and production of alcohol. The wine of the myrtle, that is to say, the fermented juice, acquires in time a particular ethereal odour, very agreeable, and which constitutes to some extent a bouquet. By a further exposure to the atmosphere, and the aid of porous bodies, vinegar is easily obtained. There are many analogies between the juice of the myrtle fruit and that of the grape. The myrtle flourishes admirably in Australia in the open air.

At the meeting on the 27th January, M. Dumas thanked the Academy for the honour it had conferred upon him in making him perpetual secretary. The President announced the loss by death of M. Serres. M. Reiset communicated three memoirs, entitled—(1.) Chemical researches on the respiration of farm cattle, and the influence of dieting. (2.) Study of the gas produced during the meteorisation of ruminants; application to veterinary therapeutics. (3.) Note on the production of nitrous gas during the progress of fermentations in distilleries. Estimation of the proportions of ammonia contained in beet-root juice. M. Marignac communicated a research upon the reduction of niobium and tantalum, M. Debray contributed a memoir on "Researches on Dissociation." M. Des Cloizeaux sent a note "on the clinorhombic form, to which harmotome and Wœlerite ought to be referred, after the late researches on the dispersion of the value of their optic axes." M. Marey addressed a note on phenomena intimately connected with muscular contraction. M. Reiset used in making the experiments which form the subject of his first memoir, apparatus of such dimensions as to enable him to submit the exhalations of calves, full-grown sheep, &c., to examination. During the respiration of calves and sheep, he found a considerable quantity of proto-carburetted hydrogen in the gaseous mixture. This, too, is under the normal conditions. Calves in some experiments were fed upon milk only; deprived thus of vegetable food, the gaseous mixture exhaled resembled more nearly in its composition that exhaled by the carnivori. The production of carburetted hydrogen became absolutely *nil*. M. Reiset considers the formation of carburetted hydrogen in the stomachs of ruminants, when upon their natural food, to be a phenomenon of incomplete combustion. He deduces from these and former researches, the general conclusion, that the respiratory products depend much more upon the nature of the food than upon the species of the animal.

M. Reiset's second memoir referred to a disease which attacks cattle feeding on pasturage. The effects are rapid

swelling, and, finally, suffocation. He analysed the gas produced, that which in fact causes the swelling. He found it to be almost wholly, 74 per cent carbonic acid. Alkalies are therefore proposed as remedial agents. The third memoir relates to beet-root juice fermentation. As the manufacture of beet-root sugar is not an English industry, an abstract of this memoir would probably possess little interest for your readers.

M. Marignac communicated to the Academy the account of a number of experiments upon the reduction of niobium and tantalum. Fluoniobate of potash is reduced by heating with sodium in a wrought iron crucible; the product is, however, nioburet of sodium, which remains as a black powder disseminated in the fused mass. Water destroys the combination, nioburet of hydrogen being produced with some disengagement of hydrogen. Nioburet of hydrogen contains about 1 per cent of hydrogen, agreeing, therefore, with the formula NbH. It is a fine black powder. having a density varying from 6 to 6·6. By roasting it is promptly converted into niobic acid, entering into ignition, though the increase in weight reaches only 37 or 38 per cent, while theory requires 41. This hydride is not attacked by hydrochloric acid; it is very stable; heated to full redness for an hour in a current of hydrogen, it only loses ·1 per cent. An attempt was made to reduce fluoniobate of potash by magnesium; a violent detonation resulted. Similar treatment with aluminium in a black-lead crucible gives place to a compound of that metal and niobium, having for its formula $NbAl_3$, which is obtained upon treating the button of aluminium with hydrochloric acid. This is a lustrous crystalline compound. It is only oxidised very incompletely by roasting. M. Marignac has obtained an analogous compound of tantalum, $TaAl_3$, by heating the fluotantalate of potash with aluminium. It is also a lustrous crystalline powder scarcely attackable by hydrochloric acid, and only oxidised slightly by roasting. The general result of his researches M. Marignac considers to be a confirmation of the analogy that has been already observed between the metals niobium, tantalum, and silicium. He thinks these three metals, with zirconium and titanium, should be grouped together. The atomicity of these metals, he remarks, is not, however, the same; niobium and tantalum are pentatomic, while the others are tetratomic.

M. Marey's memoir upon phenomena, intimately connected with muscular contraction, was purely physiological.

M. Debray states in his memoir that a hydrated salt has for each temperature a tension of dissociation which is measured by the elastic force of the aqueous vapour which it emits at this temperature. Admitting this, the phenomena of efflorescence and hydration are easily explained. A salt effloresces when the tension of its water vapour is greater than that of the aqueous vapour existing in the atmosphere. A dry salt becomes hydrated when the tension of the aqueous vapour contained in the atmosphere is greater than that which the salt emits at the same temperature. Hydrous salts which do not effloresce owe, then, this property to the fact that the tension of the aqueous vapour emitted by them at ordinary temperatures is always inferior to that commonly possessed by the atmospheric aqueous vapour; these same salts effloresce when placed in an atmosphere where the elastic force of the aqueous vapour contained in the air is less than that which they emit.

Manufacture of Sugar.—The following modification in the process of refining sugar has been invented (and patented) by L. Pierre and R. Massey. The saccharine juice after being clarified in the usual way by means of lime and carbonic acid is precipitated at boiling temperature with caustic baryta (60 parts of the latter for every 100 of sugar), the precipitate suspended in water and decomposed with carbonic acid. A pure solution of sugar is thus obtained which only requires to be evaporated.—(*Zeitschr. Rübenz, Ind. Zollv.* 1867, 85, *and Zeitschr. Chem.* N.F. iii. 667).

NOTICES OF BOOKS.

Principles of Chemistry Founded on Modern Theories. By M. Naquet, Professor Agrégé a la Faculté de Médicine de Paris. Translated from the second Edition by William Cortis, Student, Guy's Hospital. Revised by Thomas Stevenson, M.D., Demonstrator of Practical Chemistry, Guy's Hospital. London: Henry Renshaw, 356, Strand.

One of the best marked features of a truly original mind, universally denoting genius in its possessor, is the power of showing its individuality at once, and stamping with a personal authority all that is produced by it. Every one acquainted with modern scientific literature must have convinced himself by negative evidence of this truth; but the chemist, with ample materials of this kind in the ordinary run of works on chemistry, has more opportunities of estimating positively the value of originality in a text book than most scientific teachers or students. M. Naquet shows his originality, clearness of expression, and facility of explanation in his very first sentences, and by his power of blending new facts and ideas with old facts and ideas expressed in a clear and definite manner, he manages to sustain the interest of the reader from the very first words to the last. This force of language, with no disparagement to the translator (for the work is translated as well and accurately as could be), is more noticeable, of course, in the original language.

There is a style in the language of the opening sentences that reminds us of Lamartine, Guizot, or Victor Hugo—clear, precise, forcible. Interest is at once excited, and, as in a well-written tale, never flags.

"Si nous jetons les yeux sur ce qui nous environne, nous sommes frappés par la vue d'une multitude d'objets d'une diversité infinie. Tous ces objets, quels qu'ils soient, ont reçu le nom générique de corps.

"Ce qui constitue les corps s'appelle matière ou substance. D'une manière générale on peut dire, que la matière est tout ce qui frappe nos sens, et d'une manière plus scientifique, que la matière est tout ce qui obéit aux lois de la gravitation."

The original is quoted here to show the easy way in which the French language adapts itself to the expression of scientific thought; and it will be easily seen from this one specimen (space forbids more) how much grace and ease is lost in translation. A translation (we are sorry to say) was necessary, and the work could not have been more ably done than it has been by Mr. Cortis. In considering M. Naquet's book as a chemical work, and not a translation only, a clue to its character is at once given by the opening sentence just quoted. It must be classed, then, as the production of a chemical physicist, rather than that of a chemical mathematician or chemical naturalist. Matter and its properties open the chapter, and so throughout the work. To a physicist a definite law is all supreme; for him there are no exceptions, everything has definition, and must suit that definition; his teaching is absolutely material; and granting that elementary mathematics formed the basis of physics, he, nevertheless, will not allow the abstract deductions of mathematics to hold an equal value with material facts. The majority of English chemists belong to neither of the above schools, but belong rather to a naturalist class, which recognises gradation with no sudden, sharply drawn boundary line. With them a sum of characters defines a position. To illustrate by an example of what is a metal and what a non-metallic body, or, as M. Naquet calls it, a metalloid. One person will argue that hydrogen is similar to silver, and that if silver is a metal hydrogen is a metal also to all intents and purposes. The naturalist will say that from the sum of characters the extreme members of the series are undoubtedly the one or the other; but that when we reach the mean members, we may as justly say silicon as silicium, arsenicum as arsenic. M. Naquet, as a physicist with his definite rules,

gives six characters as follows for an absolute determination :—

METALLOIDS.

I. Several metalloids are gaseous.
II. Metalloids have not the lustre called metallic.
III. Metalloids are bad conductors of heat and electricity.
IV. Metalloids have a density relatively low.
V. Oxides of metalloids, on combining with water, ordinarily produce acids, seldom bases.
VI. Metalloids are *always* electro-negative in the compounds which they form on uniting with metals.

METALS.

I. There is no gaseous metal.
II. Metals possess metallic lustre.
III. Metals are good conductors of electricity and heat.
IV. Metals have a density relatively high.
V. Oxide of metals, on combining with water, produce bases, seldom acids.
VI. Metals are *always* electric-positive in the compounds which they form on uniting with metalloids.

Thus the first and the sixth characters are given as absolute. It will be seen at once, however, that there is a very considerable element of relativeness in these absolute characters. If the gaseous state be a standard, mercury is more accurately a metalloid than carbon is; and electro-positive and electro-negative are surely relative terms, and, in a chemical classification as such, should not have undue weight. As the result of this arbitrary classification to the neglect of a sum of characters, tin, titanium, thorium, antimony, bismuth, uranium, tantalium, and niobium figure as metalloids. Carbon is not to be found in either list, we suppose from inadvertence; nevertheless, it might afford difficulties to the theoretical classifier. It surely would be better, in the face of these facts, to abolish the meaningless expressions, metals and metalloids, altogether, than do violence to our preconceived ideas with no satisfactory result.

The above is only an instance of a plan generally followed of harmonising ancient with modern ideas. Thus, in nomenclature when the old chemistry is done violence to, in accordance with modern ideas, for a given rule of four lines, we find four exceptions that require for a terse explanation as many pages of the volume. The consistency of the resulting nomenclature will at once be seen by glancing at the following list :—Mercuric nitrate, mercurous nitrate; mercuric chloride, protochloride of mercury (calomel); biniodide of mercury, protoiodide of mercury; proto-sulphide of mercury, subsulphide of mercury, mercuric sulphate, subsulphate of mercury, for the two sets of salts respectively.

We venture to say that the "safe middle course" of different systems of nomenclature will satisfy no English chemist, and that many modern English text books are in this respect decidedly superior to that of M. Naquet, both in consistency and simplicity.

But not to prolong our criticism further, we will freely accord to M. Naquet what is claimed for him by his translator—that he "explains and gives with great ability the arguments for and against his theories and those of other chemists." M. Naquet designed it for the classic text book of the French student of medicine; "the work only pretends to be a point of departure." "In our opinion students enter upon a false path when they neglect a knowledge of laws to gain *simply* an acquaintance with a number of facts, with which they uselessly overload the memory." Now, we must do the English medical student the justice to say that in very exceptional instances does he overload his memory with chemical facts; whether usefully or not we will not pretend to decide. It seems to us that the work is adapted for those who have a sound knowledge of chemistry, whether it be ancient or modern. In this case any bias will be avoided. In fine, the book before us is not solid enough for a foundation; but the thoughts suggested by every page are necessary for a completion of a chemical education. To go further, in one sense, this text book of chemistry is more than any other suited for the medical or chemical student's training; for its mastery he must use his brains and enlarge his ideas; but it should not be used too early in his career. We will add that, if the French medical student is accustomed to express himself as follows: "The coefficients representing the quantities of salts decomposed in two saline couples, containing the same radicles grouped in inverse order, are complementary;" he is infinitely superior to his ordinary British representative, who has not yet mastered the law of Volkmann, who stated that the frequency of the pulse in man, as connected with his stature, was in the ratio of the ninth root of the fifth power of the height.

This translation of M. Naquet's book has been conscientiously and accurately done. A great need felt for it among certain students will be amply satisfied; and as a further recommendation to a very well printed and got-up book, the casts of the original plates of M. Kekulé's diagrams have been used.

CORRESPONDENCE.

WATER ANALYSIS.

To the Editor of the Chemical News.

Sir,—Will you allow me to state, that after a careful consideration of the points in my report of the Chemical Society, alluded to in the letters which appeared in last week's Chemical News, I am convinced that the report was substantially accurate. With regard to the point raised by Mr. Thorp, discussion upon an error (granting such to be the case) affecting himself only, is now futile, since he has corrected what he considers to have been misinterpreted.—I am, &c.,

The Reporter.

IMPURITIES IN GLYCERIN.

To the Editor of the Chemical News.

Sir,—The writer of the article on Glycerin in *Kunst and Gewcberblatt* is correct in attributing the acrid, irritating properties of some glycerin to the mode of preparation; but I have seen distilled glycerin which was quite as unsuitable for medicinal or surgical purposes as any spoken of. The volatile fatty acids, and ethers, which exist in crude glycerin, are sometimes condensed with the glycerin, and these have very irritating properties.

In the glycerin which is made without distillation, the volatile acids, and ethers exist, but not in the same state as after distillation, the high heat required for this process decomposing them into some modification of their original state.

The great cause of irritation in glycerin which has not been properly prepared, is the presence of oxalic acid and of formic acid; these are produced by the action of sulphuric acid upon the glycerin, forming the first-mentioned acid, and this in its turn acts upon the glycerin, giving rise to formic acid, the irritating properties of which are well known.

The nitrate of silver test I have always considered the most reliable. Glycerin which shows no reaction with this salt, may be considered suitable for all uses, as it indicates not only the presence of chlorine or chlorides, but is, as well, reduced by acids which may exist in the glycerin.—I am, &c.,

Henry Bower.

Philadelphia, January 16th, 1868.

MISCELLANEOUS.

Faraday.—Several letters have recently appeared in the daily papers urging the propriety of continuing Faraday's pension to his widow. It has been thought, however, by many, and especially by those best able to judge, that our great philosopher, had he been alive, would have regarded anything of the kind with repugnance. This is borne out by the letter written by Dr. Bence Jones to the *Times* of the 30th ult. Dr. Bence Jones says that he has been requested by Mrs. Faraday to express her thanks for the interest the public are disposed to take in her behalf. The whole course of her husband's life was so marked by his love of retirement that she feels most keenly the intrusion of his name even, while she cannot but be grateful for the kindness which causes her so much pain. She wishes him to assure all those who value Mr. Faraday that the recognition that has already been made of his merits has given her more than she either requires or desired; and she is most anxious that his name should not be used in a way which he never would have approved.

Country Wells.—Dr. Attfield has written a letter to the *Times* on this subject. After alluding to the fact that wells are generally sunk where most liable to contamination, and often receive the contribution of sewers, he says that mineral matter dissolved from the soil is comparatively harmless; animal and vegetable matter may be harmless, but may be poisonous, and must, therefore, be kept out by every precaution. Good soil is here our best friend, Nature's own purifier, entirely destroying the substances last mentioned, if only allowed to have fair play; but its power for good is limited, its power for harm terrible, when saturated by drainage from adjacent accumulations of filth. Polluted water does not generally betray its condition till possessed of a strong odour; earlier intimation may, however, be obtained by the following tests:—Half fill a common water-bottle, cover its mouth with the hand, violently shake for a minute, and quickly apply the nose. If nothing unpleasant is detected, tightly cork the bottle, set it aside in a warm place at about the temperature of one's body for a couple or three days, and repeat the shaking, &c. Water of very bad quality may thus be recognised without the trouble and expense of analysis.

Famine in Eastern Prussia..—The *Berlin National Zeitung* writes as follows: The government district physician, Dr. Pinkus, of Insterburg, which town is in the centre of the country suffering from famine, appealed to the public to send among the many gifts of food above all Liebig Company's extract of meat. He says: already in several districts typhus appeared; great misery exists and greater misery must be expected. Even were money always at hand it would not be possible in many cases in distant villages and cottages to procure fresh meat for the patient, and still less good strong beef tea, the best and most indispensable of all medical comforts in such cases. Medical men in Germany who are in the habit of visiting the poor, find it very useful to carry with them a small jar of extract, so as to dispense beef tea at once where they find it necessary.

Glycerin.—The value of glycerin as a remedy for various skin affections is now generally known and admitted; it was therefore both natural and desirable that it should be presented to us in the solidified and therefore most convenient form of a soap. So numerous are the uses and purposes to which glycerin may be applied, especially in combination with other remedial substances, that glycerin compounds abound. Unfortunately, many of these so-called mixtures of glycerin are so in little more than name; they are either destitute of that substance, contain it only in minute quantities, or, when even present in larger amount, the quality is often by no means good. This observation applies with more or less force to many of the so-called glycerine soaps, perfumes, and cosmetics. In Price's solidified glycerin, however, we possess an article of really definite composition and of superior quality, and one on which we believe that the profession and the public may fully rely. It is stated of this glycerin compound that it wears well, gives a rich lather, and that it contains over half its weight of Price's distilled glycerin, the accuracy of which statement we verified by the follow per-centage analysis:—

Water	21·5
Fatty acids	29·5
Soda	3·7
Glycerin	45·3
	100·0

The Lancet.

NOTES AND QUERIES.

Waterproof Paste.—I understand that calico printers use a paste called "resist paste," which is waterproof, of the following proportions. 1lb. of binacetate of copper or distilled verdegris; 3lbs. of sulphate of copper, dissolved in one gallon of water; this solution to be thickened with 2lbs. gum senegal; 1 lb. british gum; 4lbs. pipe clay; 2oz. nitrate of copper, as a deliquescent. Can any of your readers kindly inform me if I can use it to stick lithographic bills on cards without detriment to the colours or the paper on which they are printed?—C. E.

Sprengel Air Pump.—Could any one give me about the dimensions for a Sprengel air pump, and the quantity of mercury required. I wish to use it for exhausting vacuum tubes for a 4-inch spark induction coil.—EIN ENGLANDER.

Sulphur in Pyrites.—Can any of your readers kindly inform me of the method usually adopted for ascertaining the amount of sulphur in iron or copper pyrites, to determine its value for sulphuric acid making? Is there not a practical method of assaying it?—T. W. W.

Estimation of Tannin in Gall-nuts, Sumac, &c.—Can any of your correspondents favour me with a good method for estimating the amount of tannin in either of the above, and also the percentage amount of tannin in the different qualities of sumac in the market?—ASTRINGENT.

TO CORRESPONDENTS.

Sutton.—Not yet published.

J. Horsley.—Received with thanks.

G. J. De Winton.—See below.

Astringent's letter shall be attended to.

J. T.—The odour is one of the properties of naphtha inseparable from it.

John C. Tellers.—Will our correspondent forward a few specimens with particulars as to cost of production, &c.? We shall then be enabled to form an opinion.

Swedish Portable Cooking Apparatus.—Will the maker of this apparatus please communicate address to our office?

Chemicus.—We have been quite unable to find out the name of the firm. The announcement was made on the authority of the late Dr. Richardson.

R. B. C.—A communication is waiting for you at our office. Please forward your address.

Ein Englander.—A solution of isinglass in weak spirit is the best cement with which to fasten on the sides of a hollow prism for bisulphide of carbon. Chloroform will be the best solvent for india-rubber to be used for coating a bladder. Zinc may be quickly amalgamated by dipping it into mercury containing a little sodium amalgam dissolved in it. Apply to Asher's, foreign bookseller.

H. P. M.—The piece has a similar composition to a Roman mirror, which according to an analysis by Professor Church, contained 70·29 per cent of copper, and 29·91 per cent of tin. The similarity, however, is not likely to be more than a coincidence.

Communications have been received from O. W. Parker (with enclosure); Rev. B. W. Gibsone, M.A. (with enclosure); J. Cliff; E. Dring (with enclosure); Professor Tyndall, F.R.S.; W. Scott; E. Hopwood (with enclosure); The Abbé F. Moigno; F. Montgomery; H. Octavius; J. Horsley (with enclosure); W. Valentin; G. J. De Winton; Mr. Sutton; R. Googan; W. Ackland (with enclosure); W. Isbister (with enclosure); J. Emerson; H. Hodgkinson; W. Richardson; J. Murray (with enclosure; T. Cobley; Alfred Payne; W. Miller and Sons (with enclosure); W. G. Drysdale (with enclosure); Rev. E. Smith, M.A.; G. A. Keyworth; Dr. Aloin Pflughaupt; R. McAlley; Rev. M. Kiernan; J. Wilkinson and Co.; W. Smith; F. Sutton; Dr. R. Angus Smith, F.R.S.; Mottershead and Co.; W. Poole Balfern; Rev. R. Kirwan; D. Shaw and Sons; Nicholson, Maule, and Co.; G. W. Eccles; T. W. Tobin (with enclosure); J. E. Thorpe (with enclosure); Rear Admiral Sir F. Nicolson; O. Grimet; Dr. Adriani (with enclosure).

Books received.—"El Correo Hispano-Americano;" "On the Magnetic Attraction of Cosmical Bodies," by John A. R. Newlands, F.C.S.; "Scientific American;" "American Artisan;" "Pharmaceutical Journal;" "Hardwicke's Science Gossip;" "Journal of Gas Lighting."

CONTEMPORARY SCIENTIFIC PRESS.

Under this heading it is intended to give the titles of all the chemical papers which are published in the principal scientific periodicals of the Continent. Articles which are merely reprints or abstracts of papers already noticed will be omitted. Abstracts of the more important papers here announced will appear in future numbers of the "Chemical News.")

Monatsbericht der Königlich Preussischen Akademie der Wissenschaften zu Berlin.

July, 1867.

G. ROSE, "On the Preparation of Crystallized Bodies before the Blowpipe." "On the Behaviour of Titanic Acid towards Borax, and on the Preparation of Rutile and Amorphous Titanic Acid. "On the Behaviour of Oxides of Iron towards Borax, and on the Preparation of Crystallised Hæmatite and Magnetic Iron Ore. "On the Behaviour of Titaniferous Iron Ore towards Borax, and on the Preparation of Crystallised Titaniferous Iron Ore, and Titaniferous Magnetic Oxide of Iron. "On the Preparation of Rutile by Fusing Titanic Acid and Titaniferous Iron Ore with Phosphate of Soda and Ammonia." A. OPPENHEIM, "New Researches on the Isomerism of Chloride of Allyl and Chlorinated Propylene."

Poggendorff's *Annalen.*

October, 1867.

A. WANGERIN, "On the Theory of Newton's Rings." W. WEYL, "On Tetramercurammonium and its Compounds."

No. 9. October.

R. RUHLMANN, "Researches on the Effect of Temperature on the Velocity of Light in Water." J. C. POGGENDORFF, "On the Development of Heat in the Path of the Electric Spark."

Annales des Sciences Naturelles (Zoologie).

Nos. 5-6. May-June, 1867.

RABUTEAU, "Experiments on the Physiological Action of Fluorides and of Metallic Compounds in general. (Review).

Annales de Chimie et de Physique.

September, 1867.

BERTHELOT, "On the Formation of Pyrogenous Bodies." "On the Reciprocal Action of Hydrocarbons." "Synthesis of Styrolene, Naphthalene, and Anthracene." "On the Polymers of Acetylene, and on the Synthesis of Benzine." "On the Theory of Polymers, and on the Aromatic Series." "On the Formation of Pyrogenous Bodies—Continuation." "On the Synthesis of Toluene, and on the different Principles contained in Coal Tar." On the same subject. "On some Thermo-chemical Conditions which determine the mutual Action of Hydrocarbons." V. DE LUYNES and G. ESPERANDIEU, "On the Preparation and Properties of Pyrogallic Acid." BERTHELOT, "On the Formation of Pyrogenous Bodies—Continuation. "On the Action of Heat on the Homologues of Benzine."

October.

BERTHELOT, "On the Formation of Pyrogenous Bodies—Continuation: On the Action of Heat on Retene." "On the simultaneous Formation of Homologous Bodies in Pyrogenous Reactions." "On the Oxidising Properties of the Homologues of Benzene." "On the Action of Potassium on Hydrocarbons." "On the Isomeric Conditions of Styrolene." "On the Characteristics of Benzene and Styrolene as compared with those of other Hydrocarbons." "On the Combinations of Picric Acid with Hydrocarbons, and on their Use in Analysis." "On the Melting Point of Waxy and Resinous Bodies." "On the different Carbides of Hydrogen contained in Coal Tar." A. DE LA RIVE, "On a Photometer for Measuring the Brightness of Distant Objects, and on the increased Transparency of the Atmosphere due to the Presence of Moisture." VAILLANT, "On the Transparency of the Atmosphere and its Signification."

Dingler's *Polytechnisches Journal.*

October, 1867.

T. GERLACH, "On the Specific Gravity of Aqueous Solutions of Crystallised Acetate of Lead." H. SWARZ, "A Method of Separating Magnesia from Lime." "On the Manufacture of Carbonic Acid and Magnesia from Magnesite." "On the Detection of Metallic Copper in Aventurine Glass." "On a Process for Smelting Tellurium Ores." "On some Points in the Preparation of Chromic Acid by Acting on Bichromate of Potash by Sulphuric Acid." M. ROSLER, "On the Manufacture of Salts of Tin." L. WALKHOFF, "On Dubrunfaut's Process for Freeing Molasses from Salts by Dialysis."

October.

G. T. GERLACH, "On Crystallized Chloride of Tin." H. VOHL, "On Naphthalin and its Applications." A. OTT, "A Method of Testing Commercial Phenic Acid." W. A. HERB, "On the Estimation of Vinegar, with special reference to an improved Apparatus for using Fleck's Volumetric Method."

MEETINGS FOR THE WEEK.

MONDAY.—Royal Geographical, 8½.
—— Medical, 8.
TUESDAY.—Royal Institution, 3. Professor Tyndall, "On the Discoverics of Faraday."
—— Photographic, 8.
WEDNESDAY.—Society of Arts, 8.
—— Microscopical, 8.
THURSDAY.—Royal Institution 3. Professor Tyndall, "On the Discoveries of Faraday."
—— Royal, 8½.
FRIDAY.—Royal Institution, 8. Professor Roscoe, "On Vanadium and its compounds."
—— Astronomical, 8.
SATURDAY.—Royal Institution, 3. Professor Roscoe, "On the Non Metallic Elements."

PARIS EXHIBITION TWO GOLD MEDALS.

Liebig's Company's Extract of Meat, as distinguished from "Liebig's Extract of Meat," which name is daily more used for all sorts of extracts. Warranted genuine and of perfect flavour by Baron Liebig, whose signature is on every genuine jar. Cheapest and purest stock for Soups, Entrees and Sauces, highly strengthening for Children and Invalids. 1 lb. 14s, ½-lb. 7s 6d, ¼-lb. 4s, 2-oz. 2s, equivalent to 1d. half-a-pint of best beef-tea. Retail, of all Italian Warehousemen, Chemists and Grocers. Wholesale, of Crosse and Blackwell; E. Lazenby and Sons; John Burgess and Son; Barron, Harveys, and Co.; Barclay and Sons; Burgoyne, Burbidges, and Squire; Wm. Edwards; M. E. Foster; Langtons, Scott, and Edden; S. Maw and Son; G. S. Pedler; T. and H. Smith and Co., London; Duncan, Flockhart, and Co.; John Mackay; H. C. Baildon, Edinburgh; Southall, Son, and Dymond, Birmingham; Wm. Smeeton, Leeds; Raimes and Co., and B. Westworth, Liverpool; Mottershead and Co., Manchester; W. Proctor and Son, Newcastle-upon-Tyne; all Wholesale Houses, and of Liebig's Extract of Meat Company (Limited), 43, Mark Lane, E.C.

THE OXYGEN TREATMENT.

Three Great Exhibition Prizes;—
The Medal, London, 1862; Silver Medal and Hon. Men., Paris, 1867; Medal of "International Society for Aid to the Wounded in War."

CONDY'S PATENT FLUID,

For Remedial Purposes (under Government Medicine Stamp),
Is used in all the Principal Hospitals,
For Wounds, Sores, Ulcers, Burns, Sore Throats, Discharges, &c.
FROM MANY HUNDREDS OF MEDICAL REPORTS:—
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Sold by J. Bell and Co., 338, Oxford Street, and most Chemists.
Prices:—1s. 1½d., 2s. 9d., and 4s. 6d. Must be of crimson colour with Prize Medal on label, and Gov. Med. Stamp. Wholesale at the Chemical Works, Battersea, London.

MURRAY AND HEATH,
OPTICIANS, &c., TO HER MAJESTY,
69, JERMYN STREET, LONDON, S.W.

(Four doors from St. James's Street), Late of 43, Piccadilly.
MURRAY AND HEATH'S IMPROVED SEA-SIDE POCKET MICROSCOPE, with or without Tripod Stand.
The NEW FIVE GUINEA STUDENT'S MICROSCOPE, with Eye-piece and Two Acromatic Object-glasses.
BINOCULAR and other MICROSCOPES, MICRO-SPECTROSCOPES, &c., &c.
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Mr. J. Tennant, Geologist, 149, Strand, London, W.C., can supply Elementary Collections of Minerals, Rocks, and Fossils, to illustrate the Works of Ansted, Lyell, Jukes, and others, on the following terms:—

100 Small Specimens, in Cabinet with Three Trays	£2	2	0
200 Specimens, larger, in Cabinet with Five Trays.....	5	5	0
300 Specimens, larger, in Cabinet with Eight Drawers ..	10	10	0
400 Specimens, larger, in Cabinet with Twelve Drawers..	21	0	0

More extensive Collections, either to illustrate Mineralogy or Geology, at 50 to 500 Guineas each, with every requisite to assist those commencing the study of these interesting branches of Science, a knowledge of which affords so much pleasure to the traveller in all parts of the world.
In the more expensive Collections some of the specimens are rare, and all more select.

CANDLES.—If you do not want your candles exclusively for show, but with pleasantness of appearance require excellence of burning, buy "PRICE'S GOLD MEDAL PALMITINE," or their "SHERWOOD PALMITINE," or their good old-fashioned "BELMONT SPERM," or "BELMONT WAX," or their "BEST," No. 2," ""No. 3," or "BATTERSEA" COMPOSITE," in preference to the finest, and most transparent Paraffine candles. But if you must have the extreme transparency of pure Paraffine, "PRICE'S PARAFFINE" or their "BELMONTINE" will give it to you in perfection, and at a more moderate price than is usually charged for any other really first-class Paraffine Candles.
The new toilet soap, "PRICE'S SOLIDIFIED GLYCERINE," containing half its weight of their concentrated distilled Glycerine, should be in general use in every house before the winter comes on, because of its admirable effects in preventing chapping of the hands and face. In every house there ought also to be one of the sealed bottles of their concentrated Distilled Glycerine, known everywhere as "PRICE'S GLYCERINE," and prescribed by the most eminent medical men abroad as well as at home, as the one only Glycerine for medicinal use whether externally or internally.
PRICE'S FANCY SOAPS of the different sorts usually made are excellent, and command a constantly increasing sale. The "Solidified Glycerine" spoken of above is, however, the one fancy soap to use.
"PRICE'S NEW PATENT NIGHT LIGHTS," for burning in the wide glasses are believed to be the very best Night Lights made. "PRICE'S CHILDS' NIGHT LIGHTS," for burning without glasses, and their different sorts of "CHAMBER CANDLES" are so well-known, and so generally appreciated as not to need any special notice here.

THE CHEMICAL NEWS.

Vol. XVII. No. 428.

NOTE ON THE DETECTION OF GASEOUS IMPURITIES IN OIL OF VITRIOL.

By ROBERT WARINGTON.

It is essential for some purposes that oil of vitriol should contain neither sulphurous acid nor any of the lower oxides of nitrogen; both of these impurities are met with in some commercial samples of oil of vitriol. After trying several methods for the detection of sulphurous acid when present in very small quantity, I at last resorted to the following plan, which also admits of testing at the same time for the presence of the nitric oxides.

About two pounds of the oil of vitriol are placed in a bottle, which the liquid half fills; the bottle is then stoppered, and violently shaken for a minute or two. The gases contained in the oil of vitriol are thus washed out by the atmospheric air contained in the bottle. Sulphurous acid is then tested for by introducing into the air space of the bottle a slip of paper coloured blue by iodine and starch; the paper is conveniently held in the bottle by means of a wire and a cork. The bleaching of the paper gives evidence of the presence of sulphurous acid.

The test-paper is best prepared from Swedish filter paper; this is first passed through a well made solution of starch, and then dried. A slip of this paper is next placed in a weak aqueous solution of iodine, where it remains till it has acquired a distinct blue colour. It is then removed, pressed between blotting-paper, and is now ready for use.

The paper thus prepared gradually loses its colour by exposure to air, it should therefore be used as soon as made: for the same reason its exposure to the gas in the bottle should not exceed two or three minutes; no perceptible change of colour will occur in this time if no sulphurous acid be present. The colour of the paper is also at once destroyed by heat, it cannot therefore be used for testing the gases given off by hot liquids.

The nitric oxides are detected by substituting for the first test-paper one imbued with iodide of potassium and starch. As NO forms NO_2 on contact with air, and N_2O_3 produces the same compound on contact with air and moisture, the presence of either of these three oxides will suffice to liberate iodine on the moist test-paper, and colour the starch. Since sulphurous acid destroys the blue iodide of starch, the presence of an excess of this gas will prevent the detection of the nitric oxides. The nitric oxides are, on the other hand, without effect on the test-paper employed for the sulphurous acid. If therefore the sulphurous acid is not in excess, it is quite possible to obtain the reactions of both gases from the same sample of oil of vitriol, and this is no uncommon occurrence with oil of vitriol which has been imperfectly boiled.

After employing for some time the method above described for detecting sulphurous acid, I made experiments with the reaction adopted by the chemists who lately examined the atmosphere of the Metropolitan Railway tunnels.* They employed a test-paper containing iodic acid and starch, on which sulphurous acid *produces* a blue colour by the reduction of the iodic acid. This test they mention as capable of detecting in air the presence of 1-100,000th part of sulphurous acid; a smaller quantity than this was perceptible by its smell, but had no effect upon the paper. On examining by this test several samples of oil of vitriol, I found that I could obtain no reaction; although the same samples, when treated as above described, had distinctly bleached the blue paper previously employed. On proceeding to other samples, in which the smell of sulphurous acid was quite perceptible, the iodic-acid-paper gave a distinct reaction. Thus the blue iodine paper was bleached when sulphurous acid could not be recognised by its smell;† the iodic acid produced a reaction only when the odour of sulphurous acid was distinct. The following equations, representing the reactions of these two tests, will throw some further light on their relative delicacy—

$$SO_2 + I_2 + 2(H_2O) = H_2SO_4 + 2(HI)$$

$$5(SO_2) + 2(HIO_3) + 4(H_2O) = 5(H_2SO_4) + I_2$$

From these equations it appears that to deoxidise iodic acid so as to liberate a molecule of iodine, requires the presence of five times as much sulphurous acid as is needed for the conversion of a molecule of iodine into hydriodic acid; or, in other words, the bleaching of the blue iodine paper will be effected by 1-5th the quantity of sulphurous acid required to produce a reaction with iodic acid. The first test is thus apparently five times more delicate than the second.

In using either of the reactions here described for the purposes of general testing, it is to be remembered that sulphuretted hydrogen produces with each the same effect as sulphurous acid.

* Chemical News, November 8, 1867, p. 239.

† The purest oil of vitriol I have met with, gives some slight odou to the air with which it is shaken; but this odour is not recognisable as sulphurous acid.

THE MICROSCOPE IN GEOLOGY.

By DAVID FORBES, F.R.S.

(Continued from page 63.)

II. Secondary or Sedimentary Rocks.

The rocks pertaining to this class are all, directly or indirectly, formed from the breaking up, or débris, of previously existing rocks. When found in the normal state of sedimentary deposition, they may be conveniently subdivided into:—

1. Rocks formed of the immediate products of the breaking-up of eruptive rocks.
2. Rocks built up of the more or less rounded or angular débris of previously existing sedimentary or eruptive rocks.
3. Rocks composed of mineral substances extracted from aqueous solution by crystallisation, precipitation, or the action of organic life.

1. *Rocks composed of the immediate products of the breaking up of eruptive rocks.*—The little attention paid by geologists in general to the study of rocks of this class, has introduced the elements of confusion into many of their inquiries, and frequently has led to very erroneous opinions being formed as to the nature and origin of certain rocks, which could never have been entertained had microscopic investigation gone hand in hand with field observation.

Rocks of this class may either be of subærial or subaqueous origin; in the former case, for example, volcanic ashes may have been deposited as beds on the surface of the land, and afterwards been covered by lava streams poured out over them; or, from having been depressed below the sea level, may have had sedimentary beds of aqueous origin subsequently superposed on them.

When of subaqueous origin, as is by far the most common case, subærial or subaqueous outbursts may force into the sea eruptive rocks, which, being at once broken up into a state of division, more or less fine, in proportion to the greater or lesser cooling power of the water mass in immediate contact, may be spread out into beds by the action of the waves; the texture of these rocks may vary from that of the coarsest breccia down to the finest mud, and, as is usually the case, such deposits

may present themselves as alternating beds of coarse and fine character. Upon the consolidation of such formations, rocks are formed, identical in chemical and mineralogical composition with the original eruptive rock from which they were derived, and which, particularly when close-grained, often present an external appearance so like the original rocks as to be frequently undistinguishable from them by the naked eye; in such deposits it is often easy to pick out specimens having all gradations in appearance from the above described down to such as would be attributed to the consolidation of mere detrital mud.

No wonder, therefore, if the field geologist finds himself bewildered under such circumstances, and inclined to settle down in the comfortable belief of the transmutation or transition of sedimentary rocks into eruptive, &c., and even the chemist feels puzzled, when he finds that a rock taken out of apparently normal stratified deposits has the same chemical composition with one of undoubtedly intrusive nature. The microscopic examination, however, soon shows that, however similar the external appearance of two such rocks might be, their internal structure is totally different; showing in the primary rock the crystallised structure and arrangement previously described, whilst the secondary rock is resolved into a mere agglomeration of more or less broken fragments of the same minerals constituting the former. In beds formed from the consolidation of volcanic ashes, the microscopic examination occasionally affords evidence as to whether such ashes had been deposited on land or had fallen into water.

2. *Rocks built up of the more or less rounded or angular débris of previously existing sedimentary or eruptive rocks.*—Where sufficiently coarse-grained, these rocks constitute ordinary conglomerates, breccias, grits, sandstones, &c., and are easily analysed by the eye; but if fine, as shales, slates, &c., the microscope must be appealed to, in order to resolve them into their constituent mineral or rock particles, and by this means it will be seen that even the most compact and homogeneous specimens are a mere aggregate of more or less rounded and water-worn grains of quartz, weathered felspar, mica, chlorite, soft and hard clays, clay slate, oxide of iron, iron pyrites, carbonate of lime, fragments of fossil organisms, &c., arranged without any trace of decided structure or crystallisation, even when the highest powers of the miscroscope are employed in their examination. The physical structure and optical properties of the mineral components enable them, however, to be recognised with great certainty, even when in grains of less than 1-1000th of an inch in diameter.

3. *Rocks composed of mineral substances extracted from aqueous solution by crystallisation, precipitation, or the action of organic life.*—Under this class are included most beds of gypsum, rock salt, and other saline bodies, as well as travertine, siliceous sinter, flint, infusorial slates and earths, limestones, &c., many of which have been as yet but very superficially examined.

In the microscopic investigation of such rocks as owe their origin to the development of organic life, very considerable progress has been made, with corresponding important and interesting results.

As early as 1836, Ehrenberg proved that large rock masses were built up of the carapaces of minute siliceous infusoriæ; and, more lately, Sorby has done good service by his investigation of limestones; these he has proved not to have originally possessed any crystalline structure whatsoever, but to have been deposited as mere mechanical aggregates (aptly termed by him, organic sands or clays) formed of the débris of calcareous organisms, which admit frequently, not only of being recognised, but of having their relative proportions determined. The comparison of the microscopic structure of the organisms in chalk, with those now forming in the depths of the Northern Atlantic Ocean, indicates that there is an immense deposit now in course of formation, quite analogous to what had previously taken place in the seas of the cretaceous period; and the same able observer has shown that the reason why certain calcareous organisms are found so well preserved, whilst others had disappeared or become entirely disintegrated, was from the carbonate of lime in the first being in the form of the stable calcite, whilst in the latter it was present as instable Arragonite.

When a calcareous rock has undergone cleavage, the microscope shows a distortion of its particles and organisms, just as in a cleaved slate, though in much less degree; the measurement of such distortion serves as a basis for estimating the amount of compression undergone.

With the exception of having briefly referred to the alterations in igneous rocks, subsequent to their solidification, and the cleavage of sedimentary beds, all the classes of rocks treated of have been considered in their normal or unaltered condition; it remains now to direct attention to the use of the microscope in the study of subsequent alteration or metamorphism of rocks.

Many sedimentary beds become more or less indurated, at points where they are cut through by eruptive dykes; thus the coal-shales and clays of Staffordshire are found altered into a hard rock with conchoidal fracture, or even into porcellanite, when in immediate contact with basaltic dykes. An examination shows no change in mineral or chemical composition beyond the expulsion of the water always contained in such beds, and sections of such rocks are often seen to be quite identical in structure with those of common stoneware made from the same clays, the only difference being that the latter is usually more porous from not having been submitted to the pressure which rocks baked *in situ* would experience.

The alteration of rocks produced by infiltration may or may not be accompanied by chemical changes; thus a section of calcareous grit often shows that the calcite filling up the interstices between the grains of sand has been merely deposited from a solution of carbonate of lime which has percolated through it, and in otherwise unaltered limestones it is common to find microsopic veins of calcspar, due to minute cracks or fissures, filled up in a similar manner. Frequently, however, such infiltration is accompanied by an entire change in the chemical composition of the rock itself; thus the beds of Cleveland ironstone have been proved by Sorby's microscopical researches to have been originally shell limestones converted into carbonate of iron by the action of ferruginous solutions, the fragments of the original shells being still distinguishable in all stages of conversion; in the same manner he has proved the magnesian limestones of the Carboniferous and Devonian ages, as well as the Permian dolomites, to have been originally common limestones, or aggregations of organic débris, the particles of which, by the use of the microscope, can be traced back to their original unaltered state, from which they have been changed by the action of magnesian solutions.

The metamorphism of rocks produced by gasolytic action, as, for example, carbonate into sulphate of lime, &c., has, as yet, not been made the subject of microscopical enquiry.

The foliated schists, quartzites, &c., form by themselves a disinct and well-defined class of metamorphic rocks, characterised by structural peculiarities differing from all previously treated of.

This appears to be due to their crystalline development having originated in a solid body, and not from liquefaction; the minerals composing them differ greatly in structure from the same minerals when found in eruptive rocks. Instead of, as in the latter case, presenting themselves in more or less defined crystals, occurring in all positions and at all angles to one another, in the foliated rocks they are developed only in one general direction, not characterised by well-defined bounding planes, but forming a string of drawn-out and irregularly bounded crystalline aggregations.

The microscopic examination of these rocks proves their original sedimentary origin, often showing the contours of the originals and grains, and, as Sorby has pointed out, the existence of ripple-drift and wave-structure, peculiar to sedimentary rocks alone. These rocks appear to have been micaceous and argillaceous sandstones, the constituents of which have been recrystallised *in situ*, owing to molecular action developed in the solid rock.

The quartz of these schists frequently contains numerous fluid cavities, indicating that they have been exposed to a pressure under which the water, always present in more or less quantity in sedimentary rocks, has been entangled and retained during the recrystallisation of the quartz.

The direction of the lines of foliation or crystalline development is that of the lines of least resistance in the rock, which commonly will be the lines of stratification, but in cleaved rocks will doubtless be those of cleavage. Sorby has alluded to this fact by the names of "stratification foliation" and "cleavage foliation."

In conclusion, the author hopes that it may be the means of attracting attention to the subject, and thereby of causing a hitherto almost unexplored field of microscopic enquiry to be more cultivated; and leaves it to his readers to form a correct estimate of the justness of the sneering assertion that "mountains should not be looked at through microscopes."

ON

HEAT AND COLD;

A COURSE OF

SIX LECTURES*

(*Adapted to a Juvenile auditory*),

DELIVERED AT

THE ROYAL INSTITUTION OF GREAT BRITAIN,

(*CHRISTMAS*, 1867—8),

BY

JOHN TYNDALL, Esq., LL.D., F.R.S.

Lecture V.

Radiant heat.—Reflection and absorption of radiant heat.

You know that towards the end of the last lecture I failed in the experiment of freezing water in a red hot crucible by means of carbonic acid snow. As I do not like failures in experiments I will try to make that good. I have here some of this beautiful carbonic acid snow, which I will now put in this red hot crucible. I will pour upon that a quantity of ether, and then I bring down into the middle of the mixture this hollow brass ball containing water. The ether is now boiling. I will put in some more of this carbonic acid snow. It burns my hand,—it is so enormously cold. This ball is very cold, and I have no doubt that already we have produced ice in it. The quantities of the substances are much smaller than I have been accustomed to work with, but I dare say we shall succeed notwithstanding all our difficulties. [After a short interval the water was found to be frozen.] There: look at that. The water in this spheroid is converted into ice, even in this red hot crucible!

I have here some mercury, and I will pour some of it into this basin. I dare say we shall be able to solidify this mercury by means of this beautiful carbonic acid snow. Now observe here what I think you have never seen before. You know the liquid metal mercury. You have it here made solid—frozen by the cold acid. This requires a far greater cold than will freeze water. I might beat this substance upon an anvil or cut it with a knife. It becomes liquid again in a moment. If I hold this solid frozen mercury in a vessel of water, the mercury will become liquid and fall, and each little drop of mercury which falls will produce a stalactite of ice. See, the frozen mercury is being melted by that water. This is really cold water, but it is hot to the frozen mercury, and a mass of ice is produced round about the mercury which has been cold enough to do that.

The best of men and the best of boys in the world, fall and fail; but when one falls the great question with him should be, "How long am I to remain down?." Every boy falls, but if he falls and fails, he ought to be up again and at it, doing his duty. And so, as we failed at the end of the last lecture in this experiment, five minutes had not elapsed before my assistant was down in the laboratory working the pump, determined to make good our failure, which we have here done.

We have now to pass on to another and very different portion of our subject. I have endeavoured to give you a kind of image, more or less perfect, of this thing that we call heat. I have endeavoured to give you a picture, as it were, which your minds should realise.

If you take a hot body and place it in the air, you find that it gradually cools. If it be red hot the glow first of all sinks, and by and by you see nothing of it. The thing gets cooler and cooler, and at the end becomes as cool as the surrounding air. Now, this heat, in the first instance, was a motion of the particles of the hot body. When the body cools it is simply giving up its motion. Now, to what does it give up this motion when you place it in the air? Well, you might say, to the air. True: and when I held the heated piece of iron in front of the screen you saw the hot particles of air streaming up into the air above; so no doubt the motion which the hot body gives up is given up to the air. But if you put the hot body in the middle of a place where air did not exist it would still cool. Now, I want you to exercise your imagination as to the manner in which this motion is disposed of, lost, or given out, when a body cools. I believe most of you understand how it is that sound travels through the air,—at least, how it is that the sound of my voice propagates itself through the air and makes every word I say audible, I trust, to you all. I have often looked into persons' throats when they were speaking, and observed cords or tendons there which are thrown into a state of vibration when we speak or sing. They cause the air to shiver, and those tremors are propagated through it, just as motion is propagated by ripples over the surface of water when a stone is thrown into it. So if I draw this violin bow across a tuning-fork, you have this beautiful sound produced. I can actually see the fork vibrating, being thus near it, and you can hear it tapping against this card. The whole function of a tuning-fork is to throw the air into tremors, and these tremors, communicated to the air, are the cause of sound. The tuning-fork communicates its motion to the mass of air which surrounds it. The vibrations of this tuning-fork gradually become less intense, and the sound which it makes gets lower. Now, that is exactly analogous to the cooling of a hot body. It communicates its motion to what is called the "ether," by means of which bodies which are hot communicate their motion to the universe around. You all hear my voice. The human ear is one of the most wonderful organs in the universe. I often think that the human ear is still more wonderful than the human eye. It is by virtue of this wonderful organ that you hear with perfect distinctness every word I am uttering; but it does not tell that this communication of motion is going on. I want to show you something that will. Instead of the ear, I will take a flame, which I dare say will give me a very good result. Perhaps one of the boys will chirrup to that flame. Every vibration produced by the lips by the act of chirruping is communicated to that flame, and makes it dance in that peculiar way. The action of this

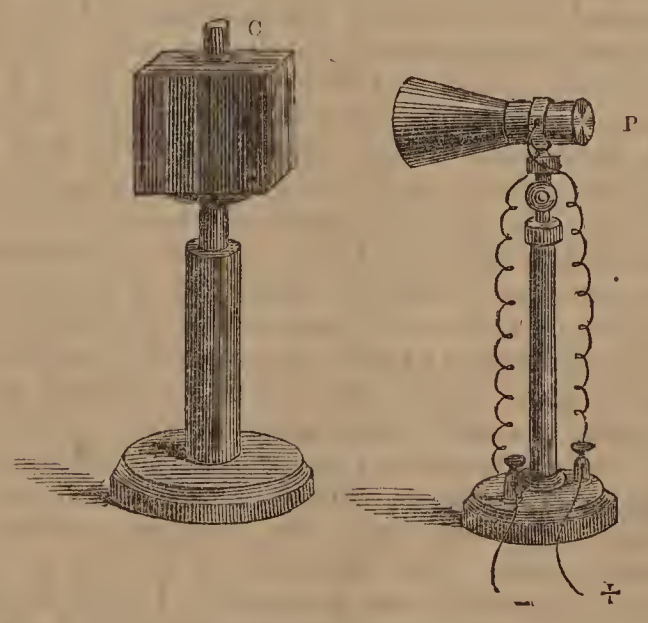
* Reported verbatim, by permission of the Author, for this Journal.

flame is an illustration of the motion produced in the air by sound. This action of flames was discovered by Professor Leconte, in the United States; and it has been worked at in this country by Mr. Barret and myself. Something passes through the air and knocks the flame down when you chirrup. The vibrations communicated to the air make the flame behave in this peculiar way.

We now come to consider the cooling of a body. I say that the act of cooling must be figured in a similar way to the action of a body producing sound. The cooling body is communicating its motion, not to the air, but to this wonderful thing called the ether. The radiation passing through the air might be called the radiation of sound; but when motion is communicated to this wonderful ether it is called the radiation of heat. To illustrate this we must employ this beautiful instrument with which you are already acquainted—the thermo-electric pile. I shall now unite the ends of these wires with the pile, and we shall observe by means of our magnetic needle whether the pile is heated or chilled. I wish I could have a warm cheek here, for every one of you herepresent is a radiating body, not luminous but radiating. [The lecturer then selected a boy from the audience, and led him to the lecture table.] I want to make my young friend here my radiating body. I will first chill the pile by turning it to the cool side of the room, and then bring the needle to rest by means of this magnet. The pile itself is now a radiating body, and hence you see the needle coming down. I will now try and extract heat from the cheek of my excellent friend here. He does not touch the pile. I will depend purely on the radiation of heat from his cheek, and I will venture to say that if his cheek is not chilled by the very cold weather, the needle will move up through an arc of 90° degrees. Observe, now, the needle goes up in virtue of the heat extracted from his cheek. We will now direct the face of the pile against this comparatively polar region of the room, and allow it to waste its heat once more. Now the heat which has produced this effect on the pile is the radiant heat which I want to examine during the rest of the lecture.

I want to show you that various bodies possess the power of emitting this radiant heat in very different degrees. My friend's cheek was an admirable radiator of heat. There are various other bodies, however, much less admirable as radiators. To show this fact, I will take this cube. (Fig. 1). It is covered on three sides with velvet.

Fig. 1.



One side has white velvet, one has scarlet velvet, and the other has black velvet, and this fourth side is a naked face of metal. I should like to make clear to you that these four sides of this cube possess the power of radiating heat in very different degrees; and for the purpose of showing you this I will fill the cube with boiling water. The sides of the cube will become equally heated by the hot water poured into the cube, and then I will allow them in succession to radiate against our thermo-electric pile. I dare say you will then see the distinction. I first bring the needle to zero by turning the face of the pile away from the audience; and now I place the cube of hot water on this little stand near the pile. I think you will agree with me that the outside of the metal side of the cube must be hotter than the velvet surfaces. You could feel this difference by placing your hand upon them. But still, I think the velvet will be able to produce a greater effect upon the pile than the metal surface. The metal side, you see, does not produce much effect upon the pile. Now I turn the velvet to the face of the pile, and you see that the needle goes up beyond the position it occupied when the metal side was there. I now turn the metal side back again, and the needle will go down. Now you see it going down; and when it has gone down a little more, I will turn the black velvet surface towards it, and you will see that the needle will go up again. Thus you see that the heat radiating from this velvet surface is much greater than the heat radiating from the metal; and we have from this fact a beautiful consequence which many boys would not think would occur. The consequence is this. If we filled with boiling water these two vessels, one of which is covered with a thick coating of flannel, and the other of which has naked sides of metal, and allowed them to rest here until the end of the lecture, and then put a thermometer in each to find out the temperature of the water, which vessel do you think would contain the coolest water?

Boys of the Audience: The metal one.

The Lecturer: You have not philosophised correctly upon the experiment I made with the cube. Your conclusion is the most natural one, but you saw that the quantity of heat sent away from the covered surface of the cube was greater than the quantity sent away from the uncovered surface. In the same way the quantity of heat from the radiating vessel coated with flannel would be greater than that radiating from the uncovered metal vessel, and therefore at the end of the lecture the water in the covered vessel would be three or four degrees cooler than the water in the other. In order that this difference should exist in favour of the covered vessel, it must be covered very closely; that is to say, the heat must communicate itself very freely from the surface of the metal to the flannel covering. If it were not covered closely the result would be different, and the heat would be preserved. This is the reason why ladies who wish to keep their tea-pots warm, put over them a kind of night-cap, which they call a "cozey." This cozey must, however, be loose about the tea-pot. If it were to fit very closely it would do more harm than good. However, if it does not fit tightly the heat radiates against the cozey, and the cozey prevents it from being radiated into space.

I have said that we find very great differences among substances in their power of radiating heat. Some are good radiators: some are bad radiators. The metals are all bad radiators. I now want to make plain to you another fact which goes hand in hand with this radiation. I think you will understand the experiment by which I want to illustrate this point. Here you see I have a metal surface which is a bad radiator. If that metal surface formed the side of a vessel containing hot water, it would radiate far less heat away than this surface which is coated with lamp-black. A vessel coated as this surface is would cool the hot water in it far more rapidly than a vessel composed of naked tin. Now, observe that bodies have also different powers of absorbing or drinking in radiant heat, and as a universal rule the body that is a good radiator is a good absorber. Both actions are perfectly reciprocal the one to the other. I want to make this evident to you by means of a device; for in working in physical science we have incessantly to address questions, as it were, to nature, and we do that by means of experimental devices. And now I am going, in your presence, to ask nature the question which of these two tin surfaces, M N or O P, absorbs and takes up the heat most speedily, most rapidly, and most effectually, if both be presented to a hot body,—which of them, in fact,

is the best absorber. The device that I want to employ in this experiment will be evident to you after a little attention on your part. Nothing is learned or nothing is understood without an act of attention on the part of the student; and if you do not think of these lectures afterwards, and read about the subject afterwards—if you do not dwell upon what we say here, and work at the subject, and reflect upon it—these lectures will pass away from your memories, and make very little impression. In fact, these lectures are very little good except for the purpose of stirring you up, and giving you, as it were, the first taste of science. I really do not care much about lectures. I would rather have ten or a dozen boys working away with me in a room than be preaching to them as I am doing now. However, there is good to be done in this way if you will only think about the subject, and bring your own minds to bear upon it afterwards.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, February 6th, 1868.

Dr. Warren de la Rue, F.R.S., &c., President, in the Chair.

The minutes of the previous meeting were read and confirmed, and the donations to the library announced. The candidates for admission into the Society were B. H. Paul, Ph.D., 8, Gray's Inn Square; Edward Dowson, M.D., 117, Park Street, London; Thomas William White, Ifield, near Crawley, Sussex; and for the second time was read the name of Mr. Martin Murphy, Royal College of Chemistry, Liverpool.

Mr. Reinhold Richter, of the Rothamsted Laboratory, was proposed by the Council to become an Associate. The following gentlemen were balloted for, and duly elected Fellows of the Society, viz.: John Wallace Hozier, B.A., Oxon, Lieutenant 2nd Dragoon Guards, Staff College, Farnborough; Herbert McLeod, Assistant Chemist in the Royal School of Mines, 61, Bridge Street, Southwark; Robert Schenk, 10, Hanover Place, Kennington; and Thomas Charlesworth, Leicester.

The adjourned discussion upon Dr. Frankland's new method of "*Water Analysis*" was resumed, and occupied the first hour of this evening's proceedings. From the press of other business—Dr. Russell's lecture, and two papers to be read—the discussion of this important subject was unduly curtailed.

Professor J. A. Wanklyn commenced by replying to Mr. Dugald Campbell, and controverting the accuracy of his statement relative to the evolution of ammonia when albumen was boiled with carbonate of soda. With respect to Mr. Abel's objection, the speaker stated that the peculiar feature of his proposed method of distillation with an alkaline permanganate, was that under its influence the organic nitrogenous matters present in the water were measured by the amount of "albuminoid ammonia" formed, and so long as the ratio remains constant and known, it matters not what is that proportion. Dr. Frankland's statement of the amount of organic nitrogen present in a water was unsatisfactory; he did not say in what form it occurred whether, for instance, as uric acid or kreatine. In some of the waters lately reported upon, it might, on the other hand, be said that nitrogenous matters occurred in quantities equivalent to 23 milligrammes of albumen. Granting that Dr. Frankland could tell the amount of organic nitrogen existing in any given sample of water, what more could he tell? The nitrogen was probably distributed in various forms of organic combination, and some of these might be more hurtful than others, but the speaker had examined a great number of such compounds, and none failed to give "albuminoid ammonia" upon distillation, and he therefore claimed the credit of suggesting that a new fundamental datum should be employed. Mr. Phillip Holland had since proposed, in the Chemical News, the adoption of a new mode of stating the results, according to which the amount of organic impurity in a water would be represented in degrees. There was also the testimony of Professor Way to the effect that "these ammonia values are a sure index of the quality of a water." The speaker characterised the new method of analysis as unsound, requiring a great length of time for its performance, and a high degree of manipulative skill; a litre of water took five days to evaporate in *vacuo*, or ten hours over a water-bath, and then the examination of the residue gave an inaccurate estimate of the amount of organic nitrogen originally present in the water, whereas the method he and Mr. Chapman described was applicable to the water itself and could be carried out in an hour and a half.

Mr. E. T. Chapman considered that the adoption of Dr. Frankland's new process of analysis depended upon the establishment of the following propositions—

1. That nitrates and nitrites are completely decomposed by sulphurous acid.
2. That none of the organic matter suffers decomposition.
3. That the ammonia is perfectly retained.
4. The determination of organic carbon demands its non-volatility in contact with sulphurous acid and sulphites.

In examining these points *seriatim* the speaker found that the decomposition of the nitrates was incomplete or uncertain; for on subjecting the fixed residue of the water to the subsequent action of aluminium and pure hydrate of soda (obtained from sodium), there was ammonia formed in small quantity from that portion of the nitrate which escaped previous destruction. Results obtained in the examination of the pump waters of Portland Street and Bartholomew Lane were quoted, as also an artificial sample of South Essex water. In all these cases some ammonia was indicated by the Nessler test; but inasmuch as the alkaline sulphites interfered, a preliminary distillation with caustic alkali was always resorted to. From Mr. Wanklyn's experiments it appears that the ammonia is not perfectly retained during the preliminary evaporation of the water, but that a loss even of one-third may be experienced. Notwithstanding this loss, however, the discrepancy between their own and Dr. Frankland's results was always on the side of excess, not in defect, and the error was often larger than the total amount of organic nitrogen said to be present in the water. In some cases organic nitrogen had been indicated by the ammonia process when none was obtained by the "gaseous method."

Mr. Dugald Campbell would not occupy the time of the Society by any remarks upon Dr. Frankland's processes which he thought required more study, consideration, and experiments than appeared to have been bestowed upon them by the gentlemen who have just spoken, but would confine himself to making a few remarks upon his own experiments upon water containing known quantities of urea and albumen,* referred to by these gentlemen. The Society will not fail to remember that Mr. Wanklyn, on behalf of himself, Mr. Chapman, and Mr. Smith, read a paper before it in June last, in which it is stated that solutions containing urea and albumen when distilled with the addition of sodic carbonate, two grammes of carbonate to a litre of water, yielded up all the nitrogen of the urea as ammonia, leaving untouched the nitrogen of the albumen to be afterwards acted upon by caustic potash, and ultimately by permanganate of potash, in order

* Paper read before the British Association at Dundee. See Chemical News, vol. xvi, p. 139, et seq.

to obtain all the nitrogen as ammonia from the albumen; and in the paper it is distinctly stated that these results were arrived at by "direct experiments in which a known quantity of urea, gelatin, and albumen were taken." * Upon that occasion he (Mr. Campbell) had remarked that this was not his experience, and that he had never, when operating upon solutions of urea in any quantity with sodic carbonate, been able to decompose the urea thoroughly in the way they said they did, and likewise, that he never had distilled albumen with sodic carbonate without obtaining ammonia from it; both these statements were contradicted at the time by Mr. Wanklyn and Mr. Chapman, the latter gentleman detailing an experiment wherein he had acted upon a known quantity of urea with sodic carbonate, and had obtained from it all the nitrogen as ammonia: how this agrees with what is afterwards stated by these gentlemen will be seen. These results being so diametrically opposed to his (Mr. Campbell's) own experience, he was induced to make some further experiments, but before doing so he thought it advisable, as he had been operating upon rather strong solutions, to write to Mr. Wanklyn and ascertain from him what strength of solutions of urea and albumen he should employ in order to obtain results such as it was stated had been obtained by himself and colleagues; to this he got no reply, but Mr. Wanklyn called upon him in a few days, and after discussing the question, he (Mr. Wanklyn) was of opinion that their process would be fairly tried if solutions were made with fresh white of egg, containing not more than 1-10th of a grain of dry albumen in a gallon of water, and also with not more than the 1-20th part of a grain of urea in a gallon of water; and these proportions were at the time written down on a slip of paper by Mr. Wanklyn, which was in the possession of his (Mr. Campbell's) assistant until recently, but cannot now be found. From his (Mr. Campbell's) experiments above referred to, he proved that solutions of urea generally were not perfectly decomposed by sodic carbonate, as stated by Messrs. Wanklyn, Chapman, and Smith, and also that all solutions of albumen when distilled with the quantity of sodic carbonate stated by them give off some nitrogen as ammonia. To meet the first case, Mr. Wanklyn now states that pure urea in water does not give off ammonia to any great extent when boiled with sodic carbonate and caustic potash, and that albumen when boiled with half the quantity of sodic carbonate they originally proposed, only gives off a small percentage of the nitrogen in the albumen. This proves exactly what he (Mr. Campbell) had stated on hearing their paper read, namely, that sodic carbonate did not entirely decompose urea, and that when he distilled albumen with sodic carbonate with the quantity of that reagent used by these gentlemen, he never failed to get ammonia evolved. It is rather a remarkable circumstance that Mr. Wanklyn, commenting upon his (Mr. Campbell's) paper when read at the British Association at Dundee, should then have "questioned the purity of the urea employed by Mr. Campbell," and should now write "that the urea occurring in waters contaminated with sewage is not pure urea, and that the circumstance that extreme purity imparts to urea a power of resistance which impure urea does not possess, does not in the least degree impair the applicability of our method to natural waters; † this proposition he (Mr. Campbell) ventures to think may be quite satisfactory to Mr. Wanklyn and his colleagues, but would not after a careful consideration of all the circumstances of this case, from first to last, be acceptable to many members of this Society without proper experiments and data fully detailed, and in such a manner as to be capable of being checked by independent operators. In his (Mr. Campbell's) experiments before referred to, every endeavour was made to arrive at the truth, and if they were inaccurate to any great degree, which he can scarcely believe they were, it must be owing to the distilled water with which the standard solutions were made not having been perfectly free from ammonia, although the water he used was tested carefully for ammonia and showed none; but since he had made these experiments he had been preparing some others, but had been prevented completing them from finding it difficult to obtain water on a large scale, which, when more rigorously tested, and in a manner differently to what was done at the time he made his experiments, was perfectly free from ammonia. Without this he was disinclined to proceed, but he hoped to be able to do so soon, when he would be in a position to lay these experiments before the Society.

Dr. FRANKLAND could not presume to reply to all the objections raised by Messrs. Wanklyn and Chapman, but would appeal to experiment and not to argument. Since the last meeting he had, with the assistance of Mr. Armstrong, made further experiments in certain directions which were confessedly imperfect. His results were suspected of being erroneous on the side of excess, because the permanganate method furnished lower numbers in all instances, but the speaker thought he should be able to show proof to the contrary by an appeal to figures. Here were some experimental results with artificial waters:—

In 100,000 *parts of Water.*

Expt.	N. by Permanganate.	Organic Nitrogen.	N. as Nitrates and Nitrites.
I.	·016	·068	·015
II.	·016	·042	·006
III.	·022	·076	nil.
IV.	·308	1·015	nil.

No. 3 was a peaty water, made by infusion of the peat cut five feet deep at Preston, in Lancashire, and the last was a much stronger decoction of peat prepared with the aid of an alkali. The figures in the second column were obtained after treatment with sulphurous acid, and evaporation in *vacuo*. Some of this solution was then precipitated by sulphurous acid, and the resultant solid peaty matter was examined both by the permanganate and by the combustion method, when exactly twice as much nitrogen was indicated by the latter. The residual filtrate from the last product was likewise treated comparatively, and gave nearly three times as much nitrogen gas as that furnished by distillation in the form of ammonia. A still stronger infusion of peat evaporated over a steam bath gave the numbers ·422 and 1·175 respectively.

Proceeding now to natural waters, Dr. Frankland stated that the Thames showed abnormal results during the last month (January), due to the circumstance of the river overflowing its banks, and becoming not only very muddy, but highly charged with organic matters. As before, comparative experiments were made on identical samples.

Water Supply.	Quality.	N. by Mn.	Org. N.
Chelsea ..	Muddy	·011	·058
West Middlesex	Clear	·012	·027
Southwark ..	Very turbid	·024	·061
Grand Junction	Clear	·006	·031
Lambeth ..	Turbid	·030	·062

Next, with reference to the destruction of nitrates and nitrites by sulphurous acid, the speaker was to some extent prepared to admit the force of Mr. Chapman's objection; for on treating a mixture of nitre and salt with sulphurous acid only, one-third of the nitric acid was expelled, but if phosphoric acid, or ferric chloride were at the same time present, all the nitrogen of the nitrates would be expelled. The action of phosphoric acid in this case was not easily explained. Dr. Frankland concluded with a description of the behaviour of nitrogenous alkaloids under both circumstances, thus:—

	Ammonia obtained.	
	By permanganates.	By combustion.
Strychnine	·00032	·00101
Narcotine	·00031	·00068
Sulphate of Quinine	·00073	·00128

* The "Journal of the Chemical Society," ser. 2, vol. v., p. 448.

† The "Journal of the Chemical Society," vol. v., second series, p. 594.

Such were the series of numbers afforded by the two methods of analysis; for his own part the speaker was not at all surprised at the want of accordance manifested in the several instances brought forward. He knew of no precedent in organic chemistry which would lead him to believe that nitrogenous matters should split up in a definite manner, and furnish always the same proportion of ammonia when attacked by oxidising agents. Mr. Wanklyn and himself had both encountered the same difficulty in attempting to fix the nature of the organic matters occurring in samples of water, but at any rate we were not sure of its existence as albumen, and, until something more definite were proposed, he should not be inclined to abandon the combustion process.

Dr. De la Rue said that this discussion furnished evidence of the importance to be attached to a right appreciation of the results of water analysis, and no method was now brought forward but it was immediately sifted and discussed, and the truth ultimately elicited. This was a confessedly difficult branch of analytical chemistry, and there seemed to be opportunities of further research.

Dr. Attfield had placed upon the table a sample of water from Jamaica, the constitution of which was very remarkable, and the flavour peculiar. It contained:—

	Grains per gallon.
Chloride of calcium	1500
Chloride of sodium	1000
Chloride of ammonium	2½

The President also referred to a small but powerful voltaic battery of ten cells, constructed by Dr. Hugo Müller and himself, upon a new principle. The negative element was chloride of silver fused around a central silver wire, which served as conductor; this was bent over and connected by means of a small caontchouc band or collar, to a rod of zinc, which need not be amalgamated. The exciting liquid was salt water, which in course of time became charged with chloride of zinc, and only required to be renewed when metallic zinc commenced to deposit on the negative plate. Ten of these little couples, three inches or less in height, were mounted on a wooden frame supported and sliding upon glass uprights, so that the battery was very easily put in action; and its tension was so great that a cubic inch of the mixed gases was given off from water in about twenty minutes. Mr. Gassiot thought somewhat highly of this arrangement, and the President was now having made for further trial a battery of two hundred cells.

Dr. W. J. Russell then proceeded to deliver a lecture "*on Gas Analysis*," the report of which must stand over until next week.

An important paper, which Dr. Frankland characterised as describing "one of the greatest triumphs of modern synthetical chemistry," was next read by the Secretary. It was communicated by Professor H. Kolbe, and entitled "*Reduction of Carbonic Acid to Oxalic Acid*," by Dr. E. Drechsel. A mixture of pure sodium and dry sand was heated in a flask to the boiling point of mercury, and a rapid stream of dry carbonic acid passed. After a few hours the silvery aspect of the metal changed to a red mass, and ultimately became nearly black; towards the end the heat should be moderated to avoid reduction to carbon, and the whole slowly cooled. Left in the air for the sodium to oxidise and then exhausted with water, it furnished a solution containing oxalate of sodium. From ten parts of sodium one part of calcic oxalate was obtained. Potassium amalgam containing 2 per cent of the alkali metal acts in the same way.

A paper by Mr. W. H. Perkin, F.R.S., "*On some new Benzylic Derivatives of the Salicyl series*," was then read. [An abstract of this communication will appear next week].

The meeting was at a late hour adjourned until the 20th inst., when Mr. David Forbes, F.R.S., will deliver a lecture "*On some Points of Chemical Geology*."

NOTICES OF BOOKS.

A Manual of Inorganic Chemistry, arranged to facilitate the Experimental Demonstration of the Facts and Principles of the Science. By Charles W. Eliot and Frank H. Storer. Second Edition. London: John Van Voorst. (Pp. xiv. and 605).

The favourable reception awarded to this work in America is doubtless the immediate cause of its re-publication in England. Its plan, moreover, differs in several essential points from that usually adopted by the compilers of elementary chemical manuals in this country, since it is primarily designed directly to accompany a course of practical study in the laboratory. In some respects it resembles the once popular manual of Stoeckhardt; indeed to this book the authors avow their obligations for many experimental details. The work is not written in support of any particular theory, or in the interest of any one system of notation, the fundamental idea of its authors having been to facilitate the acquisition of a knowledge of inorganic chemistry as far as possible by, as they say, the experimental and inductive method. To this end they give a large number of experiments simple and inexpensive to perform, although perfectly adequate to demonstrate the leading facts and generalisations of the inorganic portion of the science, for the authors plainly indicate that much of the complicated paraphernalia with which our modern lecture-rooms are equipped is by no means absolutely necessary to elucidate the radical laws and principles of chemistry. These experiments are intended to be made by the student himself, and in general sufficiently minute instructions are given to insure their successful performance. We cannot, however, always compliment the authors on the elegance or clearness of the style in which these instructions are conveyed; but as an example of the character of the experiments we quote the following method of demonstrating that ammonia is actually produced from materials which are proved to generate a mixture of hydrogen and nitrogen, since a more direct synthesis is still a desideratum (p. 79).

"Exp. 45. Place in an ignition-tube an intimate mixture of 3 grammes of fine iron filings with 0·2 grammes of caustic potash; adapt a delivery-tube to the ignition-tube, heat the contents of the tube over the gas-lamp, and collect the gas which escapes in a test-tube over the water-pan. Examine this gas, which will prove to be the inflammable hydrogen. Caustic potash, as we have already learned (p. 74), consists of potassium, hydrogen, and oxygen; at a high temperature, metallic iron is able to seize upon a portion of the oxygen in this compound, setting free hydrogen, which finds no place in the new combinations.

"Exp. 46. Heat in a second ignition-tube, similarly disposed, a mixture of 3 grammes of fine iron filings and 0·2 grammes of nitrate of potassium, and collect the gas, as before, over water. This gas has neither taste nor smell, and when tested with a lighted splinter it is found to be uninflammable, and in fact to extinguish the taper. It is nitrogen. Nitrate of potassium contains, as has been already stated (p. 75), potassium, nitrogen, and oxygen; at the high temperature employed, the salt is partially decomposed, the metallic iron combines with the oxygen of the nitrous vapours formed, and their nitrogen is set free.

"Exp. 47. In a third ignition-tube, heat the same quantities of the same materials which have been used in the last two experiments, at once and together. A delivery tube is not necessary in this case; the tube may be held by the wooden nipper by the open end. Neither hydrogen nor nitrogen will be evolved as before, but instead of them we have ammonia, whose presence may be manifested by holding a bit of reddened litmus paper at the mouth of the tube. The intense alkaline reaction of the gas, and its

odour, sufficiently distinguish it from both hydrogen and nitrogen."

The authors, however, clearly discriminate between chemistry and chemical manipulation, and give, in the form of an appendix, very practical instructions on the latter subject.

Many arguments may be adduced in support of such a method of instruction—arguments, too, which would probably have greater weight now than formerly, when the practical study of physical science was ignored and banished from the ordinary *curricula* of our schools. Not the least weighty of these arguments will be found in the incontestable fact that this method necessarily tends to discipline and develope the student's perceptive faculties—one of the capital results of a well-devised method of teaching physical science. The student is enabled directly to examine for himself—to see, smell, and handle for himself—and he thus becomes acquainted with the main facts of the science by a process similar to that by which the facts themselves were originally perceived and established. Everyone will readily grant to the authors that scientific study fails of its true end if it become a mere exercise of the memory. Moreover, such a method, above all, materially facilitates the attainment of a definiteness and exactitude in the knowledge of the subject, without which, as an authority on this matter has recently declared, the introduction of physical science into our school system is worse than useless; and although chemical lecture illustration has never been carried to such a degree of perfection as at this time, when our professors appear almost to base their reputation, in the lecture theatre, on the brilliancy and effectiveness of their demonstrations, we venture to assert that under the present system of science tuition this exactitude is by no means so generally acquired as it ought to be. The result to the student is not commensurate with the labour of the teacher. Doubtless, the evil to some extent is inherent in the system, but much of the ill-success, it seems to us, is to be directly ascribed to the student. The almost universal complaint of teachers, even in this age of the multiplication of manuals, is that students, as a rule, will not sufficiently study their text-books. An undue prominence is given to the teaching in the lecture-room; by some it is invested with a value which it cannot in strict reason intrinsically possess. There is a proneness to regard the text-book as merely supplementary to the lecture. Even the most conscientious students err in considering they gain their object merely by a regular attendance in the class-rooms, unremitting attention to the lecturer, and a careful transcript of their voluminous notes at leisure. But how is it possible for the student mentally to digest the lecture when his sole aim is apparently to get a verbatim report of it? How frequently in the hurry of mechanically noting the definition of a principle, or the description of a property, does the student miss the point of the experiment by which the one or the other is intended to be illustrated? That *ex abusu non arguitur ad usum* everybody knows; but the practice of note-taking is often carried to an injudicious excess, and operates injuriously against both the teacher and the taught.

Hence it appears to us that the method employed in this book will go far to obviate this tendency, and we venture to predict that manuals based on this or a similar plan will multiply with the more general introduction of the practical study of physical science into our school system.

The authors presuppose the students of their manual to be already acquainted with the rudiments of physics, and, therefore, contrary to the practice which obtains in England, they plunge at the very outset *in medias res*. Still, as they have thought fit to recapitulate at length many of the physical properties of bodies, it would, we take it, have been well to have directed the student's attention to the natural effects of many of these properties—to the beneficial consequences resulting from the singular anomaly of water possessing a condition of maximum density, for example. And, as the question at issue between Tyndall and Magnus is apparently settled at last, we also regret that all mention of the effect of atmospheric moisture in retarding and modifying the intensity of solar radiation is omitted. Every true student of physical science knows the quiet innate sense of enjoyment to be derived from the knowledge and contemplation of such phenomena.

We observe that the statements of the older manuals with respect to the existence of definite hydrates of the so-called "mineral" acids are repeated, although Roscoe and Dittmar showed some years ago that the uniformity of the composition of these bodies was only apparent, and in reality an accidental circumstance depending simply on the pressure under which their distillation had been effected; they proved that for every pressure an aqueous solution exists which, when distilled under that pressure, possesses a constant boiling point and fixed composition.

The chapter on antozone is mainly made up of the vague and unsatisfactory statements of Meissner and Houzeau. The authors, however, plead in extenuation for thus setting forth whatever is known respecting antozone, "the impossibility with so obscure a subject of making such a just discrimination between salient and unimportant points as with a well studied subject is both easy and desirable." In our opinion, there is but little satisfaction to the unfortunate student who is thus shown "how vague and uncertain the prospect is when once the narrow limits of established knowledge are past, and the inquirer ventures out into the obscurity which perpetually separates the knowledge of to-day from that which shall be knowledge to-morrow" (p. 154).

In taking leave of a work to which it gives us pleasure to direct attention, we cannot refrain from quoting the following just discrimination of the relative position and value of fact and theory:—"In the midst of the doubts and discussions which to-day envelope chemical theories, the student will do well to remember that all these questions lie without the sphere of fact. They do not affect the actual composition or properties of a single element or compound; they are questions of interpretation, classification, and definition. The existence of atoms is itself an hypothesis, and not a probable one; all speculations based on this hypothesis, all names which have grown up with it, all ideas which would be dead without it, should be accepted by the student provisionally and cautiously, as being matter for belief but not for knowledge. All dogmatic assertion upon such points is to be regarded with distrust. The great majority of chemists, devoted to the applications of chemistry in mineralogy, metallurgy, dyeing, printing, and the manufacture of chemicals, remain completely indifferent to discussion of chemical theories. Hence the student will find that in technical chemical literature the older notation and the corresponding smaller atomic weights are almost invariably employed. Theories, however, are of great importance to the progress of the science and to the clear ordering of the ground already won. It is on this account very much to be wished that the great attention now devoted to the discussion of the best methods of representing symbolically the constitution of chemical substances and the changes to which they are subject may result in the elaboration of a system good enough to command general acceptance."

Blowpipe Vesicular Reactions.—Captain Ross desires us to state that the colouration of borax and P. salt by certain substances, is so *extremely delicate* when it is blown into a vesicle, that he believes many new reactions of metals will soon be made under this head. In the meantime, the two following additions may be confidently made to the blowpipe tables of Plattner and others:—

Silver (oxide of)—In Borax.—Opaline by *reflected* and a beautifully delicate pink colour by *transmitted* light. (No other substance can be mistaken for this.)

Tungstic Acid (in *Wolfram*)—In Borax.—A peculiar *amber-yellow*, which cannot possibly be mistaken for the yellow given by oxide of iron.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Sulphophtalic Acid.—O. Loew. This compound is obtained by heating phtalic acid with sulphuric anhydride to 100° or 105°C. in a sealed vessel. The products of the reaction are dissolved in water, neutralised with plumbic carbonate and the plumbic sulphophtalate, which remains in solution, is decomposed with hydric sulphide. The composition of the baric sulphophtalate corresponds to the formula—

$$\left.\begin{matrix}2BaO\\HO\end{matrix}\right\} (C_{12}H_3)''' \left(\begin{matrix}(C_2O_2)_2\\S_2O_4\end{matrix}\right)O_3$$

—(*Ann. Chem. Pharm.* cxliii. 257.)

Phenylenediethylacetone and Ethylenediethylacetone have been obtained by G. Wischin, by acting upon phtalic and succinic chloride with zincic ethide. Phenylenediethylacetone

$$\begin{matrix}C_{12}H_4\\(C_4H_5)_2\end{matrix}\left(\begin{matrix}C_2O_2\\C_2O_2\end{matrix}\right)$$

is soluble in ether and alcohol, insoluble in water. An ethereal solution yields large crystals on slow evaporation. Ethylenediethylacetone

$$\begin{matrix}C_4H_4\\(C_4H_5)_2\end{matrix}\left(\begin{matrix}C_2O_2\\C_2O_2\end{matrix}\right)$$

is a pale yellow liquid. Neither of the two acetones combine with alkalic disulphite.—(*Ann. Chem. Pharm.* cxliii. 259.)

Action of Ethylene on Sulphuric Oxychloride.—F. Baumstark. Sulphuric oxychloride absorbs ethylene with disengagement of chlorhydric acid. A thick brown oil is formed which presently solidifies, and on being treated with water, yields anhydride and acid of isethionic, and a new acid of the composition $C_2H_8SO_6$. The latter is obtained as a syrup which gradually assumes crystalline condition. Its salts mostly crystallise well. If sulphuric oxychloride is treated with an excess of ethylene, it takes up 2 mol. of the latter, and a yellow oil is formed, the smell of which resembles that of oil of mustard. It boils at 154° C.; its composition is $C_2H_5SO_3Cl$. It decomposes on exposure to air, separating at the same time into two layers, the upper of which being an oil of the composition $C_4H_{10}SO_5$. The action of dry ammonia upon $C_2H_5SO_3Cl$ gives rise to the formation of a well crystallising body of the composition $C_2H_7SO_3$.—(*Zeitschr. Chem.*, N.F. iii. 566.)

Conversion of Hydrocarbons into Ketons.—E. Linnemann has continued his experiments on the conversion of monobromated hydrocarbons of the CnH_2n series into ketons of fatty acids of the same number of carbon atoms, and extended them to ethylene, propylene and amylene. The reaction the author makes use of consists of an oxidation by means of mercuric acetate. In the case of ethylene only traces of aldehyde could be detected. The proportions of ketone obtained from propylene and amylene were likewise very small, acetic acid being the principal product of the reaction in all cases.—(*Ann. Chem. Pharm.* cxliii. 347.)

Iodides of Organic Bases.—S. M. Jörgensen has prepared and examined the superiodides of a great number of organic bases, more especially those of strychnine and brucine, and also such of their derivatives as contain besides iodine an alcohol radical. The former were generally prepared by precipitating a salt of the base with a solution of iodine in potassic iodide, the latter by mixing alcoholic solutions of the base, and iodide of alcohol radical. All these compounds are obtained in brilliant crystals from their alcoholic solutions. For detail the reader must be referred to the original paper.—(*Ann. Chim. Phys.* [4] xi. 114.)

CORRESPONDENCE.

BEET-ROOT SUGAR.

To the Editor of the Chemical News.

Sir,—Your foreign correspondent in yesterday's number remarks, "as the manufacture of beet-root sugar is not an English industry, an abstract of this memoir would probably possess little interest for your readers." As the manufacture of beet-root sugar will probably be commenced this year, in more than one locality in England, all information respecting it will be peculiarly valuable at a time when the best processes should be at once adopted. Your correspondent in Paris, by communicating the earliest information, will probably be conferring great benefit on an industry which will in all probability soon become of national importance.

If you can obtain for me the title of the best and most recent French works on the growth of the sugar beet, and the manufacture of sugar from it, you will greatly oblige.—I am, &c.,

Robert Oxland.

Compton Gifford, Plymouth, February 8th, 1868.

THE RECENT DISCUSSION AT THE CHEMICAL SOCIETY.

To the Editor of the Chemical News.

Sir,—Having only renewed—not commenced—the discussion at the last meeting of the Chemical Society, I believe that I had no right of reply at the end of the discussion.

Mr. Campbell's remarks, however, demand an answer. Mr. Campbell stated that I had not published a single experiment in which I took white of egg, and failed to get ammonia from it on boiling with dilute solution of carbonate of soda.

I quote a passage from my paper (*Laboratory*, 28th September, 1867, page 442), and make the remark that Mr. Campbell had read that paper.

"III. A litre of spring water, 1·864 grm. of carbonate of soda, and 3·5 milligrm. of fresh white of egg (weighed on a bit of platinum foil), were introduced into a retort and distilled:—

1st distillate,	100 c.c.	= 0·000	milligrm. NH_3
2nd ,,	100 c.c.	= 0·000	,, ,,
3rd ,,	100 c.c.	= 0·000	,, ,,
		0·000	,,

This extract speaks for itself, and is surely sufficient to justify my interruption of Mr. Campbell's speech.

Equally contrary to the fact, is Mr. Campbell's representation that the dispute between us was whether traces of white of egg (not a considerable proportion) were decomposed.

If your readers will turn to Mr. Campbell's paper (*Lab.*, September 21st, 1867), they will find that according to Mr. Campbell, he got off about 33 per cent of the total nitrogen in the form of ammonia, when he boiled 0·093 grain of (moist) white of egg with dilute carbonate of soda, and that on taking still more dilute solutions of albumen *all* the nitrogen came off as ammonia.

In my reply (*Lab.*, 28th September, 1867), your readers will find the following:—

"I have thus taken 5·00, 0·40, and about 0.04 milligrm. of *albuminoid* ammonia in the shape of white of egg, and in no case got over two and a half per cent of the albuminoid ammonia evolved by carbonate of soda."

Mr. Campbell's paper is of a piece with his speech, According to him he took a quantity of urea, containing nitrogen equivalent to ·0062 grain of ammonia, and having boiled it with dilute carbonate of soda, then with potash.

and finally with permanganate of potash, got altogether .0061 grain of ammonia, ·0015 grain of this ammonia being evolved by the permanganate. In a second experiment, he describes himself as having taken the same quantity of urea, and obtained accurately ·0062 grain of ammonia, this time ·0025 grain by the permanganate. When I add, that since the publication of Mr. Campbell's paper, the observation has been recorded, that boiling with alkaline permanganate actually oxidises urea, and evolves its nitrogen, in great part, as nitrogen gas, or as nitric acid, the character of these experiments of Mr. Campbell's will become intelligible. Notwithstanding this oxidation, Mr. Campbell finds accurately all his nitrogen in the form of ammonia.—I am, &c.,

J. Alfred Wanklyn.

London Institution, February 8th, 1868.

MISCELLANEOUS.

Chemical Society.—The following lecture arrangements have been made :—February 20th, "On Chemical Geology," by David Forbes, F.R.S., F.G.S., F.C.S. On March 19th, "On the Manufacture of Glass," by Henry Chance, M.A., "On Steel direct from the Ore," by C. W. Siemens, F.R.S.

Soiree of the Chemical Society.—The President has issued invitations for a soirée, on the evening of March 11th, 1868, at Willis's Rooms. Gentlemen who are willing to send contributions, are requested to describe their nature to the Secretaries of the Chemical Society, Burlington House, so that arrangements may be made for properly displaying them. All contributions must be delivered and arranged before 2 p.m. on the 11th, and removed before 11 a.m. on the following day.

Obituary.—We have this week to record the decease of another of our greatest philosophers—Sir David Brewster,—who died on Monday evening, at Allesley House, near Melrose. To Sir David we owe many of the vast researches made in physical science. Commencing his scientific career at the University of Edinburgh, he very quickly had the honorary degree of M.A. conferred on him, and a few years afterwards was elected a Fellow of the Royal Society of Edinburgh. In 1815 he gained the Copley medal of the Royal Society, for a valuable paper on the "Polarization of Light by Reflection," and was also elected a Fellow. He afterwards gained the Rumford medal for further discoveries relating to the polarization of light, and the Keith prize from the Royal Society of Edinburgh, for his discovery of two new fluids in minerals, and his analysis of solar light. He was also a member of most of the foreign academies. In 1831 Sir David proposed the scientific meeting at York, which resulted in the establishment of the British Association for the Advancement of Science. During the same year he received the honour of the Hanoverian Guelphic Order, and in 1832 he was knighted by William IV. We here cannot but express our surprise that no greater honour than knighthood can be conferred on such men as Sir David Brewster and Sir Charles Wheatstone, whose discoveries have added so much to the wealth and prosperity of our country. Sir David Brewster retained his love for science to the last, almost weekly contributing papers to the scientific journals.

Improved Spectroscope.—Professor Osborn, of Lafayette College, Easton, Pa., has made improvements in the spectroscope, by which it may be readily applied to a variety of practical purposes, especially in metallurgical operations. In a letter to the *Scientific American*, he says :—"The instrument complete is so arranged that the observer reads the degree on the scale by the actual light which he is analysing. The very light which comprises, in its flame, the vaporized metal, as lime, iron, chromium, titanium, sodium, &c., discloses to the observer in the spectral form its own nature, not only, but often to a great degree, the approximate quantities found in the original ore or even in the coal used, or from the wasting brick of the furnace. Nothing can exceed the beauty of the spectral forms which suddenly appear and disappear in the otherwise darkened tube, as the observer stands at the 'tunnel head' of the furnace, watching, as it were, the spectral secrets of that terrible flame which pours forth from the stack, especially when, after the 'cast' and consequent cessation of the blast, that blast is again turned on. The bright yellow bar of sodium is almost always present during examination of all flames resulting from the use of any and all forms of anthracite in the furnace and forge, or from decomposing soda feldspars. But one of the most striking facts in my examinations occurred at our last analysis of a flame from a re-heating furnace on the Lehigh, at the wire works of Stuart & Co. The workmen held partly out a bar of intensely heated iron on the hearth of the furnace, when, at rapid intervals, the dark lines which are seen in the solar spectrum appeared faintly, but certainly flitting over the spectrum of the fierce flame by which the intensely heated iron was enveloped. An instrument, of a circular form, is in course of construction, under my direction, for the easy examination of these flames, and which may be used at any time and at considerable distances, and I am hoping that such shall be its sensitiveness that the furnace master may sit in his room and know much of the efficiency and value of the operations proceeding at the furnace by its use. I am situated on a hill, and by means of my instrument, placed upon my dinner table, I can get a beautiful spectrum from a reheating furnace situated not much less than a half-mile from my instrument, and am able to detect the sodium in the coal, or from the decomposed fire brick, and also any lime, potash, &c., which proceeds from the furnace mouth. I have no doubt that some exceedingly important uses may be made of this discovery of the spectroscope in the line of metallurgical operations."

Nitroglycerine and Greek Fire.—We have been requested to publish the following memorandum, which has been prepared under authority, and has been issued by Lieutenant-Colonel C. B. Ewart, R.E., by order of the Secretary of State for the Home Department :—"Nitroglycerine is not applied as an incendiary agent, and, if used as an explosive, it will not be scattered loosely about, but will be employed in cans or other closed vessels. If such should be discovered, they should be carefully removed, some heavy body should be attached to them, and they should be thrown into deep water, without any attempt being made to open them. True Greek fire is simply a solid highly combustible composition, very similar to 'Carcass Composition.' What is now commonly called Greek fire consists of a solution of phosphorus or of sulphur and phosphorus in a very volatile liquid, the bisulphide of carbon, to which occasionally some mineral oil is added with the view of increasing its incendiary powers. When this liquid is thrown on to any surface exposed to the air, the solvent evaporates, leaving a film of the phosphorus or sulphide of phosphorus, which will then inflame spontaneously, but will not very readily set fire to wood or combustible materials. The proper mode of extinguishing the flame produced by such an incendiary agent is to throw upon the burning surface a quantity of wet or damp sand, ashes, sawdust, lime, or any other powder, or wet sacking or carpeting, any material, in short, by which the flame can be stifled by exclusion of air. No attempt should be made to remove the covering for some time after the flame has been extinguished. The place should afterwards be thoroughly scoured by playing upon it for some time with a powerful jet of water. Should any scattered liquid be discovered which has not become inflamed, it should be washed away, as above directed, as quickly as possible ; and if a jet of water is not immediately at hand, it should in the meantime be covered in from the air by application of any of the materials named above."

CONTEMPORARY SCIENTIFIC PRESS.

Under this heading it is intended to give the titles of all the chemical papers which are published in the principal scientific periodicals of the Continent. Articles which are merely reprints or abstracts of papers already noticed will be omitted. Abstracts of the more important papers here announced will appear in future numbers of the "Chemical News."

Journal des Fabricants de Papier.

October 1, 1867.

E. BOURDILLIAT, "On Testing the Materials and Chemical Products used in Paper Making." (Continuation.) "Detection of the various Fibres used in the Manufacture of Paper." "A Method of Ascertaining whether a paper has been Sized with Gelatine or Resin."

Journal für Praktische Chemie.

October, 1867.

CARSTANJEN, "On Thallium and its Compounds." J. LOWE, "On the Transformation of Gallic Acid into Tannic Acid." H. BAUMHAUER, "On the Production of Light during the Oxidation of Potassium and Sodium when exposed to the Air." K. FRISCH, "On the Composition of the White External Coating, and of the Black Inner Mass of a Flint from the Island of Rügen."

Comptes Rendus.

November 4, 1867.

SIR D. BREWSTER, "Letter to Chevreul on the Nature of the Relations which existed between Newton and Pascal." CHASLES, "On the same Subject." BECQUEREL, "Third Memoir on some newly discovered Electro-Chemical Effects of Capillary Action." E. PELIGOT, "On the Distribution of Potash and Soda in Plants." E. BOUCHOTTE, "On the Dialysis of Induction Currents." C. BLONDEAU, "On the Action of Induction Currents on Plants."

November 11.

SIR D. BREWSTER, "Letter to Le Verrier on the Nature of the Relations which existed between Newton and Jacques Cassini." "Letter to Chevreul on the Authenticity of the Newton and Pascal Correspondence." BALARD, "On the same Subject." CHASLES, "Answer to Sir David Brewster's two Letters on the Nature of the Relations which existed between Newton and Pascal." GRANT, "Letter to Le Verrier on the Astronomical Observations of which Newton and Pascal may have made use." F. LAROQUE, "On the Penetration of Air Bubbles into a Liquid on the Passage of a Projectile into the same." VELTER, "On the Value of Sea-Salt as Manure by reason of its Transformation into Carbonate of Soda, and finally into Nitrate of Soda." F. BELLAMY, "On the Use of Subsulphate of Alumina for detecting and estimating certain Organic Matters in Water." H. SCHIFF, "On Condensed Ureas." A. BOBIERRE, "On the Mannfacture of Chloride of Lime, and on Chlorimetry." C. MENE, "An Analysis of some Samples of Coal from Prussia."

November 18, 1867.

SIR D. BREWSTER, "Letter to Chevreul on the Authenticity of the Newton and Pascal Correspondence." CHASLES, "Answer to R. Grant's further Communication on the Newton and Pascal Correspondence." A. GAUTIER, "On some new Nitriles of the Fatty Series." DE ROMILLY, "On the Preparation of Cyanides."

Poggendorff's *Annalen.*

October, 1867.

R. RUHLMANN, "Researches on the Effect of Temperature on the Velocity of Light in Water." G. QUINCKE, "Optical Researches." 9. "On Jamin's Compensator, and on a new Method of Determining the Refractive Index of Glass Plates for different Lines of the Spectrum." C. FREESE, "On the Combinations of Iron with Phosphorus." L. SOHNCKE, "On the influence of the Motion of a Source of Light on the Refraction of the Light emitted, being some Critical Remarks on Professor Klinkerfues' Recent Discovery."

Bulletin de la Société Chimique de Paris.

October, 1867.

BERTHELOT, "Answer to Fritzsche's Remarks published in the *Bulletin de la Société Chimique* for September, 1867, on the Author's Paper on Anthracene." BERTHELOT, "On the Hydrocarbons of Coal Anthracene. Fluorene. Acenaphthene or Tar; Styrolene; Cymene. Hydrides of Naphthalene. Acetylonaphtalene." WIEDERHOLD, "A new Green, derived from Linseed Oil and Oxide of Copper." BERNARD, SCHEURER, and TEMPE, "A new Process for the Extraction of Indigo from Dyed Woollen and Cotton Rags." BERNARD, "A new Process for Dyeing Stuffs Turkey Red."

Journal für Praktische Chemie.

November, 1867.

CARSTANJEN, "On Thallium and its Compounds." C. F. SCHONBEIN, "On the Transfer of the Oxygen absorbed from the Atmosphere by Turpentine and similar Organic Substances to Water." "On the Presence of Active Oxygen in Organic Substances: 1, on the Quantity of Ozone contained in Blue Guaiacum Wood. 2, on the Free-Active Oxygen of Quinone. 3, on the Combination of Cyanine with Ozone, 4, On the Combination of Olefiant Gas with Ozone." "Researches on Guaiacum Resin." "On Brasilin and on its Fluorescent Properties." J. WOLFF. "On Two new Derivatives of Aniline." W. STEIN, "Contributions to the Knowledge of Orellin, a Yellow Colouring Matter derived from *Bixa orellana*." OTTO, "On the Characters of Thallium, and on the Metallic Group to which it belongs. G. VORBRINGER, "A Method of producing a Black Pharaoh Serpent."

Dingler's *Polytechnisches Journal.*

November, 1867.

O. ZABEL, "An Electro-magnetic Apparatus for regulating the Temperature at which Substances are dried in Chemical Operations." G. LUNGE, "On the Analysis of the Materials used in and of the Bye-Products of the Manufacture of Soda." E. BRESCIUS, "On the Simulation of Arsenic by Chloride of Zinc in Marsh's Test, and on the Detection of Arsenic and Antimony in Hydrochloric Acid."

NOTES AND QUERIES.

Sulphite and Hyposulphite of Soda.—Can any of your correspondents enable me to discover the consumption of sulphite and hyposulphite of soda in England, and also its uses.—G. W. R., Liverpool.

Chlorimetry.—Is it possible to devise a ready way of estimating the chlorine in a solution of bleaching powder containing a large proportion of nitrate of lime?—S.

Price of Sulphuric Acid.—Perhaps some reader of the CHEMICAL NEWS can inform me what price I ought to pay for sulphuric acid of 1·6 specific gravity, supposing I pay £4 per ton specific gravity, 1·72.—V. C.

Swedish Cooking Apparatus.—I see you ask the address of the inventor of the Swedish, or rather Norwegian, Cooking Apparatus. It is Mr Sörensen, 13, Duke Street, Grosvenor Square, W. I can vouch, from my own use, for the practical efficiency of this simple converse of a refrigerator.—MARSHALL HALL, 3, Cleveland Terrace, Hyde Park, W.

Portable Cooking Apparatus.—In the CHEMICAL NEWS, "To Correspondents," I note "Swedish portable cooking apparatus," and it occurs to me that a knowledge of a lamp I fortunately came across in Scotland, and took with me to Iceland, might prove interesting to travellers; though I may be describing what you have seen. The lamp is of copper, and is partly filled through the cock with methylated spirit, the tap closed, and a little spirit poured into open centre and lighted—the flame soon warms the sides and top of the lamp, and the consequent expansion of the vapour inside presses the spirit in a strong jet from a bent tube connected with bottom of spirit chamber, which lights, and increases the heat of the lamp, thus getting a stronger flame. Mine would boil a pint of water in five minutes, and use from one to one and a half ounce spirit.—JOHN CLIFF.

The Sprengel Air-pump.—"Ein Englander" will find a paper entitled "Sprengel's Researches on the Vacuum," wherein his mercurial air-pump is described, in the January number, 1865, of the *Journal of the Chemical Society*. With an instrument of total height of about 6 feet, and from 10 to 15 lbs. of mercury, a receiver of half a litre capacity may be exhausted in from 20 to 30 minutes: the fall tube must not be of greater calibre than 3 millimètres, and had better be of 2½—2¾ millimètres only.—C. R. A. WRIGHT, B.Sc.

Sulphur in Pyrites.—In answer to F. W. W, I beg to point out that if it is *not* required to obtain the absolute quantity of sulphur met with in pyrites, a pretty near estimate of the value of the mineral for use in the manufacture of sulphuric acid may be obtained by taking a fair average sample of the pyrites ground to an impalpable powder, weigh off 25 to 30 grains, and expose these on a flat platinum dish, best in a thoroughly heated muffle to pretty strong incandescence for two or three hours; if no muffle is at hand a good gas flame will answer, and after cooling the loss in weight may be taken; but this method is *not* to be used but as an approximative estimation; the analysis of pyrites for sulphur requires many precautions to yield good and reliable results; of course the sulphur is oxidised to sulphuric acid, and the latter estimated, and from this result the amount of sulphur is calculated.—DR. A. A.

MEETINGS FOR THE WEEK.

TUESDAY.—Royal Institution, 3. Professor Tyndall, "On the Discoveries of Faraday."
WEDNESDAY.—Meteorological, 7.
—— Society of Arts, 8.
—— London Institution, 6½.
THURSDAY.—Royal Institution 3. Professor Tyndall, "On the Discoveries of Faraday."
—— Royal, 8½.
—— Zoological, 4.
—— Chemical, 8. "On Some Points of Chemical Geology," By David Forbes, F.R.S., F.G.S., F.C.S.
—— Royal Society Club, 6.
FRIDAY.—Royal Institution, 8. Rev. M. W. Mayow, "On Macbeth and Hamlet."
SATURDAY.—Royal Institution, 3. Professor Roscoe, "On the Non-Metallic Elements."

TO CORRESPONDENTS.

Communications have been received from Johan Sörensen; J. Cliff; O. Hopkinson; J. Rugeley; F. L. Bigg (with inclosure); A. Chorlton; E. Whittingham; Professor Wanklyn; E. F. Jones (with enclosure); R. Oxland; Dugald Campbell; H. Bird; W. Ellis; T. E. Thorpe; H. Stephenson (with enclosure); J. Dempsey; W. Schofield; F. Sarl (with enclosure); W. Harding: W. Valentin (with enclosure); M. Phillips; E. Smith; E. B. Marten (with enclosure); W. Hofmann; O. Kerl; H. J. Helm; Marshall Hall; H. R. Williams & Co.; W. Bird Herapath, M.D., F.R.S.; J. Taylor; Lord Sackville Cecil; A. H. Allen; J. Marples; J. C. Lee; F. Shaw; D. Forbes, F.R.S. (with enclosure); Mottershead & Co.

Books Received.—"Chemical Notes for the Lecture Room. On Heat, Laws of Chemical Combination, and Chemistry of the Non-Metallic Elements," by Thomas Wood, Ph.D., F.C.S., 2nd edition. London: Longmans & Co.; "The Transference of the Telegraphs to the State," by John Stephen. London: Longmans & Co.; "Brief Extracts of Reports on Boiler Explosions in 1867," by Edward B. Marten; "Minutes of Proceedings at the Extraordinary Meeting of Shareholders of the Atlantic Telegraph Company"; "Réforme Scientifique."

Paris Exhibition Two Gold Medals.

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CRYSTALLOGRAPHY AND THE BLOWPIPE.

By Captain W. A. ROSS, R.A.

Some new and apparently incontrovertible facts deducible from the vesicular reactions observed and recorded by me, would seem to be :—

1. *Every* inorganic substance, chemical or mineralogical, crystallises inevitably from its solution in borax.
2. These crystallisations are *not* isomorphous.
3. Those substances which crystallise soonest are most deliquescent.
4. Crystallisation *always* precedes deliquescence.
5. Alkaline are more crystallisable and more deliquescent than acid salts.
6. There seem to be *two distinct kinds* of crystallisation in nature, having widely differing forms. One, the primary kind, in which every element and every combination of elements has a crystalline form *peculiar to itself;* the other, or secondary, the aggregate of many primary forms, in which the crystals are for the most part isomorphous, as recorded by Mitscherlich. Supposing this hypothesis correct, snow and ice are familiar examples in which the difference of the snow crystals would correspond to some difference in the composition of the drops of water of which they are formed. Whatever may be the value of these deductions, there can be no doubt that in order to describe vesicular crystallisations, a very different nomenclature must be employed from that uesd in crystallographical works.

I soon found also, from the extraordinary resemblance of most of these new crystalline forms to those of the vegetable world, that the only glossology applicable to them would be one derived from that used in botany. I therefore propose, with great deference, the adoption of the following terms to enable observers to record their observations until some more complete or better system may be enunciated :—

A. *With reference to the Vesicle.*

1. *Diaphanous*, quite transparent ; (D.)
2. *Diaphanebulous*, slightly clouded, but partially transparent ; (D.N.)
3. *Diaphachromous*, clear, but coloured ; (D.C.)
4. *Diaphanunctuous*, clear, but having a moist or oily look; (D.U.)
5. *Nebulous*, clouded over thickly, crystals not distinguishable ; (N.)
6. *Nebulunctuous*, clouded, and having a moist or oily look ; (N.U.)
7. *Lumenebulous*, clouded, but crystals distinguishable by transmitted light; (L.N.)
8. *Chromenebulous*, in which the vesicle is at first *Diaphanous*, but colouring matter appears in the crystals afterwards ; (C.N.)

B. *With reference to the crystals.*

1. *Zonate*, having rings, like exogenous wood ; (Z.) *Ex.* Some of the metals of Group 3 in chemistry.
2. *Areolate*, an aggregation of zonate crystals, like a tesselated pavement ; (A.) *Ex.* Magnesium.
3. *Acrogenate*, fern shaped ; (Ac.) *Ex.* Cerite, &c.
4. *Aciculate*, like needles ; (Aci.) *Ex.* Antimony.
5. *Campanulate*, like the flower *convolvulus* ; (C.) *Ex.* Soda.
6. *Ciliate*, having fine hairs or fringe at the margin ; (Ci.) *Ex.* Lead.
7. *Discoidate*, like a disc of polished steel ; (D.) *Ex.* Tungstic acid—in Wolfram.
8. *Dendroidate*, like winter trees ; (De.) *Ex.* Silicates of the alkalies.
9. *Discoistellate*, discs having a star in the centre ; (D.S.) *Ex.* Potash.
10. *Digitate*, fingered ; (Di.) *Ex.* Apophyllite.
11. *Filiformate*, thread-like ; (F.) *Ex.* Silicic acid.
12. *Florate*, flower-like ; (Fi.)
13. *Guttate*, small irregular rings ; (G.) *Ex.* Chloride of platinum.
14. *Grammate*, like hieroglyphical letters ; (Gr.) *Ex.* Arsenic.
15. *Hypocrate*, salver shaped ; (H.)
16. *Macculate*, large irregular rings ; (M.)
17. *Palmate*, like the leaves of a palm tree ; (P.)
18. *Plumosate*, feather-like ; (Pl.)
19. *Reticulate*, a net-work of veins; (R.)
20. *Stellate*, like a star; (S.) *Ex.* Silver.

It will be seen that among the examples given I have left some deficient. This has occurred precisely from the want I am now endeavouring to supply—a *systematic* arrangement of observation and record. I wrote every one of the above terms (taken chiefly from "Lindley's Botany") with reference to some particular vesicle, but in some cases I did not apply my new arrangement to the vesicle described, and have now therefore forgotten to which vesicle the example refers ; in others the crystals were not sufficiently developed, and have altered slightly, as in the case of silica, or I have now placed the crystal under another denomination found afterwards, which I consider more expressive of its appearance. Silica at first appeared like a number of short marks or hieroglyphics scattered over the vesicle at hazard, these shortly after grew into elegantly grouped filaments or threads, having something like the appearance of minute branches or twigs of a tree, and I am therefore half inclined to change the term for silica again to "dendroidate."

It is obvious therefore that it is only by the agreement of different observers, *pursuing the same system of observation and using a similar glossology*, that the real value of these crystallisations as analytical agents will be obtainable. I am all but convinced now, not only that these primary crystallisations are isomorphous, but that the slightest change in any of the constituents of a salt will exhibit a corresponding difference in some part of the primary crystallisation. Thus *caustic* potash combined with silica only, clouds over (*i.e.*, crystallises), deliquesces, and indeed vanishes almost immediately after it is formed. In borax it clouds over unctuously, and shows discoistellate crystals. *Carbonate* of potash clouds over, and deliquesces in a very considerably longer time, and the crystals are also distinctly referable to the term *discoistellate*. *Chloride* of potassium clouds over very shortly also, but there is apparently no deliquescence, and no crystals are distinguishable. The first, second, and third of these vesicles I therefore call *nebulunctuous*, the fourth *nebulous*. Again, the only substance the formation of whose crystals I have yet found to colour the previously diaphanous vesicle is ferrocyanide of potassium. A chemical gentleman and myself imagining that the cyanogen could not possibly have been retained under the heat of the blowpipe flame in charging the borax bead, I blew a vesicle containing carbonate of potash, and a saturation of oxide of iron, but the result was a *diaphachromous* instead of a *chromenebulous* vesicle, and the crystals were different, although both exhibited the discoistellate forms of potash.

Thus, if these crystals, as I believe, respond to the very slightest change made in the composition of their component parts, but remain isomorphous *as long as these component parts are inviolably maintained*, it is evident that nothing could be more invaluable to the chemical analyst than reactions so delicate as these, made by the hand of nature herself.

I have now to record a circumstance so remarkable that I almost hesitate to put it on paper, and yet I believe it to be a fact, namely, that most, if not all the metals under Group III. of the chemical arrangement, not only produce,

as I stated in my last paper, zonate discs like sections of exogenous wood, but that these rings *actually grow, each in a certain time, exactly as they do in wood, substituting weeks or days for years, so that the age of one of these crystals can be ascertained like that of a tree.*

I will now, with your permission, make a few remarks regarding *molybdenum*. The powdered ore *molybdenite* yielding on several occasions a *nebulous* vesicle in as short, if not a shorter time than the alkaline earths, I was led to examine this metal more closely than I would otherwise have done. Professor Bloxam says in his valuable work on chemistry (page 393) "The bisulphide of molybdenum (*molybdenite*) is roasted in air *at a dull red heat*, when sulphurous acid is evolved, and *molybdic acid* (MoO_3) mixed with oxide of iron is *left*," but both myself and Mr. Charles, chemical assistant at the R.A. Institution at Woolwich, failed to drive off completely the SO_3 from this ore after ten minutes or quarter of an hour of the strongest roasting in a fierce blowpipe flame. I however obtained in a pipe-clay crucible, from this process, crystals of the so-called "molybdic *acid*." On rubbing these crystals with moistened test-papers, they gave neither an acid nor alkaline reaction. I found a similar account of molybdic acid in Miller's Chemistry to that above given, and going further back (to Brande,) that Scheele, the discoverer of the metal, had employed the bisulphide (*molybdenite*) in his experiments. It then struck me that the SO_3—supposed to be eliminated—had perhaps combined with ammonia and the oxide of molybdenum to form the "molybdenate" of that alkali, and Mr. Charles and I tested the salt so called in the laboratory here, which we found to give a strong sulphur reaction on silver foil, and with barium. I think therefore it would be worth while to re-investigate the properties of this metal.

The following vesicles last made by me, I have laid in a tray according to their chemical arrangement, recorded in the following manner after my system, and I quote them here that they may serve as a guide to intending observers, the results of whose labours I should be glad to see published in the CHEMICAL NEWS.

Vesicles made Feb. 8, 1868, *in Borax.*

GROUP I.

1. *Ammonium*, Chl., 9th,* L.N. (C. 2nd S.)† The two orms appeared almost simultaneously.
2. *Potassium*, caustic, 8th, N.U. (D.S.)
3. *Sodium*, carb., 8th, N.U. (C.)

GROUP II.

4. *Barium*, cl., 9th. D.N. ( ). Crystals not yet distinguishable by pocket lens.
5. *Calcium*, oxalate, 9th, N. ( ) Crystals not yet distinguishable by pocket lens.
6. *Magnesium*, carb. Vesicle not blown till 15th.
7. *Strontia*, carb. Vesicle not blown till 15th.

GROUP III.

8. *Alumina*, corundium. Vesicle not blown till 15th.
9. *Chromium*, oxide, 12th, D. (S. Fl.)
10. *Cobalt*, oxide, 10th, D.C., (D. Fl.) Crystals not distinguishable by *transmitted* light.
11. *Iron*, oxide, 10th, D. (D.Z. 2nd S.) A peculiar blue colour by *reflected* light.
12. *Manganese*, oxide, 10th D.C. (D.Z.) A peculiar blue colour by *reflected* light.
13. *Nickel*, oxide, 10th, L.N. (D.) Appears to deliquesce slightly.
14. *Zinc*, carb., 10th, D. (D.S.)

GROUP IV.

15. *Arsenic*, oxide, 12th, D. (Gr.) Appears to deliquesce slightly.
16. *Antimony*, oxide. Vesicle not blown till 15th.
17. *Bismuth*, oxide, 10th, L.N. (D. Fl., 2nd S.)
18. *Cadmium*, oxide, 11th, D. (Fl.)
19. *Copper*, oxide, 10th, L.N. (Fl., 2nd D.Z.C_2.‡ The first form of crystals transparent—a very unusual thing.
20. *Gold*. No vesicle formed.
21. *Lead*, oxide, 10th, D. (D.Z.Ci.)‡
22. *Mercury*, oxide, 10th, D. $\left(\frac{D.S.}{Fl.}\right)$
23. *Platinum*, chlor., 10th, D. $\left(\frac{G.}{Fl.}\right)$
24. *Silver*, oxide, 12th, D.N. (S.) The crystals in myriads a little larger than those of barium, are distinguishable as the small stars of astronomical nebulæ. The colouring matter round the wire ring is opaline with pink transmitted like the noble opal.
25. *Tin*, oxide, 12th, D. (S.) Crystal stars very few and far between up to date.

Non-Metals.

26. *Silicon*, SiO_2, crystallised after three weeks, D. (De.)
27. *Boron*, sassoline, D.U. $\left(\frac{G.}{S.}\right)$ Both forms of crystals transparent.
28. *Sulphur*. Vesicle not blown till 15th.

Although the crystallisation of these vesicles has naturally first and most occupied my attention, I have not by any means given up the idea (of examining their optical peculiarities) with which they were originally made. Although I have not an apparatus for observing them in polarised light, without which it is impossible to draw many valuable conclusions from their diffractive phenomena, I could easily see in forming the vesicles enumerated above, that the light was refracted much more by some than by others, and indeed Dollond's discovery of achromatism was based upon this principle. I found that *tin* and *silver* were almost the only two oxides which yielded a perfectly diaphanous non-refracting glass. *Lead*, *arsenic*, and *bismuth* were highly refractive, and *sulphur* even more so than these. *Chromium* also appeared to give a clear glass where blown out, but its colouring qualities would of course prevent any useful application of it to glass in this way.

Wishing to ascertain, as I had before observed the development of electricity in vesicles, if metals in this condition assume the electro-positive or negative state, and if this state affects their crystallisation, I blew a vesicle of *zinc* at one end of a piece of platinum wire, and one of *copper* at the other. To the ends of another piece of wire I affixed similarly a vesicle of *iron* and one of *tin*. In the first, the *zinc* crystals appeared within a few hours, of the *campanulate* order, very perfect, and very considerably larger than those of the single *zinc* vesicle; while the connected copper vesicle had comparatively few crystals, and so small that I cannot distinguish their shape. Similarly, in the *tin* and *iron* series, the *tin* crystals appeared disproportionately enlarged at the expense of the *iron* ones, and neither of them bore much resemblance to the typical crystals of single vesicles. If, then, the voltaic current is actually induced between couples of vesicles under crystallisation, it would appear to be in the reverse direction to that passing between the electrified metals, so that copper in this case becomes electro-positive to zinc. Iron to tin.

I have omitted to mention that different metals produce primary crystals of different magnitudes, to express which the Greek letters α, β, &c., might be used.

Woolwich, 15th February, 1868.

[Since the above was written, I have received (on the 17th February) what I cannot but consider as a confirmation of the hypothesis here advanced. I argued that crystallisations formed from *liquids*, as acid solutions, &c., are of the isomorphous (or secondary) form, because they occupy more time in formation, *i.e.*, aggregation of the primary forms, whereas the germs of crystals formed from fusion by fire, must be produced almost instantaneously on cooling, their growth afterwards being the only matter in

* Date of crystallisation.

† When there are two distinct forms of crystallisation, the *second* in time will be shown by a "2nd" prefixed. When the form gradually assumes a second shape, the ulterior will be placed *under* the first as in mercury.

‡ *Discoizociliate*. Compound terms of this kind can be used conveniently.

which *time* is concerned. To prove this, therefore, I plunged a hot borax vesicle (in which unfortunately I had taken up some oxide of *antimony*) into a tumbler of cold water. There was no apparent change at first, and I laid the vesicle on cotton in the usual way. The same evening, I was delighted to find, on examining it with the microscope, indubitable forms of isomorphous crystals—tetrahedra with bevelled edges, hexagonal planes, and a most remarkable combination of the two crystallographical systems in the shape of a flower like a convolvulus, whose petals were formed of the ends of *prisms*. The *rationale* of the experiment seems to be as follows:—The pyrogenous crystallisation is checked by the plunge into cold water, and the borax being soluble, a liquid crystallisation is commenced. Snow crystals being (although formed from a liquid) of the primary form, would appear to be due to the rapidity with which they are frozen.]

ON THE

ESTIMATION OF SULPHUR IN COAL GAS.

By WM. VALENTIN, Esq,

Assistant in the Royal College of Chemistry.

Sulphur is known to exist in coal gas in several forms of combination, the principal of which is bisulphide of carbon (CS_2). By the combustion of gas for domestic purposes a certain amount of sulphurous acid is formed, which diffuses itself into the atmosphere of our rooms, together with the steam which is simultaneously generated, and becomes rapidly converted into sulphuric acid by the absorption of oxygen from the air.

Advantage has been taken of this oxidation of sulphur into sulphur acids by combustion, and methods of quantitative analysis have been based upon it. The best known, and I believe the most generally employed method for the quantitative estimation of sulphur in coal gas is that devised by Dr. Letheby, and which was described in the Chemical News of Feb. 14, 1863.

In my capacity of gas examiner to one of the London gas companies I have had frequent occasion to observe that this method of Dr. Letheby's, which recommends itself, at first sight, by its great simplicity and facility of execution, does not comply with the requirements of a *quantitative* test for sulphur in coal gas, for the simple reason that it never tells, even approximately, *how much* sulphur there really is in gas.

This arises from two causes—viz., imperfect combustion, and consequently imperfect oxidation of the sulphur compounds contained in coal gas; and, secondly, imperfect condensation of the sulphur products of combustion.

Whilst endeavouring to overcome these defects in Dr. Letheby's sulphur test, I first tried various burners that promised to consume the gas and to oxidise the sulphur compounds more perfectly; but I found that I made but little progress, and sometimes obtained even a lesser per centage of sulphur than that given by Dr. Letheby's apparatus. I was more successful in preventing a loss of sulphur arising from incomplete condensation. This loss was, however, not so great as to account for the deficiency in the sulphur indicated by Dr. Letheby's test and that given by more perfect methods, such as the soda-lime process.

Professor Anderson, of Birmingham, who bestowed much attention upon the method now generally employed for estimating sulphur in coal gas,* and who, it would appear, directed his efforts principally towards a more complete oxidation and absorption of the sulphurous products that pass off with the large amount of non-condensable gaseous products of combustion, sums up his results as follows:—

"1. That a single (fishtail) burner, consuming the gas at the rate of 2½ cubic feet in five to six hours, effects its combustion most completely; that even under these circumstances 2½ per cent of the sulphur in gas cannot be burned into sulphurous acid.

"2. That in no case can the sulphurous products of the combustion be wholly recovered where condensing receivers open to the external atmosphere are employed. The best arrangement of apparatus set up on this principle loses 40 per cent of sulphur, and the arrangement given by Dr. Letheby, I find, from the same cause, always entails a loss varying from three-fourths to four-fifths of the bisulphide sulphur in the gas."

At page 49, referring to the corroborative quantitative results obtained by M. Ellisen,† of the Paris Gas-Works, and by Mr. Evans, the engineer of the Chartered Gas-Works, Professor Anderson again states "that by the employment of the 'Leslie' jet and open receivers not more than from one-fourth to one-fifth of the bisulphide sulphur of coal gas can be estimated."

After failing in various attempts to convert the sulphur compounds entirely into sulphuretted hydrogen, by passing the gas together with steam over heated copper, &c., my endeavours were mainly directed to secure complete combustion of the gas, so as to obtain all the sulphur impurities in the form of sulphuric acid, and not as sulphurous acid, as I had observed how difficult, or rather how impossible, it is completely to oxidise a small quantity of sulphurous acid diffused throughout a large amount of gaseous products of combustion, and to absorb it by passing these products through various oxidising solutions.

There is, as is well understood, a definite amount of air required to completely burn coal gas, and to convert it into its two principal ultimate products of combustion—carbonic acid and water (steam). Sulphur compounds are oxidised readily into sulphurous acid; complete oxidation to sulphuric acid, however, is effected with much difficulty only, as will appear hereafter. It appeared to me, then, that a mode of combustion which supplied to the gas that amount of atmospheric oxygen which is requisite to cause complete combustion (or a slight excess of atmospheric air even), and which brought the gaseous particles into the most intimate contact with the oxygen of the air, at a high temperature, during their passage over a highly porous material, such as spongy platinum, known to possess that power in the highest degree, would most effectually accomplish the object in view.

On testing this theory by experiment, I found that complete combustion was effected by causing the gas to pass, together with an adequate amount of air, through a porcelain tube strongly heated in a small Hofmann's gas combustion furnace. Gas and air are mixed just before they enter the tube, and are made to pass slowly over ignited spongy platinum, loosely packed in a platinum cage made of a sheet of fine platinum gauze, which completely fills the tube. With an insufficient amount of air—about three parts of air to one of gas—carbonic oxide is mainly formed, and the sulphur in the gas is converted chiefly into sulphurous acid, part of which resolves itself into sulphuretted hydrogen on passing along with the steam over the ignited spongy platinum—

$$SO_2 + OH_2 = SH_2 + O_3$$

yielding, in fact, oxygen to aid in the combustion of the carbon compounds of the gas.

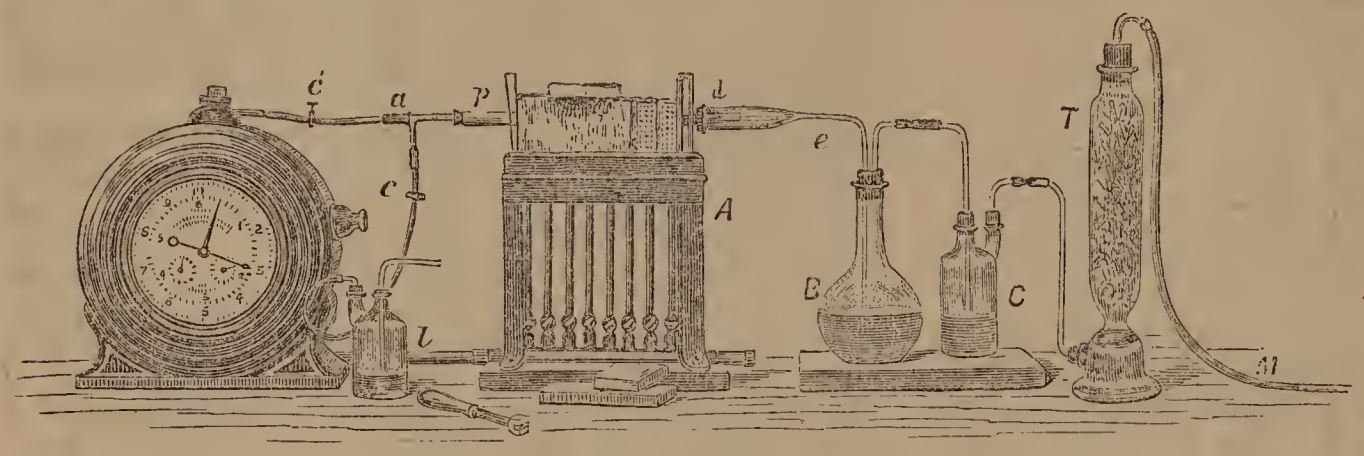
A vivid combustion takes place in the anterior portion of the tube, just where the mixture of gas and air first comes into contact with the spongy platinum. On passing the gaseous products of combustion through several Woolfe's bottles and towers containing powerful oxidising solutions, such as chlorate of potash and hydrochloric acid, and lastly through a solution of pure soda and through distilled water, I satisfied myself that it is

* "On Defects in the Apparatus generally used for the Determination of Bisulphide of Carbon in Coal Gas." A reprint from the *Journal of Gas Lighting*, p. 53.

† "Sulphur in Coal Gas." Report by Thos. G. Barlow, C.E., and Albert Ellissen, Chief of the Experimental Works of the Paris Gas Company.

extremely difficult to completely oxidise and absorb the sulphur product of the combustion, since I almost invariably found traces of sulphurous acid in the last tower containing distilled water only. It was evident that no reliance could be placed upon the oxidising and absorbing power of the various solutions. Gaseous sulphurous acid is not oxidised nor retained so readily, when mixed and diffused throughout an overwhelming amount of other gases, as is generally supposed, and it became, therefore, necessary to modify the mode of analysis at first adopted. This is a fact of great importance, since it throws light upon the discrepancies observed by various chemists in the results obtainable by Dr. Letheby's apparatus.

After various alterations I fixed at last upon the apparatus represented in the following figure.



A is a small gas combustion furnace, containing three perforated clay burners in a row or line. Eight or ten rows of such burners suffice. On the low burners of the middle row rests a Berlin-ware porcelain tube, *p*, capable of resisting a high degree of heat and rapid changes of temperature. This porcelain tube, *c*, is 12 inches long, and has a diameter of half an inch. It is best embedded in thin layers of asbestos, spread out in a tinned iron trough, to prevent the direct action of the gas-flames upon it. A platinum tube, made of fine platinum gauze, is made to fit tightly into the porcelain tube. It need not be longer than from 5 to 6 inches. One end is closed by causing the platinum gauze to overlap, and the tube can then be filled with spongy platinum, and when closed at the other end and fastened together at short distances with thin platinum wire, is ready to be introduced into the porcelain tube A tight fitting cork fixes a narrow glass tube, *a*, drawn out to a point, into the anterior part of the porcelain tube. The latter reaches far enough out of the furnace, and the flow of cold gas and air mixed keeps this part of the tube sufficiently cool to render slight explosions in the anterior part of the porcelain tube and in the glass tube of rare occurrence, Although harmless enough in themselves, they may be entirely avoided by admitting a slight excess of air over that required to completely burn the gas. The gas is supplied through one leg of the short bifurcated tube, and the air through the other. Both gas and atmospheric air are measured by being passed through meters of sufficient capacity to register from 5 to 10 cubic feet of gas per hour. The air may be passed through a solution of acetate of lead, contained in a small two-necked Woolfe's bottle, *l*, to deprive it of any trace of sulphuretted hydrogen before it enters the meter. Compression cocks, *c c'*, regulate the flow of the gas and air.

Over the posterior end of the porcelain tube is fitted an adapter-tube, *d*, drawn out and joined on to a narrow glass tube, *e*. The narrow end of the tube is bent at right angles, and fits tightly into a perforated cork, so as to deliver the gaseous products of the combustion into a solution of pure caustic soda, made entirely free from sulphuric acid, by burning sodium under water, such as is obtained now in commerce from the Magnesium Company, Manchester. From 10 to 15 grammes are a convenient quantity to be employed. This solution of caustic soda may be placed into a two-necked Woolfe's bottle, or, as shown in the drawing, into a small flask, B, capable of holding about a pint of liquid, fitted with a doubly perforated cork. If sufficient caustic soda is present in the flask, the whole of the sulphuric acid—for such only is obtained when the combustion is properly conducted—is retained in the first liquid. The greater portion of the sulphuric acid is even found to condense in the adapter-tube, *d*, and may be washed out, and estimated separately.

From the flask containing the caustic soda the gaseous products may be passed through a second flask or through a two-necked Woolfe's bottle, containing a few grammes of chlorate of potash, and moderately dilute hydrochloric acid; and from this into a third, containing a little pure carbonate or caustic soda. Any trace of sulphurous acid is thus oxidised into sulphuric acid, and is retained in the various solutions. And, lastly, the gases are passed through a tubulated cylinder containing a column of a few inches of distilled water, and in its upper part large pieces of broken glass, offering a large moist surface to the gases, and from the top cork, through a bent tube, towards the aspirator M.

I find, however, that when the mixture of gas and air is properly adjusted, *the whole of the sulphuric acid is retained in the first flask*, B, and that the Woolfe's bottles containing the oxidising solutions and the alkali may be entirely dispensed with, retaining for precaution's sake merely one Woolfe's bottle, C, containing a few grammes of pure soda solution, and the condensing-tower, T, as shown in the drawing.

It is, of course, out of the question to drive gases through a series of solutions offering a resistance of a couple of inches of water pressure, without the aid of an aspirator. When a plentiful water supply can be obtained, an aspirator may be used, constructed on the principle of the Catalonian water-blast, with a fall of water of from 8 to 10 feet. Another convenient aspirator, which, moreover, strongly recommends itself on account of its economical use of water (bulk for bulk of air, or nearly so), was devised, some short time ago, by my colleague, Mr. M'Leod, and is described in the *Journal of the Chemical Society*, March number, 1867, page 164.

Perhaps the most simple and convenient means of aspirating air consists in using a 5 or 10 cubic feet gasholder, employed exhaustively. When full the combustion may be temporarily interrupted, till the products of combustion have been discharged from it. Such gasholders are generally found in large gas works, and are used for testing gas meters. Thus any loss of water will be altogether obviated.

The flow of the gases is best regulated by means of a gas-tap, connected with the india-rubber tube leading to the aspirator from the condensing-tower.

The combustion must at all times be so regulated as to cause the products of combustion to pass off without showing a peculiar white smoke or cloudiness within the condensing-flasks. This is effected by having about ten times as much air as coal gas. It is quite possible to burn from 0·5 to 0·6 of a cubic foot of gas per hour, and, as only 2 or 3 cubic feet of gas have to be burnt to obtain a sufficient amount of sulphuric acid for a correct estimation, one is enabled to conduct and finish an estimation during a time of the day when the chief consumption of gas takes place—viz., in the evening.

The following tables contain the results of a series of experiments, conducted at the Laboratory of the College, upon ordinary coal gas supplied by the Chartered Gas Company:—

Table No. I.

Date of Experiment.	Cubic Feet of Gas burned during the Experiment.	Amount of Gas burned per Hour.	Sulphate of Barium obtained, in Grammes.	Sulphur per 100 Cubic Feet of Gas, in Grains.
April 16	2·3	0·48	·2822	25·98
17	3·3	0·73	·4240	27·21
18	3·2	0·71	·4680	30·95
20	3·2	0·80	·418	27·92
23	5·	1·10	·627	26·55
24	3·2	0·96	·430	28·45
27	5·8	0·93	·8375	30·58
30	3·9	0·78	·632	34·33
May 15	3·7	0·85	·4623	26·58
17	4·	0·89	·7283	38·56
20	5·	0·79	·5967	25·27
21	5·	0·94	·5806	24·59
	*4·5	1·00	·260	12·23
22	5·	0·95	·5595	24·03
	*6·	0·90	·3902	13·78
23	5·1	1·18	·6285	26·07
	*6·5	0·93	·501	16·32
24	6·5	1·24	·683	24·19
	*7·75	0·97	·6545	17·91
25	7·2	1·07	·875	25·75
28	8·8	1·34	·975	23·48
	*6·5	0·93	·543	17·70
29	4·1	0·75	·5365	28·73
June 27	2·75	0·42	·280	21·60
July 1	3·5	0·64	·5778	34·87
2	3·5	0·58	·347	20·98
3	4·5	0·66	·462	21·62
4	3·25	0·65	·382	24·89
	*4·25	0·70	·2741	13·6

* This mark indicates the analysis made simultaneously by the Letheby apparatus.

Table No. II.

July 5	2·75	0·51	·2705	20·00
	†2·85	0·88	·3814	28·35
6	3·	0·64	·334	23·50
	†3·02	1·00	·3818	26·79
8	3·5	0·56	·4525	27·37
	†3·4	0·97	·5224	32·56
9	†4·6	0·84	·6971	32·14
10	3·	0·60	·3278	24·05
	†4·8	0·91	lost	—
11	3·	0·50	·3457	24·32
	†5·1	0·98	·7770	32·25
12	3·	0·53	·3585	25·32
	†5·	1·00	·7057	30·00
13	1·5	0·52	·2203	31·09
	†2·65	0·93	·4252	33·95
Sept. 18	†5·0	0·83	·7422	31·46
20	2·55	0·50	·353	29·34
	†4·5	0·90	·7167	33·74
21	1·3	0·40	·1815	29·58

† This mark indicates the analysis made simultaneously by the soda-lime process.

On half-a-dozen occasions an experiment was carried on simultaneously with the apparatus devised by Dr. Letheby, and it will be seen that the results are considerably below those which were obtained at the same time by the new method; by far not so low, however, as Professor Anderson states the loss to be.

There can be little doubt that the so-called *lime process* is by far the most perfect method of estimating sulphur in coal gas which can be found. It consists in passing the gas over lime (best soda-lime) loosely packed in a combustion tube of hard glass, and heated strongly from the outside in a gas combustion furnace.

Unless, however, *pure* lime and *pure* soda (free from sulphuric acid) are used, little reliance can be placed upon the process. The glass, moreover, is acted upon to a disagreeable extent, silica being dissolved out; and unless the gas be sent through the tube at a very slow rate, much carbon is deposited. The sulphur products also require oxidation after being dissolved out, and it needs no little experience in chemical manipulation to steer clear of all these drawbacks to an otherwise excellent method.

In order to check the above results by those obtained with the soda-lime process, I prepared some perfectly pure soda-lime by calcining marble, and slacking the pure caustic lime so obtained with a solution of pure caustic soda, and I thus succeeded in getting a soda-lime which was perfectly free from sulphuric acid. In order to avoid the action of the alkalies upon the glass, I used a narrow-bore gun-barrel, coated over with fire-clay made into a stiff paste by means of starch solution, or solution of British gum, and dried, previous to being placed on the furnace. In this manner I succeeded in getting, to a great extent, over the above-described shortcomings of the method.

It will be seen from Table II. that the results obtained by the soda-lime process were invariably somewhat higher than those obtained by combustion of the gas over spongy platinum. I convinced myself that this arose from a slight loss of sulphuric acid, on account of its being retained by the spongy platinum, and condensed on the inside of that part of the porcelain tube nearest to the adapter-tube. It is, therefore, advisable to invariably wash out with distilled water both the porcelain tube and the cage of spongy platinum. The latter appears to retain the sulphuric acid with great pertinacity, and it requires repeated digestion with hot distilled water, slightly acidulated with hydrochloric acid, before the sulphuric acid can be dissolved out entirely. The cage of spongy platinum must be dried and ignited before it is put again into the dry porcelain tube.

If it were not for the difficulty and extreme tediousness with which the soda-lime process is attended, there can be little doubt that it would deserve the preference over any process known at present for estimating sulphur in coal gas.*

It has been my endeavour to provide the practical gas engineer with an apparatus for estimating the sulphur in gas which is easily manageable, and which requires but little supervision when once set going; also to obtain the sulphur at once in the shape of sulphuric acid without the aid of oxidising agents, such as nitric acid, bromine or chlorine water, chlorate of potash, and hydrochloric acid, solution of hypochlorites, &c. The soda solution contained in the flasks is simply rinsed out into a beaker. The adapter-tube, as well as the porcelain tube and spongy platinum, are carefully rinsed out with distilled water; the liquid is acidulated with hydrochloric acid, the whole heated to ebullition, and the sulphuric acid precipitated by means of chloride of barium as sulphate of barium. The precipitate is filtered off, washed, dried, and weighed in the usual manner.

Since writing these lines for publication in the *Journal of Gas Lighting*,† I have made further experiments with a view of combining the advantages of the combustion method by means of spongy platinum with those of the soda-lime process. By introducing a few grammes of pure soda-lime into a short platinum tube, about 4 inches in length, made of thin sheet platinum, and placing the tube so charged into the porcelain tube so as to cause the gas and air first to pass over the spongy platinum, and then over the ignited soda-lime, I succeeded in fixng the principal amount of the sulphuric acid produced by the combustion of the gas as sulphate of sodium and calcium. I have, as yet, not been able to fix and retain the whole of the sulphuric acid, within the porcelain tube, so as to dispense entirely with the solution of pure caustic soda in flask B., but have little doubt that by substituting a pla-

* The results obtained by the lime process by M. Albert Ellissen, of the Paris Gas-Works, and given on page 16 of the reports on the sulphur compounds present in coal gas, by Mr. Thomas G. Barlow, C.E., and M. A. Ellissen, differ so widely one from another that I am inclined to think there must be some error. I have always found the amount of sulphur obtainable by the soda-lime process, as well as by the combustion process described, to vary but little from day to day

† January 7th, 1868.

tinum tube for the porcelain tube, and by employing a somewhat larger amount of soda-lime packed directly into the platinum tube, so as not to give to the gaseous products of the combustion a chance of passing off between the porcelain tube and the small platinum tube, by means of which I now introduce the soda-lime into the porcelain tube, without being brought into contact with the alkaline absorbents, I shall succeed in retaining every trace of the sulphuric acid formed.

I subjoin a few results obtained by this modified process of combustion.

Date of Experiment.	Cubic Feet of Gas burned during the Experiment.	Amount of Gas burned per hour.	Sulphate of Barium obtained in Grammes.	Sulphur in 100 Cubic ft. of Gas in Grains.
January 28th	2	·36	·352	37·10
29th	3	·52	·474	33·42
31st	2·8	·50	·503	38·05
February 3rd	3·3	·55	·522	33·52

In the last experiment the sulphuric acid was estimated separately in the portion of liquid derived from the soda-lime, and the washings of the spongy platinum; it amounts to 29·39 grains, whilst the sulphuric acid collected in flask B., by means of solution of pure caustic soda, amounted to 4·128 grains or 14 per cent of the total sulphuric acid formed.

The advantage of merely having to dissolve out the alkali with dilute hydrochloric acid without having to remove the cage of spongy platinum, is quite obvious, and as the soda-lime is obtained perfectly free from carbon-particles or from sulphide of calcium or sodium, the solution can without any previous oxidation or filtration be precipitated directly with solution of chloride of barium.

I hope in a future communication to be able to give you the results of such modified combustion in a platinum tube.

It is obvious that the process of combustion over spongy platinum is applicable to other gaseous mixtures containing sulphur compounds, such as the volatile products, which escape during the process of incineration of various vegetable or animal matter, containing sulphur and phosphorus in combination with albuminoids, as well as in that of metallic sulphates and phosphates There is, at present, no process known by which the *volatile* sulphur and phosphorus in albuminoid substances, can be ascertained with anything like satisfaction, *independently* from the sulphur and phosphorus, which is determined in the ash. I have, before this, tried slow combustion of such organic bodies, *ex. gr.*, wheat, flour, coal, &c., &c., in a current of air or oxygen, passing the products of combustion into bromine water and pure alkali, and obtained results which lead me to think that our knowledge of the amounts of sulphur which is present in grains, for instance, is very imperfect, and that a reliable process for the estimation of the albuminoid sulphur and phosphorus, in contra-distinction to the sulphur and phosphorus present as sulphates and phosphates. would be a great desideratum.

I hope shortly to be able to throw some light upon this important subject.

Determination of Nitric Acid.—C. Nöllner. In the manufacture of potassic nitrate from Chili saltpetre liquors are obtained in which from the presence of large quantities of foreign salts the estimation of nitric acid by any of the methods commonly employed is almost impossible. The author therefore proposes the following new method. About one gramme of the solution or salt to be tested is gently heated with a concentrated solution of ammonic sulphate; absolute alcohol is added, and thus all salts are precipitated with the exception of ammonic nitrate. The latter after filtration is precipitated with an alcoholic solution of potassic hydrate (free from silicic acid), and the potassic nitrate washed with alcohol, dried and weighed.—(*Zeitschr. Analyt. Chem.* vi. 375).

ON

HEAT AND COLD;

A COURSE OF

SIX LECTURES*

(*Adapted to a Juvenile auditory*),

DELIVERED AT

THE ROYAL INSTITUTION OF GREAT BRITAIN,

(*CHRISTMAS*, 1867—8),

BY

JOHN TYNDALL, Esq., LL.D., F.R.S.

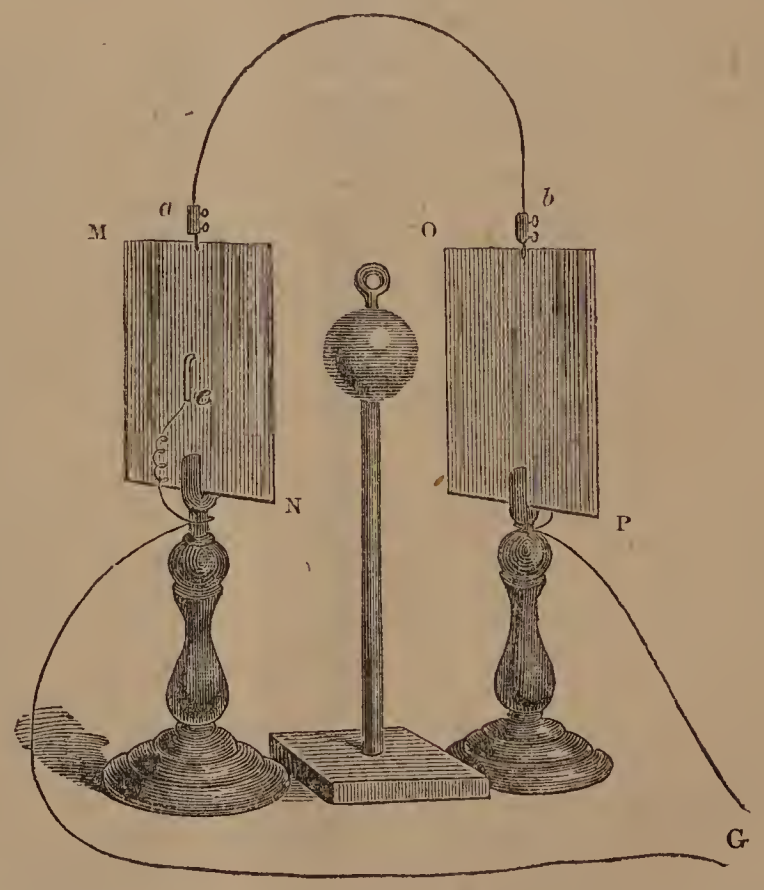
Lecture V.

(Continued from page 79.)

Radiant heat.—Reflection and absorption of radiant heat.

You see I have here two sheets of tin, M. N. and O. P., one covered with lamp-black, and the other uncovered. I place them facing each other, and I put this stand exactly midway between them. Now, I have a little device here—a tell-tale—which will inform me which of these plates is heated. Suppose I heat this plate. Observe what occurs at the magnetic needle. I simply warm that plate by putting my finger upon it. The red end of the needle moves towards me. I cannot explain the wonderful power which moves the needle. It is what we call an electric current, and is produced by the union of the two metals of the thermo-electric pile. When the plate is heated you see

FIG. 2.



that a deflection of the needle is produced. The needle will return to zero when I withdraw my hand. I want you now to judge which of these two surfaces absorbs radiant heat most freely. The needle will not rest at zero unless these two plates are exactly at the same temperature. If one becomes warmer than the other the needle will deviate from zero. Thus we have it in our power to determine which plate absorbs heat most greedily. Now Mr. Cottrell will give me a ball of copper which is heated to redness. You observe it is radiating its heat as a luminous body radiates light. [The red hot copper ball was placed equidistant between the two plates of tin, one of which was coated with lamp-black. In a few seconds the needle

* Reported verbatim, by permission of the Author, for this Journal.

of the pole began to travel from the zero.] Thus we prove that this surface coated with lamp-black, which is the best radiator, is also the best absorber. We might experiment with a variety of substances in this way, and prove that great differences exist as regards their absorptive powers.

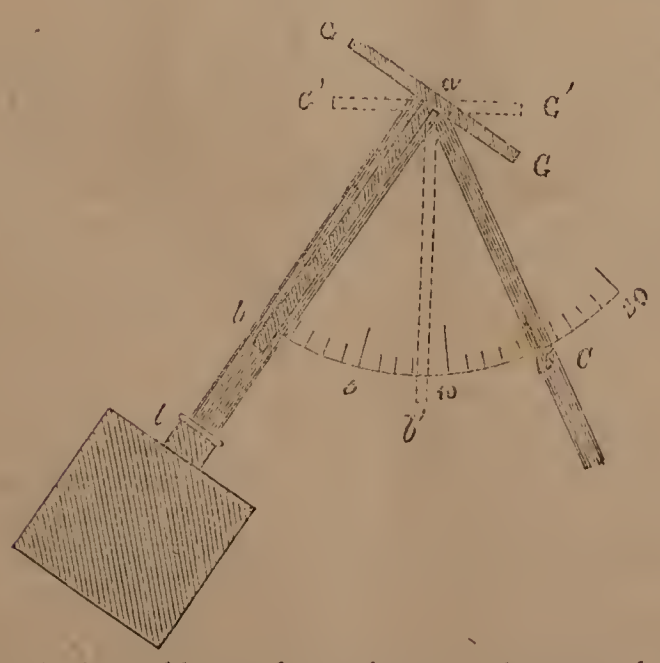
It is very wonderful what a slight and trivial thing will be sufficient to prevent the absorption of radiant heat. I have here an exceedingly instructive substance. It is a piece of paint given me by Mr. Hills, of the firm of Bell

Fig. 3.

and Co. A portion of this paint is coated with gold leaf, and though the gold leaf is infinitesimally thin, it has been competent to protect the surface of the paint from the action of radiant heat to which the whole thing was exposed, while the other part of the surface, which was not covered with gold leaf, has become blistered. Where the gold leaf was present it prevented the rapid absorption of the heat.

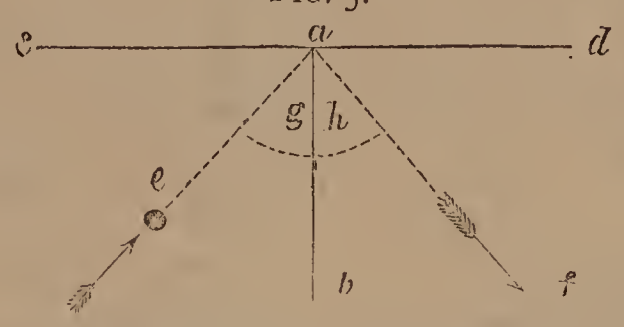
I have here a sheet of paper covered on one side with iodide of mercury, a substance which has its colour discharged by heat. On the other side of the paper there are certain figures represented by a thin coating of metal. I place the paper with the iodide of mercury side downwards; and over the other side I will hold a hot spatula which will radiate heat to the surface of the paper. Where the thin coating of metal is, the heat will be rejected, but where the paper is not coated the heat will be absorbed, and then it will reach the iodide of mercury on the other side and destroy its colour. You will find that in this way we shall produce on the underside of the paper a perfect picture of the figures on the upper side, for you will find that the red colour of the iodide of mercury will remain underneath the metal coating, for that coating has the power of rejecting the heat as the gold leaf rejected the heat in the other case, and so protected the paint and prevented its blistering. [The experiment was performed with a successful result.]

Fig. 4.



The radiation of heat obeys the same laws as the radiation of light, and it obeys the law of reflection due to light. This we can illustrate by means of our beautiful thermo-electric pile; but I will first of all make a single experiment that shall impress upon our minds the law according to which light is reflected. It is a very simple experiment, but I trust it will be very effective as far as regards the proof of the law. Mr. Cottrell, who knows my requirements very well, is now placing there in front a little looking glass, G G. I intend to send a beam of light, *a b*, from the electric lamp, *l*, towards the mirror G G. The beam will strike upon the mirror, and be reflected. How? So that the reflected beam will lie as much on the left of this index, *a b*, which is perpendicular to the mirror, as the direct beam lies upon the right side of it. There are two terms employed in connection with this subject which the elder boys ought to remember. This angle, *g*, made between the perpendicular, *a b* (Fig. 5), and the line, *c a*, along which the direct ray goes to the mirror, is called the

Fig. 5.



angle of incidence. The angle, *h*, between the perpendicular *a b*, and the reflected ray, *a f*, is called the *angle of reflection*; and the law as regards both light and heat is this—that "the angle of incidence is equal to the angle of reflection." If I am right in what I have stated you will find the reflected beam as far from the perpendicular on one side as the direct beam is from the perpendicular on the other side. I want now to prove the same with regard to radiant heat by a very rough experiment, and show you that it obeys the same law as light. I take this piece of tin, which will reflect heat, and hold it so that the radiant heat from this fire will fall upon it, and then be reflected, according to the law I have just mentioned, on to the face of the pile. I have no doubt that reflected heat will warm the face of the pile, and cause the needle to move towards me. We thus see that heat exhibits the same law in this respect as light.

I wanted to make one or two experiments more, and I wished to do so, as before, by means of our thermo-electric pile; but I find that the needle does not act freely although the pile does its duty. Hence I think I must tell you by my tongue what that needle, if it were in a proper condition, would have told you by its motion. I intended to make the needle my voice, but it has become dumb. I wanted to show you that this thing we call radiant heat passes in very different degrees through different bodies. I wanted first to compare the passage of heat through glass with its passage through other bodies. I have here a piece of rough glass, and I have also a beautiful substance—a very common one, but to me more precious than the diamond, though the diamond is a beautiful thing. This substance is rock salt. This would allow heat to pass through it with perfect freedom, while the glass would cut it off. So with different liquids. I have here a liquid called bisulphide of carbon, and here I have some of the well known liquid called water. If I filled one cell with water and another with bisulphide of carbon, I should find that the bisulphide of carbon would transmit heat with great freedom, while the water would not transmit it at all. Water is, indeed, as regards heat, one of the most opaque bodies in nature to all but incandescent or luminous heat. It is a perfectly opaque body to all rays emitted, say from the surface of a boiling kettle, or from the heated cube, or from the cheek of the young philosopher who helped me in an experiment in the early part of this lecture. During the burning of Her Majesty's Theatre the heat struck upon the windows of a club house opposite, and as the glass would not allow the heat to pass through, the windows became hot, and thus the glass was broken. Had

Fig. 6.

those windows been composed of rock salt the heat would have passed through them, and they would have remained perfectly cool, although there might have been an efflux of the most powerful radiant heat. If time allows, I will show you in the next lecture that we can boil water by radiant heat passing through bisulphide of carbon, though the same heat does not boil the bisulphide of carbon through which it is transmitted, notwithstanding that bisulphide of carbon boils at a lower temperature than water.

I have told you that different bodies, both solid and liquid, possess the power of transmitting heat in different degrees. Now, the body which absorbs the radiant heat, instead of transmitting it, becomes warm by the absorption. Ice is a body which is exceedingly opaque to the rays of heat, but allows light to pass through with freedom. I intend to place a piece of ice in the path of a beam from the electric lamp, and which will be a mixed ray of heat and light. The ice will stop by far the greater portion of the radiant heat, and the heat will be lodged within the ice. But the temperature of ice cannot be raised beyond 32° Fahrenheit without the ice beginning to melt, so that the portion of the beam arrested by the ice will occupy itself in liquefying the interior of the ice. It will liquefy the ice internally, and I want you to see the wonder and the beauty involved in this beautiful substance which you skate over every winter, but, perhaps, never think of. This beam of light and heat passing into the ice will dissect the ice and separate the crystals, and you will see the beautiful figures into which the ice resolves itself. The ice will break up internally into most beautiful flowers consisting of six petals. In order to enable you to see these figures I must magnify them very much, and for that purpose I shall cause an image of them to be thrown on this large white screen. The lamp is placed in the gallery to increase the distance from the screen, and so make the figures appear larger. Mr. Cottrell has a lens there, and he will now take a piece of ice, and make the surface smooth by putting it on a warm body, and then place it in the path of the beam. The ice has been cut parallel to the plane of freezing from a block of the so-called Wenham Lake ice. It has been cut, I say, parallel to the surface along which the ice grows. [After a short time the image of the ice-flowers began to appear on thes creen.] I do not know any experiment that I have ever made which is more delicate and beautiful than this. The flowers are growing larger and larger. First of all you see these leaves, and within you see a crimping. Those spaces which you see are spaces entirely devoid of air, for you know that the water occupies less space than the ice. The ice is larger than the water which formed it, and as the inner portions of this piece of ice melt, the water occupies less space than the ice, and a small vacuum is produced at that spot. This screen presents a glorious surface of ice-flowers. Every particle of ice is built up in this beautiful way. The ice has now become disintegrated, but I do not think your patience has been ill rewarded.

Detection of Aniline in presence of Toluidine.—A. Rosenstiehl. Chloride of lime produces a blue colour with aniline, and a brown one with toluidine, but a mixture of the two only shows the latter reaction. If, however, ether is added the brown substance is taken up by the latter, and the blue colour of the aqueous solution becomes visible. The test as proposed by the author therefore is: Dissolve the base in ether, add an equal volume of water, then, drop by drop, a solution of chloride of lime, shake, and observe the colour of the aqueous layer.—(*Zeitschr. Analyt. Chem.* vi. 357.)

Analysis of Silicates.—R. Hofmann. Silicates in which the alkaline metals are to be determined and which are not decomposed by acids, may be brought into solution by the combined action of ammonic fluoride and sulphuric acid. The finely powdered mineral is mixed with three or four times its weight of the fluoride, moistened with sulphuric acid, and the whole gently heated on the water-bath, finally over a flame to expel excess of sulphuric acid. The dry residue is dissolved in chlorhydric acid, and proceeded with as usual.—(*Zeitschr. Analyt. Chem.*, vi. 366).

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, February 6th.

Dr. Warren de la Rue, F.R.S., &c., President, in the Chair.

In continuation of our report of this meeting we have now to give an account of Dr. W. J. Russell's lecture, "*On Gas Analysis.*" The apparatus in its modified form as now employed by Drs. Wililamson and Russell, was exhibited in the meeting room. It consisted of a wooden table on which was mounted a cast-iron mercurial trough of simpler form than that formerly described by Dr. Russell, and figured in the Chemical News, vol. ix. p. 282. The "laboratory tube," or vessel C, is dispensed with, and all the absorptions are conducted in the same tube as that in which the gases are afterwards measured. The pressure tube A, containing a standard volume of air, is retained, and all measurements are observed at uniform temperatures and at the same level. Dr. Russell described a handy little contrivance which enabled him to introduce potash and other reagents into the gas tube without admitting air or interfering with the volume of gas. This little instrument consists of an iron or steel wire, No. 9 or 10, passed through a crooked piece of glass tube and having one extremity roughened for the purpose of enabling it to hold firmly a tuft of moistened cotton wool. The glass tube $\frac{1}{8}$ inch in diameter being used as a director, the wire is pushed forward, underneath the mercury, until the cotton tuft rises above the level of the quicksilver in the absorption tube. By kneading the cotton wool in water, every trace of air could be expelled, and then the water could be displaced by potash or other solution; a little grease applied lubricated the passage of the wire through the glass tube. A number of analytical details were then given in proof of the accuracy with which a number of operations of this kind could be conducted without sensible alteration of the volume of gas. Carbonic acid introduced and then absorbed by potash (one part of saturated aqueous solution of hydrate of potassium mixed with two parts of water), caused no error, and five parts of such solution absorbed about 80 of carbonic acid. Oxygen could be easily removed by the same alkali, into which a few drops of pyrogallic acid was passed up. Olefiant gas and other hydrocarbons must be attacked by Bunsen's coke-balls, since the strong sulphuric acid would destroy the cotton; the lecturer thought, however, that gun-cotton might be used. In coal gas analysis this apparatus worked exceedingly well, and for hydrogen and other eudiometrical purposes the method of explosion was resorted to in a supplementary wooden trough, to which the eudiometer was transferred in a suitable transfer-spoon. The spindles and catgut adjustments of the old apparatus were retained, and likewise the mode of illumination and the sliding support for the telescope. Alterations of the level of mercury could either be effected by pouring in the liquid metal, or a stout glass tube sliding through a caoutchouc collar could be depressed into the cistern.

The President, in moving a vote of thanks to Dr. Russell, took occasion to notice the ingenious character of the contrivances made use of in several parts of the apparatus.

Dr. Frankland spoke in approval of the whole apparatus, and inquired whether the lecturer had constructed and used a reduced model.

Professor Wanklyn found that a piece of india-rubber tube used as a casing, overcame the objection of fragility usually ascribed to Frankland and Ward's apparatus.

Mr. Maxwell Lyte asked whether the use of soda instead of potassa was admissible?

Dr. Russell said that the efflorescent character of this alkali and all its salts was objectionable, as tending to soil the tubes. In reply to Dr. Frankland, he would simply affirm that very small volumes of gas could be manipulated in the present apparatus, since by raising the tubes high above the level of the mercury in the cistern the volume of gas might be read off when greatly expanded.

A paper "*On some New Benzylic Derivatives of the Salicyl Series*," by Mr. W. H. Perkin, F.R.S., was then read by the Secretary. The author had examined the actions of chloride of benzyl upon the hydride of sodium-salicyl and gaultherate of sodium (sodium salicylate of methyl), respectively, and succeeded in obtaining bodies representing the salicylic aldehyde and acid, in which the phenolic hydrogen is replaced by benzyl. These new products have been named the *hydride of benzyl-salicyl*, and the true *benzyl-salicylic acid*. Combustions of these substances were made, and the ammonium, silver, mercury, lead, and copper salts of the latter were prepared and analysed. Their formation was thus explained—

$$\text{I.}\quad \left[\begin{matrix}\text{CO,H}\\ \text{C}_6\text{H}_4\\ \text{Na}\end{matrix}\right\}\text{O} + \text{C}_7\text{H}_7\text{Cl} = \left[\begin{matrix}\text{CO,H}\\ \text{C}_6\text{H}_4\\ \text{C}_7\text{H}_7\end{matrix}\right\}\text{O} + \text{NaCl}.$$

Hydride of sodium-salicyl. — Hydride of benzyl-salicyl.

The hydride of benzyl-salicyl is a colourless viscid oil having an odour like that of cloves, and boiling at a point above the range of the mercurial thermometer. It is possessed of aldehydic properties, although combining slowly with alkaline bisulphites.

$$\text{II.}\quad \left[\begin{matrix}\text{Me}\\ \text{CO}\\ \text{C}_6\text{H}_4\\ \text{Na}\end{matrix}\right\}\text{O} + \text{C}_7\text{H}_7\text{Cl} = \left[\begin{matrix}\text{Me}\\ \text{CO}\\ \text{C}_6\text{H}_4\\ \text{C}_7\text{H}_7\end{matrix}\right\}\text{O} + \text{NaCl}.$$

Sodium-salicylate of methyl. — Benzyl-salicylate of methyl.

The benzyl-salicylate of methyl was then decomposed by boiling with alcoholic hydrate of potassium, when wood-spirit was evolved; and the potassium-salt in aqueous solution, then treated with hydrochloric acid, furnished the new acid in the form of an oil which slowly solidified to a mass of minute plates. Its fusion point is 75° C.

Benzyl-salicylic acid has the following composition—

$$\text{C}_{14}\text{H}_{12}\text{O}_3 = \left[\begin{matrix}\text{H}\\ \text{CO}\\ \text{C}_6\text{H}_4\\ \text{C}_7\text{H}_7\end{matrix}\right\}\text{O}$$

A vote of thanks having been passed both to Dr. Russell and to Mr. Perkin the meeting was adjourned as already reported.

FOREIGN SCIENCE.

Paris, Feb. 18, 1868.

Essays for the Prize of the Société de Pharmacie.—Detection of Kreatinine in Urine.—Manufacture of Pyrogallic Acid.—Academy of Sciences: Propagation of Waves through Gaseous Media.—Manufacture of Charcoal, and Metallurgy of Iron.—Dialysis of Induction Currents.—Beet-root Fermentations.

Every year the Société de Pharmacie offers a prize for the best essay upon some subject connected with pharmacy. This year the announcement of the examiners' decision was enhanced in interest by a speech from M. Coulier, (reporter of the examining commission), in which he reviewed the work of the candidates. The detection of arsenic in cases of poisoning was the subject of an essay by one of the competitors, M. Aly-Read. The author had made experiments to determine exactly the temperature at which sulphide of arsenic is decomposed by sulphuric acid. Another competitor, M. Barret, chose for his subject a study of the preparations of opium described in the Codex of 1866. He sought to determine the causes which influence the proportions of morphine contained in different varieties of opium. Methods were given by which the amounts of morphine and narcotine might be

estimated. The study of arsenic formed the basis of an essay from M. Dupuy. The first part related to the history of this element, taken first in a purely chemical aspect, then as a toxic agent. The second part contained the results of experiments upon the absorption and elimination of arsenical compounds. M. Dupuy states that an ordinary bath containing an amount of arseniate of soda up to 20 grammes will not affect a man. M. Eberlin sent an essay devoted to the chemical study of glycerine and its pharmaceutical applications.

Officinal cantharides was the title of an essay in four chapters, by M. Fumouze, (1) Natural history of cantharides; (2) Chemical history of cantharides; (3) Causes which can alter or enfeeble its properties; (4) Insects and acarides met with in cantharides.

The resins employed in pharmacy was the subject of an essay by M. Guelliot. Finally, M. Guichard, a competitor, sent an essay on the alkaloids of the cinchonas. A resumé of the actual state of science regarding the constitution of artificial alkaloids, and of the genus of natural alkaloids, opens the subject. Then there is a chapter in which the question is treated historically, followed by six others upon those alkaloids which are obtained from the cinchonas besides quinine, and the chlorinated, brominated, iodinated derivatives. These chapters contain a complete history of the chemical properties of these substances. The salts of quinine, cinchonine, and quinidine are treated separately. After these the extraction of the alkaloids, and their commercial preparation, form the subject of consideration, and then the adulterations of quinine are taken. M. Guichard devotes a chapter to the special study of the red colouring matter which forms when cinchonine, quinine, and especially quinidine, are submitted to distillation; pure quinine he finds does not furnish these purple vapours, the presence of a glucoside is necessary. The commission unanimously awarded the prize to M. Guichard.

M. Roussin has proposed the use of bichloride of mercury for the detection of kreatinine in urine; kreatinine is precipitated from its solutions by the mercurial salt.

MM. de Luynes and Esperandieu have published a research on the preparation and some properties of pyrogallic acid. They remark in commencing, that the processes actually in use yield only about 25 per cent of the weight of gallic acid employed. By the action of water at 200—210 degrees they are enabled to transform gallic acid into pyrogallic acid and carbonic acid. The process is conducted as follows:—Into a brass cauldron with a tightly fitting cover, the gallic acid is introduced with two or three times its weight of water; the cauldron is heated to 200—210 degrees, and maintained at this temperature for an hour and half to two hours. At the end of this time the vessel contains a slightly coloured solution of pyrogallic acid. By boiling with animal black the colour is removed; the solution is filtered, and the water removed by evaporation. Upon cooling the pyrogallic acid solidifies in the form of a hard crystalline mass, slightly amber, and sometimes rose coloured. To obtain the product white, it suffices to distil in vacuo; an operation which goes on very rapidly, almost instantaneously. The yield of pyrogallic acid obtained by this process is equal to the amount theoretically obtainable. These are the properties of pyrogallic acid described. A solution of pyrogallic acid added to lime-water, gives rise to a magnificent violent colouration. Ethylamine causes the same colouration. A concentrated slightly acid solution of quinine, added to a concentrated aqueous solution of pyrogallic acid, produces a yellowish crystalline deposit, which contains the elements of sulphate of quinine and pyrogallic acid. If perfectly pure filtered solutions are mixed, no precipitate is formed until a little crystal of sulphate of quinine is added; in which case the solution becomes immediately a solid mass. Orcine and resorcine react just in the same way with sulphate of quinine, whence this reaction with sulphate of quinine would appear to be common to those substances designated as phenols.

The memoirs brought before the Academy of Sciences on the 3rd of chemical interest were the following. On the rapidity of the propagation of waves through gaseous media, by M. Regnault. On the carbonisation of wood, and the metallurgy of iron by M. Gillot. On the decomposition of nitrates during fermentation by M. Schlœsing.

M. Bouchotte sent a third note on the dialysis of induction currents.

M. Gillot states in the first part of his memoir, that the only condition necessary for a good carbonisation of wood to take place, is that the operation be made to proceed slowly. The decomposition of the wood commences at about 100°, wherefore analyses of samples of wood dried at 150°, do not give the true composition. During the decomposition of the wood, resulting in production of carbonic acid, and hydrocarbons, heat is developed in the interior vessel, which is thus raised to a temperature in excess of that of the oven. This result is produced when the temperature of the oven approaches 300°, and it must continue to the end of the operation. Too rapid an increase of this internal heat gives rise to the formation of tar and gaseous products in unnecessarily large quantity, diminishing in a corresponding degree the useful accessory products, as well as the yield of charcoal. The condensed products are richest in acetic acid when the temperature of the oven is 218°; at this heat they contain 48 per cent. Wood, when the operation of carbonising is well carried out, may be made to yield 7 or 8 per cent of monohydrated acetic acid; finally, the resulting volume of carbon is two-thirds of that of the wood employed. The second part of this memoir relates to the employment of fuel in the furnaces used in the metallurgy of iron. M. Gillot says that it has been demonstrated that in operating in the ordinary way, with the blast furnace in general use, the calorific power of the combustible gases, escaping at the mouth, represents, with but slight variations, two-thirds of all the combustible matter employed. It has also been demonstrated that to convert the pig iron produced into steel or iron, the heat required is much less than the total heat which the combustible gases, lost at the furnace mouth, would produce by combustion. M. Gillot collects these gases by means of an exhauster, in a gasometer; he afterwards liberates them according to the requirements of the operation.

M. Schlœsing's note on the decomposition of nitrates during fermentation, referred to M. Reiset's memoir on beet-root juice fermentations. In answer to the request of the subscriber whose letter was forwarded to me, intimating that the subject of beet-root fermentation was one with regard to which details would be acceptable in England, an account of M. Reiset's research is introduced into this letter. The production of nitrous gas during the fermentation of the saccharine juice is regarded by the manufacturers as a serious accident. This result is, however, nearly always observed if the juice does not contain a sufficient quantity of free acid. Under these circumstances the fermentation is arrested, and usually it cannot be made to proceed again, no matter how much yeast is added. The lactic fermentation is developed, it predominates, and the sugar is rapidly converted into lactic acid. Juice which contains before the fermentation only two grammes of free acid, rapidly increases to eight or ten grammes the litre without any further addition of acid being made. M. Reiset has established by numerous experiments, that in a general way the juice resulting from the maceration ought to contain an amount of free acid equivalent to three grammes of monohydrated sulphuric acid in the litre. In well conducted distilleries it is customary to regulate methodically the proportions of sulphuric acid, too often used as a remedy for all accidents. The ammonia present in the beet-root juice, combined with feeble acids, is almost sufficient to completely saturate the sulphuric acid added during the operations. M. Reiset employs the method proposed by M. Boussingault to estimate the amount

of ammonia: 30—50 c.c. of saccharine juice are distilled with a litre of pure distilled water, and 5 c.c. of solution of potash of 40 degrees, two fractions of 200 c.c. each are collected, and the ammonia deduced from the amount required to saturate a known volume of titrated sulphuric acid. The production of nitrous gas during fermentations has often been explained as due to a reduction of the nitrates found in the juice, but how then admit with the manufacturers that treatment with sulphuric acid prevents it? M. Reiset, thinking, on the contrary, the formation of nitrous gas to be attributable rather to oxidation of the ammonia when this alkali is not saturated by a powerful acid such as sulphuric acid, always keeps careful account of the amount of ammonia present in the beet-root, and regulates the employment of acid by the amount of this alkali. The idea has been put in practice in a distillery and has been at work three years; excellent results have been obtained, nitrous fermentations have happened only very rarely.

M. Schlœsing takes quite an opposite view with regard to the formation of nitrous gas in fermentations, and he advances experiments to prove that it is really a phenomenon of reduction. Experimenting with tobacco juice (naturally acid) to which he had added nitrates, he found that these latter remained intact until, owing to decomposition of the organic matter, the solution became alkaline, then they gradually diminished in amount. M. Schlœsing explains the nitrous gas as the effect of putrifying organic matters upon nitrates; he asks what there is astonishing in the bodies which are able to reduce sulphates to sulphides, being enabled to reduce nitrates to nitrites. The neutral or alkaline state he considers particularly favourable to the production of reducing matters, and therefore applies the fact observed by the alcohol makers, that addition of sulphuric acid prevents nitrous gas being formed, as a confirmation of his view.

CORRESPONDENCE.

THE RECENT DISCUSSION AT THE CHEMICAL SOCIETY.

To the Editor of the Chemical News.

Sir,—In the report of the meeting of the Chemical Society, given in your last number, we observe that our speeches are rather inadequately given, and that inaccuracies have crept in. This is not to be wondered at, inasmuch as both of us read them rapidly from manuscript, which was afterwards (at the request of the President) handed to the Secretary for publication by the Chemical Society.

Our speeches contained matter which might have been appropriately given in answer to Dr. Frankland, who spoke later in the evening.

We wish now to make a few remarks on Dr. Frankland's speech as reported in your last number. We notice four examples of results obtained by taking what are there termed "artificial" waters, and operating on them by Dr. Frankland's method, and by our method. These "artificial waters" were prepared by treating water with peat, and were, therefore, waters containing unknown quantities of organic impurities, and consequently the want of coincidence between results got from them by the employment of the rival methods of analysis is in itself evidence of nothing beyond the fact that the methods give different results.

In Dr. Frankland's recent lecture, he gave four instances of the employment of his own method on another sort of "artificial" water, viz., on water of known composition. In these instances he dissolved known quantities of sugar, and in one case known quantities of sugar and chloride of ammonium in water, and so prepared waters containing known quantities of organic carbon and organic nitrogen.

In these cases—and these are the only published instances of a testing of Dr. Frankland's process—he had errors of about 3 cubic centimeters, 0.8 c.c. and 0.4 c.c. of carbonic acid, and in the nitrogenous instance he observed about 0·07 c.c. of nitrogen more than he had put into the water, and probably had committed an error of much greater magnitude.

As was said by one of us during the debate in the Society, errors of this magnitude are a satire on the claim to work to the 1-100th of a cubic centimeter, and hardly any severer censure could be passed on our method, which really does work to the 1-100th of a cubic centimeter, than for such a process as this of Dr. Frankland's to furnish results coincident or parallel with those given by it.

In reference to Dr. Frankland's experiments on the action of alkaline permanganate on some alkaloids, we have to remark that earlier in the evening one of us handed to the Secretary a short account of the action of this reagent on certain alkaloids, and on a variety of organic nitrogenous substances, and since the last meeting we have much extended these researches.

In your report, in giving Dr. Frankland's alkaloidal results, you give "Ammonia obtained." "By permanganates," "By combustion." In place of "by combustion," it should be "ammonia calculated from the formula." In point of fact, Dr. Frankland compared the ammonia equivalent to the total nitrogen of the alkaloid with the ammonia got from it by our process.

Dr. Frankland was unfortunate in his selection of strychnine, narcotine, and sulphate of quinine to exhibit want of *parallelism* between the ammonia given by our process, and the ammonia equivalent to the total nitrogen of the substance.

Dr. Frankland's numbers are:—

	"Albuminoid" NH_3	Total NH_3
Strychnine	·00032	·00101
Narcotine	·00031	·00068
Sulphate of Quinine ..	·00073	·00128

The real numbers, however, exhibit the "albuminoid" ammonia as exactly one-half of the ammonia which the total nitrogen could furnish. In place of ·00032 for strychnine, Dr. Frankland should have given ·00051.

At any rate, we have obtained from strychnine exactly one-half of its total nitrogen in the form of ammonia.

The total nitrogen got from strychnine, narcotine, and sulphate of quinine, and the "albuminoid" ammonia which their alkaloids yield, are quantities parallel to one another.

In conclusion, we have to remark that we do not remember to have heard Mr. Campbell make the admission of the possibility of error (owing to the possibility of there being ammonia in the distilled water used in his former experiments), which we find at the end of his speech as reported by you.—I am, &c.,

J. Alfred Wanklyn.
Ernest T. Chapman.

London Institution, February 17th, 1868.

MISCELLANEOUS.

Obituary.—Mr. W. Herapath, sen., who was well known as an analytical chemist, died on the 13th inst. at his residence, the Manor House, Old Park, Bristol. For some time he had suffered from diabetes, but till a few days previous to his decease he persevered in his professional pursuits. He was professor of chemistry and toxicology at the Bristol School of Medicine, of which institution he was one of the founders. The subject of this notice was the father of the well known analytical and toxicological chemist, Dr. W. Bird Herapath, F.R.S.

NOTES AND QUERIES.

Solvent for Essential Oils.—Can any of your able correspondents inform me of a solvent for the essential oils or the resinous essences, such as cinnamon or bergamotte, so as to enable me to mix them with water without creating a muddiness? Information upon the above will oblige a SUBSCRIBER.

Clarke's Process for Softening Water.—What quantity of lime, or lime water (assuming that 1 lb. of lime would dissolve in 90 gallons of water) would be required to precipitate the calcareous matter in spring water (by Clarke's process), the analysis of spring water being as follows: Per gallon in grains—carbonate of lime, 12·8, sulphate of lime, 1·5, chloride of sodium, 3·8, organic matter (vegetable), 2, hardness, 9°, ditto after boiling, 4°?—AQUARIUS.

Determination of Free Sulphuric Acid in Superphosphates.—The following is a simple, and yet efficient plan: Estimate the sulphuric acid in the original sample by dissolving a weighed portion of the superphosphate in hydrochloric acid, not contaminated of course with any sulphuric acid, separate the insoluble matter by filtration, and after thorough washing with boiling distilled water, precipitate with chloride of barium, and let stand for some hours, separate the sulphate of baryta, and calculate the sulphuric acid from its weight. Now take another weighed portion of the superphosphate, ignite it gently, and proceed as just described; the difference of the two results as regards sulphuric acid obtained represents the free sulphuric acid in the sample.—DR. A. A.

To Prevent Water Freezing.—Your correspondent "Volta" desires to know (1.) what proportion of salt has to be added to water to prevent it freezing when exposed to the coldest weather known in this country. I would advise "Volta" not to use salt, especially not when in contact with iron, but rather apply caustic soda, which dissolved in water does not affect iron, and at the same time if the solution is made strong enough will not freeze, while, as is well known, a solution of salt freezes, partly leaving a more concentrated saline mixture unfrozen. 20 parts by weight of caustic soda upon 100 parts of water are sufficient to keep water fluid even at as low a temperature as 10° F. As regards the use of alcohol, I think methylated spirits might do, a mixture of water and spirits of the specific gravity of 0·9760, *i.e.*, containing 20 per cent by volume of absolute alcohol will not congeal even at 8° F. A strong solution of commercial glycerine is very much used in the colder climes of Europe, *e.g.*, Russia, Poland, Sweden, to substitute water in wet gasmeters, as such a solution of glycerine does not freeze even in the cold weather, which is usually very severe in these countries in winter. During the winter from 1847 to 1848 at Stockholm, the temperature for several days was below the freezing point of mercury, that is,—40° F. During these days the late celebrated Berzelius made some experiments with frozen, *i.e.*, solid mercury, which could then be obtained readily in large quantity; this, in passing, and without reference to the non-freezing of the mixtures above alluded to at so low a temperature.—DR. A. A.

TO CORRESPONDENTS.

W. Kirk.—In a short time.

J. Leasson, Glasgow.—We expect to receive the report in a few weeks.

W. B.—We will refer to the paper, and communicate particulars if they appear likely to be useful.

D. S.—Some Technological Dictionary would probably give you the required information. See "Ure's," "Brande's," "Cooley's," or "Watts's Dictionaries."

Jurors' Report of the Paris Exhibition, 1867.—We have had many applications for this report. On enquiry we find that no report of the Jurors is likely to be published in this country, and it is not known if such will be published in Paris.

Marshall Hall.—Dr. Roscoe has made the discovery of an ore of vanadium in the Alderly Edge copper ore, in Cheshire. This discovery formed the basis of the Bakerian Lecture, delivered before the Royal Society, by Dr. Roscoe. A report will shortly appear in our columns.

F. Blair.—The extract from the *Panama Star and Herald* is received with thanks. Professor Delisse's theory of the cause of earthquakes, however improbable it seems, appear to have been borne out by the accurate fulfilment of the predictions founded upon it. It requires careful attention before we could say whether this appeared a coincidence or not.

Communications have been received from Marshall Hall; G. Marrison, Tasmania; F. Blair; W. Thompson (with enclosure); G. Hain; W. Kirk; F. Foord, Victoria, Australia; W. Kellner; Captain W. A. Ross (with enclosure); A. L. E.; T. G. Barlow; Dr. T. L. Phipson (with enclosure); C. R. C. Tichborne; T. Hill; W. Venables (with enclosure); J. Samuelson; Dr. Adriani; J. Stephens; Dr. W. Bird Herapath, F.R.S. (with enclosures); S. Dowell; J. Taylor; M. L'Abbé Moigno; Philip Holland (with enclosure); Phillips and Co.; J. H. Atherton; T. M. Drown; F. Muspratt; Professor Pavesi; Negretti and Zambra; E. Leeds (with enclosure); M. Leoni and Co.; F. Suckling; W. Smith; J. B. Giles (with enclosure); R. Eaton; Marshall Hall.

Books Received.—"Reports of the United States Patent Office, 4 vols. for 1863 and 1864," from the Honourable the Commissioners of Patents. Stevens, Bros., Agents for Europe; "The Harrowgate Herald"; "Bristol Times and Mirror"; "Western Daily Press"; "Rollisson and Sons' Catalogue of Floricultural and Culinary Seeds"; "Journal of Gas Lighting"; "Darlington and Stockton Times"; "El Correo Hispano-Americano"; "Scientific American"; American Gas Light Journal"; "Zeitschrift Für Chemie, 4 parts."

CONTEMPORARY SCIENTIFIC PRESS.

Under this heading it is intended to give the titles of all the chemical papers which are published in the principal scientific periodicals of the Continent. Articles which are merely reprints or abstracts of papers already noticed will be omitted. Abstracts of the more important papers here announced will appear in future numbers of the "Chemical News.")

Bulletin de la Société Industrielle de Mulhouse.
October, 1867.

E. KOPP, "On the Preparation of Extracts of Garancine for Calico Printing."

November.

ROSENSTIEHL, "Report on the Methods used in Dieuze for Utilising Chlorine Residues and Soda Waste."

Journal des Fabricants de Papier.
October 15, 1867.

E. BOURDILLIAT, "On Testing the Materials and Chemical Products used in Paper Making. (Continuation.) Researches on the Mineral Substances contained in Paper. On the Detection of some Colouring Materials used in Paper Making. On the Determination of the Tenacity of Paper."

November 1.

E. BOURDILLIAT, "On Testing the Materials and Chemical Products used in Paper Making. Continuation. On some Changes which take place in Paper."

L'Invention.
October.

P. ALFRAISE, "On Raddisson's new Process for the Manufacture of Aniline Colours."

November.

C. SAIX, "On the Natural Formation and Artificial Production of Diamonds."

Kunst und Gewerbeblatt.
August—September.

H. LE PLAY. "On the Use of Lime for Extracting Crystallisable Sugar from Saccharine Juices, Syrups, aud from the Molasses of Beet and Cane Sugar." O. DESAGA, "On the Use of an Infusion of Guaiacum Wood for Testing Kirschwasser." C. R. VON HAUER, "On an Alloy of Bismuth, Lead, Tin, and Cadmium fusible at a very Low Temperature."

Bulletin de la Société d'Encouragement.
September.

E. PELIGOT, "On some Processes now in Use for Engraving on Glass by means of Hydrofluoric Acid." DE LUYNES, "On a Method of Obtaining Colouring Matter from Orcine."

Comptes Rendus.
November 25.

E. BOURGOIN, "On the Electrolysis of Organic Acids and of their Salts." A. SCHEURER-KESTNER, "Some Experiments on the Manufacture of Chloride of Lime." A. GAUTIER, "On the Isomers of the Nitriles of the Fatty Series."

MEETINGS FOR THE WEEK.

MONDAY.—Geographical, 8½.
TUESDAY.—Royal Institution, 3. Professor Tyndall, "On the Discoveries of Faraday."
WEDNESDAY.—Society of Arts, 8.
—— Geological, 8.
THURSDAY.—Royal Institution 3. Professor Tyndall, "On the Discoveries of Faraday."
—— Royal, 8½.
—— Zoological, 8½.
—— Philosophical Club, 6.
FRIDAY.—Royal Institution, 8. A. Vernon Harcourt, Esq., "On the time in which Chemical Actions take place."
SATURDAY.—Royal Institution, 3. Professor Roscoe, "On the Non-Metallic Elements."

CANDLES.—If you do not want your candles exclusively for show, but with pleasantness of appearance require excellence of burning, buy "PRICE'S GOLD MEDAL PALMITINE," or their "SHERWOOD PALMITINE," or their good old-fashioned "BELMONT SPERM," or "BELMONT WAX," or their "BEST," No. 2," ""No. 3," or "BATTERSEA" COMPOSITE," in preference to the finest, and most transparent Paraffine candles. But if you must have the extreme transparency of pure Paraffine, "PRICE'S PARAFFINE" or their "BELMONTINE" will give it to you in perfection, and at a more moderate price than is usually charged for any other really first-class Paraffine Candles.

The new toilet soap, "PRICE'S SOLIDIFIED GLYCERINE," containing half its weight of their concentrated distilled Glycerine, should be in general use in every house before the winter comes on, because of its admirable effects in preventing chapping of the hands and face. In every house there ought also to be one of the sealed bottles of their concentrated Distilled Glycerine, known everywhere as "PRICE'S GLYCERINE," and prescribed by the most eminent medical men abroad as well as at home, as the one only Glycerine for medicinal use whether externally or internally.

PRICE'S FANCY SOAPS of the different sorts usually made are excellent, and command a constantly increasing sale. The "Solidified Glycerine" spoken of above is, however, the one fancy soap to use.

"PRICE'S NEW PATENT NIGHT LIGHTS," for burning in the wide glasses are believed to be the very best Night Lights made. "PRICE'S CHILDS' NIGHT LIGHTS," for burning without glasses, and their different sorts of "CHAMBER CANDLES" are so well-known, and so generally appreciated as not to need any special notice here.

THE CHEMICAL NEWS.

Vol. XVII. No. 430.

REMARKS ON THE VOLUMETRIC ESTIMATION OF PHOSPHORIC ACID.

By CHARLES F. BURNARD, F.C.S.

Having been for a considerable period engaged in the analyses of phosphatic substances, raw and manufactured, I send for insertion in the pages of the Chemical News, some of my experiences in the pursuit of the volumetric process. I have confined myself almost entirely to the use of nitrate of uranium in the well known manner, which need not here be described; but in the following of which, according to the published methods, several precautions are necessary; while often with the greatest care as to uniformity of volume in samples tested, it is frequently doubtful when the point of colouration is obtained. For (say) in two determinations of the same sample made side by side, it is seldom that complete uniformity of results are obtained. Much depends on the size of the drop falling into the little pool of ferrocyanide, something in the manner in which the said drop falls, while in all cases *time* is an essential element in the question. If testing be continued, as is usually directed, until the brown colouration is evident, the result will be far too high. To prove this, let the operator in reaching the desired indication, cover over his slab (to prevent drying) and leave it so some hours, say until next morning, when he will find the point to be five or six units below that indicated over night. But even now there is much uncertainty, for say he has (as he should always have) tried two side by side, he will frequently be perplexed in making his decision. I am happy, however, in being able to record a method by which all doubt is dissipated, and much greater accuracy obtained.

If the composition of the substance (say a manure) be quite unknown to me, then I make a preliminary examination, which soon shows the probable range of the per cent of its phosphoric acid. But in general this is unnecessary. I use a plain porcelain slab, pits or indentations for the pools being objectionable, as hindering due access of light to the body of the pool. To prevent flowing about, a ring of cork, giving a clear space of ⅜ of an inch, pressed on some hard tallow and then on the slab, leaves a faint but effectual wall of grease.

My method may be best explained as follows, giving an actual determination by way of illustration. Three portions of the same solution, being each 100 septems, were taken and tried side by side on the same slab.

No. 1.—26	28	30	32	34	Septems.*
No. 2.—25	27	29	31	33	,,
No. 3.—24	25	26	27	28	,,

Now, at the conclusion of the actual testing, not one exhibited the slightest trace of brown colouration; they all appeared precisely alike. They were then covered over and left until the morning, when one only, viz. 34, showed the red brown colour, and that as an intense bright eye in the centre of the pool.

Now, following out my new plan, the slab was carefully put on a levelled stand before a fire, and the spots dried by radiant heat falling on their surfaces. Gently drying in this way being preferable to any other, for rising from below the heat disturbs the settlement of the precipitate. When dry, and the slab just warm, water was carefully dropped on each spot, so as to dissolve the dried-up ferrocyanide, when as if by magic, although on the dried slab there was not a trace of brown visible, the truth was revealed, and the reading became—

No. 1.—30	stood	as the	number
No. 2.—29		do.	do.
No. 3.—29		do.	do.

At the time of testing there was no exhibition of colour; next morning 34 stood revealed; but on drying and redissolving, as explained, 29 was unquestionably the number. It is obvious that while a precipitate may be so slight as to render the colouration of a small pool difficult, yet by its settling down on the white slab it is immediately revealed on dissolving away the crust covering it.

Side by side with the above, has been tried another volumetric process, of my own devising, at least new, so far as I am aware of. It is exceedingly simple, and has afforded satisfactory results. Of course it is not suitable in all cases, but may be applied to the determination of the phosphoric acid, in the great majority of the so-called superphosphates; its value being increased, moreover, by the fact that it necessarily involves the estimation of the free acid in the manure. Suppose a superphosphate made in the usual manner; in such a manure the bone phosphate may be measured by the quantity of sulphuric acid employed in its solution. I extract all that is soluble in water from 100 grains of the manure, and divide it into two equal volumes of one thousand septems each, in beakers of the same dimensions. Into one I drop a standard solution of soda from a burette, when, as is well known, a precipitation of bone phosphate occurs; this, however, on gently moving with a stirrer is re-dissolved; and I continue to drop in until there is a faint trace of a permanent precipitate, which may be the better detected by comparison with the other volume in the second beaker. When sufficient soda has been added, then after duly noting the number of septems employed, an additional septem may be dropped in, when a decided milkiness and agitation will be manifest. The number of septems thus employed is the measure of the free acid existing in the manure. A little practice will enable the operator to very nicely determine the point of incipient precipitation. I now throw in a piece of litmus paper, if blue it instantly becomes red, and then continue the soda dropping until the red litmus becomes nearly blue. A few minutes' repose will allow sufficient time for the precipitate to somewhat settle down, leaving a clear space above; into this a drop of soda solution may be carefully let down, when, if further precipitation occurs, more soda may be added, the whole stirred, and allowed again to subside. In practice it is found that the litmus should be brought to a decided but not to a deep blue. Now, the further volume of the standard soda solution employed, is the measure of the sulphuric acid economically employed in the manure, and is therefore the measure of the amount of the phosphoric acid in solution. In my own practice I have worked out a number, by which, on multiplying the number of septems of the standard solution of soda used, there is at once obtained the equivalent of the phosphoric acid in the shape of tribasic phosphate of lime. In comparative determinations, this process has given me results nearly constantly one half per cent too low.

Compton Gifford, February 24th, 1868.

* In the above each number is intended to represent a thin pool of ferrocyanide of potassium, of about ⅜ inch diameter; and also in each case the number of septems of nitrate of uranium employed.

Crystallisation by means of the Blowpipe.—G. Rose finds that the opaqueness which blowpipe-beads frequently assume on cooling is due to a separation of crystals. He prepares them for examination under the microscope either by flattening out the bead or by dissolving it in water. Titanic acid under such conditions forms anatases. If titanic acid is fused with microcosmic salt in a covered crucible, a blue glass is obtained in which crystals are enclosed, and if a small piece of this glass is now heated in the oxidising flame crystals are formed, differing from anatas in shape and optical properties.—(*Akad. z. Berlin*, 1867, 129.)

METHOD FOR THE

DETERMINATION OF SILICON IN IRON AND STEEL.*

By V. EGGERTZ,
Professor at the School of Mines, Fahlein, Sweden.

EVERYBODY who has been engaged in the analysis of iron and steel is well aware how very uncertain the determination of silicon becomes when the method hitherto used for its separation in the form of silica is followed, because not only cast iron, but also bar iron and steel, is never found absolutely free from slag intermingled with it. This slag is decomposed by the ordinary method of dissolving the iron in acids, and its silica then augments the amount of silica formed from the silicon contained in the iron or steel; accordingly, too much silicon is obtained when the ordinary method has been employed, as nearly the whole of the silica remains unacted upon, a very small portion only going into solution.

The same thing cannot be said of certain sorts of cast iron, but these sometimes contain blast-furnace slag. In the collections of the Mining Institution at Fahlein are some specimens of spiegeleisen which evidently contain particles of slag; at the same time, pig iron containing slag may be considered as rare.

It ought also to be mentioned here, that, according to the *Berg. und Hüttenmannisch Jahrbuch*, vol. xi., page 289, crystallised silicon has been found in crystallised cast iron from Krain, in the form of small silvery plates, which were neither acted upon by boiling aqua regia nor by ignition in oxygen gas; but they were converted into silica by fusing with carbonates of potash and soda.

Crystallised silicon is insoluble in hot solutions of carbonate of soda, but soluble, with development of hydrogen, in hot solutions of caustic potash, and also in hot hydrofluoric acid. In working at the determination of silicon in cast iron at the Mining Institution, there has never been occasion to suspect the presence of crystallised silicon. Cast iron which had a thin white pulverulent coating of silica on its surface, has been sometimes observed.

After fruitless efforts to dissolve iron in highly diluted organic or inorganic acids, which should have no effect on the refinery slag, such a solvent was finally discovered in bromine, which, when mixed with water, dissolves the iron without the slightest action on the accompanying slag.

But as experimenting with bromine in large quantities is very disgreeable, trials were made to use iodine instead; and this, like bromine, has been proved to have no effect on the slag, nor on the oxide or proto-sesquioxide of iron, or proto-sesquioxide of manganese.

At the same time bromine dissolves iron quicker than iodine, and is, perhaps, more easily obtainable in the requisite state of purity.

Neutral chloride of copper may also be used as a means of solution, if copper is not precipitated.

Moreover, as continued experiments have shown that a solution of carbonate of soda can separate finery slag from the silica, which has been formed by the use of iodine or bromine on the silicon contained in the iron, the following method for the determination of silicon and slag in bar iron or steel has been used and considered successful; the same method may be employed for cast iron, because blast-furnace slag, when such is found, is not perceptibly changed by iodine or bromine, nor by solutions of carbonate of soda.

Three grammes of bar iron or steel which has passed through a sieve of 0·2 of a line at the most (the filing or boring must be made with great precaution, so that no scale nor the least trace of the file may get into the sample, and so affect the results); 15 grammes of iodine are added in small portions at a time to 15 c.c. of water in a beaker of 100 c.c. capacity. The water must be previously boiled to expel the air, which would otherwise oxidise the iron. The iodine is stirred in the water with a glass rod, in order to get rid of the air which has accompanied it, and the floating iodine and iron particles are allowed to sink.

The beaker with the iodine† and water, which is kept covered with a watch-glass, is cooled in ice water before the iron is put in, and during the solution it is kept at the temperature of 0° C. For the first few hours it must be well stirred every hour, or oftener, with a glass rod, but afterwards not so frequently.

By means of the low temperature and the careful admixture of the iron (by which heat is prevented), the solution may be performed without the least development of gas, and the iron has less inclination to become oxidised by the air at this low temperature.

By pressure, and by agitating with the glass rod, the solution of the iron particles which collect at the bottom of the beaker is much facilitated; but if no iron is visible, the beaker may be kept at an ordinary temperature, or, preferably in ice water. If some of the solution has risen, and dried up on the sides of the beaker or on the glass rod, it must be well moistened with the same solution before the water is added.

About 30 c.c. of water, which should be very cold in order to prevent the formation of basic salts, are added to the solution; it is then well stirred, left to settle, and the fluid with the lighter particles of graphite is poured into a filter of 2 in. diameter; the filtration is kept up without interruption until there remains only a somewhat heavy dark powder of slag, &c.; at the bottom of the beaker about 5 c.c. of water, with a few drops of hydrochloric acid, are now poured in and stirred with the glass rod; if hydrogen gas is given off, it is an indication that there is still some metallic iron undissolved.

The acidified water is quickly poured on the filter in order not to act on the slag. If a development of gas is perceived, a little iodine, with carbonate of soda and water, is added for the complete solution of the iron, and the residue is thrown on the filter and washed with cold water, until a drop of the filtrate gives no reaction with a solution of 0·2 per cent of ferrocyanide of potassium contained in a small porcelain crucible. Iron solutions containing 0·00001 gramme of oxide of iron per c.c. show in this way very distinct reactions, particularly if a drop of nitric or hydrochloric acid be added. The filtrate is evaporated to dryness, in which operation some of the iodine is sublimed away. Thirty c.c. of hydrochloric acid, 1·12 sp. gr., are then added, and it is again evaporated in order to obtain the silica which may be dissolved in it. When intending to estimate the amount of graphite, it must be washed only with water, because hydrochloric acid would dissolve the slag.

The filter, previously dried and weighed, is again dried and weighed when containing the precipitate. It is then ignited, and the residue weighed. After ignition, the residue is boiled in a solution of soda, in order to extract the silica, and weighed. It should be observed that some part of the silica which has been formed from the silicon in the iron may possibly unite with the slag during the drying and ignition. In consequence of this, it is difficult to extract it by means of a soda solution, whence this method is not to be recommended in exact determinations of silicon.

* From *Engineering*, July 24, 1868. Translated by C. P. Sandberg.

† The iodine should not leave any residue when exposed to a high temperature. Impure iodine may be purified by sublimation in the following manner: It is placed on a large watch-glass resting on a plain glass plate, and heated on a sand-bath to 107° (the melting-point of iodine), a plainly polished beaker is inverted over the watch-glass, and on this the iodine is condensed. The purity of the iodine may be tested by dissolving in it 3 grammes of iron containing a small but accurately determined amount of slag; if these results agree, the iodine may be considered fit for use.

When using bromine as a solvent, there must be taken 6 c.c. to 3 grammes of finely powdered iron or steel with 60 c.c. of water, which has been previously boiled, and cooled to 0° C.; and this temperature preserved by placing the beaker in ice water until the solution is complete, which usually takes place in two or three hours; it is cautiously stirred once or twice with a glass rod; if stirred hastily, the solution proceeds too violently. The solution is placed on a table or in ice water, and stirred with the glass rod now and then. The further operations are conducted in the same manner as when using iodine. Bromine is preserved under water, and is taken up by a pipette, which is introduced into the bottle, the upper end being kept closed by the finger.

When it is preferred to dissolve iron or steel in pieces, instead of in powder, it may be done; but in this case it is not necessary to place the beaker in ice water, as the metal is less violently acted upon in this form. Several days are required for the solution; the iron, and particularly the steel pieces, must be kept clear from the graphite which adheres to their surface.

In experiments using the solution at the temperature of 40° C. for iodine, and 25° C. or 30° C. for bromine, it occasionally happens that yellowish-brown basic salts have formed; therefore this temperature must not be used, but the solution of iron ought to be operated upon at a temperature of 0°.

In order to determine the silica (formed from the silicon in the iron) and slag, the filter, which contains graphite (in combination with iodine or bromine and water), silica, and slag, is unfolded, whilst it is still wet, on a watch-glass. The contents are washed away from the filter (which ought to rest only upon one-half of the filter whilst in the funnel) with a very fine jet from a wash-bottle (so as not to obtain too much water) into a platinum or silver crucible of the capacity of 30 c.c. The loosening of the mass may be facilitated by a fine paint-brush. The water in the crucible is evaporated to about 6 c.c., 3 c.c. saturated solution of carbonate of soda, free from silica, are added, and the crucible put in the copper ring in a water bath, the hole being large enough to allow the crucible to project ¼ in. above it. It is kept in the boiling water 1 hour, during which time the liquid is stirred two or three times, and the insoluble mass crushed with a platinum spatula. The liquid is carefully poured from the insoluble mass on to a small filter, and to the mass in the crucible is added 1 c.c. of saturated solution of carbonate of soda and 2 c.c. of water. When this has been boiled 1 hour, ‡ the whole contents of the crucible are thrown on the filter and washed. The solution of silica in soda is acidified by hydrochloric acid, and mixed with the iron solution, and the whole evaporated to dryness on a water bath. When the solution attains the thickness of ordinary syrup, it is stirred very often with the glass rod, until the mass becomes a dry powder, and heated until the smell of hydrochloric acid has nearly gone off; the beaker is then placed in boiling water for 6 hours, 15 c.c. of hydrochloric acid of 1·12 sp. gr. are then added, and the beaker left on the water bath 1 hour. When the red powder is entirely dissolved, 50 c.c. water are added; and when no crystals of chloride of iron are visible, the solution is thrown on a filter and washed with cold water, warm water forming basic iron salts, which make the silica appear red. The filter containing the silica is drid and ignited in a porcelain crucible, gradually increasing the temperature to a full red heat, and weighed; ‖ if silica is coloured red by oxide of iron, a little hydrochloric acid, 1·19 sp. gr., must be poured into the crucible.

(To be continued.)

‡ 0·1 gramme of ignited and pure silica obtained from analysis is dissolved in the above manner in 6 c.c. of a saturated soda solution and 12 c.c. of water. If any residue is observed after the second boiling this arises from some impurity which has united in small quantities with the silica, rendering it insoluble. When strong hydrochloric acid is boiled with this insoluble silica, it may afterwards be dissolved. When the solution from the 1 gramme of silica is diluted with water to the volume of 50 c.c. at the ordinary temperature, it has no tendency to come into the form of jelly. Quartz powder is dissolved by the preceding method, but very slightly, but ignited titanic acid and finery slag are not acted upon, and the tersilicate slag from blast furnaces but very little.

‖ When the silica is quickly exposed to a high temperature, a considerable loss may arise from the spirting of the water combined with the silica. Silica dried at 100° C. has been proved to retain 1 equivalent of water to 3 equivalents of silica, that is, about 6 per cent of water, which is lost by a strong ignition.

MR. RODWELL ON PHLOGISTON.*

The theory of Phlogiston is invariably regarded as a distinct development, unconnected with any previous theory, and uninfluenced by any prior mode of scientific thought. The object of Mr. Rodwell in the paper, of which we here give a short abstract, is to prove that "the theory of phlogiston was not the result of a sudden development; it did not owe its existence to an intellectual exploit, but it arose by a process of evolution, and by a gradual *modus* of development.

In Section 1 ("*Of the subtilis ignis of the Ancients*") the author shows that from the earliest times philosophers recognised a subtle fire innate in matter, and distinct from ordinary fire. The opinions of Zeno, Chrysippus, Lucretius, and others, are quoted to this effect. In the four element theory *fire* ("under which term was included light, the heat inherent in all bodies, flame, incandescent bodies, together with lightning, and all visible manifestations of electricity") was regarded as the *anima*, while air, water, and earth together constituted the *corpus*. Passing on to the Middle Ages (Section 2. "*Of old Chemical literature, and of the significance of the terms sal, sulphur, mercurius, as employed by mediæval chemists*") we find the four element theory existing nearly the same in form, but somewhat modified in name, in the three chemical principles, Sal, Sulphur, Mercurius. These are *principia*, not (as is too often imagined) *corpora*; "they are *αναλογα*—representative bodies, types of classes, type of qualities." The fire of the four element theory was included in *sulphur*, the principle of combustibility. Into this section an account of some old typical chemical works is introduced, together with some remarks upon the rise and growth of the system of chemical symbolisation.

Section 3 treats "*Of the supposed nature of fire prior to the rise of the theory of phlogiston; specially of Descartes' 'Materia Cœlestis,' and of Hooke's Theory of Combustion.*" It is here shown that from early times the idea of intestine material motion has been connected with heat. The Cartesian philosophy is discussed, and a connection traced between the *Materia Cœlestis*, and the *subtilis ignis* of earlier writers. The extended Cartesianism of Lemery is touched upon, and the following passage (which bears upon the history of thermo-dynamics) is quoted from his *Cours de Chimie*, published in 1675:—"But because there may be some difficulty in conceiving what is meant by little igneous particles ("*corpuscules ignées*"), I do understand by them a subtle matter, which, having been thrown into a very rapid motion, still retains the aptitude of moving with impetuosity, even when it is enclosed in grosser matters; and when it finds some bodies which by their texture or figure are apt to be put into motion, it drives them about so strongly that their parts rubbing violently against each other, heat is thereby produced."

Section 4 treats "*Of the ideas regarding the calcination of metals which prevailed prior to the rise of the theory of phlogiston.*" From this we learn that Glauber suggested that the increase of weight observed in some metals

* "On the Theory of Phlogiston."—*Philosophical Magazine* for *January*, 1868.

after calcination might arise from the coagulation of heat by the metal during calcination; while Boyle assigned the gain of weight to the absorption of "*extinguished flame*," or, as he otherwise expresses it, of "*igneous particles*;" and Lemery, to the assimilation of "*corpuscules de feu.*" These views were in all cases published before Stahl wrcte on phlogiston.

In Section 5 we have an account "*Of Becher and Stahl, and of the rise and development of the theory of phlogiston.*" The writings of Becher are here discussed, and it is shown that he has used the word φλογιστον solely in its original adjective sense; while "Stahl converted Becher's φλογιστον into a substantive, and applied it to designate the *materia ignis*, so often spoken of in the works of former writers on chemistry; and at the same time he endued it with certain extended functions, many borrowed from Descartes, some added by himself.

Phlogiston was defined by Stahl as "*materia aut principium ignis, non ipse ignis,*" and was conceived to be "a very subtle matter, capable of penetrating the most dense substances; it neither burns, nor glows, nor is visible; it is agitated by an igneous motion (*igneo motu*), and it is capable of communicating its motion to material particles apt to receive it. The particles when endued with this rapid motion constitute visible fire . . . . . The igneous motion is '*gyratorius seu vorticillaris.*' . . . . Heat is an intestine motion of the particles of matter.'" As an almost invariable rule the expression '*loss of phlogiston*,' which so frequently occurs in the works of the Phlogistians, means in our language, *combination with oxygen;* while "*gain of phlogiston*," or '*assimilation of phlogiston*' signifies *deoxidation.*

The sixth and last Section treats "*Of the Syncretistic nature of the theory of phlogiston*," and in this the author summarises the matter of the preceding sections, portions of which summary we give below *verbatim.*

"Phlogiston was a new name for an old principle. We have seen that the idea of the existence of a subtle fire innate in matter has pervaded physical philosophy from the earliest times. *Phlogiston* was another name for the "*pure fire*" of Zoroaster; the *ατεκνεκον πυρ* of Zeno; the "*subtilis ignis*" of Lucretius; the "*elemental fire*," "*astral fire*," "*sulphur*," or "*sulphureous principle*" of the chemists; the "*calor cælestis*" of Cardanus; the "*sideric sulphur*" of Paracelsus; the "*materia cælestis*," of Descartes; the "*terra inflammibilis*" of Becher. The functions of this entity had been varied by different thinkers, almost as much as its name, until Descartes gave them accurate definition. The theory of phlogiston was the theory of the "Materia Cœlestis" extended in a chemical direction. Phlogistic chemistry was Cartesian chemistry. Descartes defined the physical functions of the *Materia Cœlestis*, Becher and Stahl defined its chemical functions, and applied them to the explanation of diverse chemical phenomena. Throughout the writings of Becher and Stahl we find a sprinkling of Cartesianism; they did not, however, adopt the system in its entirety; they appear to have discarded the second and third elements, and adopted the first as the parent of their own system. Enough, I think, has been said in the preceding section to show clearly that the dominant functions of the *Materia Cœlestis* were conferred upon its synonym Phlogiston."

"The theory of Phlogiston was essentially and completely a syncretistic theory. It was built up of *idola theatri*, collected from various sources, and these were cemented together by the particular *idola specus* of Becher and Stahl. In this process of syncretism, the merit of these men lay; their fault was a too hasty generalisation. In that stage of chemistry, syncretism was inevitable; indeed all theories are more or less tinctured by it, with the exception of those which emanate from a new mode of experimenting, such, for example, as Kirchhoff's theory of the constitution of the sun. . . . . A theory proceeds by slow evolution until it dominates, or is destroyed. It was thus with the theory of phlogiston: arising under the most favourable conditions, it attained full development, became most cardinal, most sovereign, and fell. For twenty-eight years it was looming a half-formed thing through the mists of chemistry; for thirty-four years it was growing in strength and proclaiming its dynasty; for fifty-four years it was dominant, and it was fully ten years yielding up the ghost. There are men amongst us now who have listened to the echoes of its departing steps. Becher and Stahl were the prophets of a new mode of chemical thought, essentially classificatory, systematic, and syncretistic. In their day chemistry was at the commencement of a period of transition, and they bridged the gap which existed between empirical chemistry and modern chemistry. They did not collect the materials for the structure, they did not altogether construct it, but they designed it, and helped in the work of building. Albeit a bad bridge, and built upon shifting sands, yet it was a channel of escape from mystic science, and many passed over to take refuge on the other side."

The author concludes as follows:—

"Of the influence of the theory of phlogiston, I need say but little. It was not the first chemical theory; it did not give the first explanation of combustion, and it was established in the face of facts which carried with them its refutation. When the first stage of its development was passed, facts were adapted to the theory, and phenomena were tortured and garbled so as to fit in with it, by which means the progress of chemical science was somewhat retarded. Even when Lavoisier had conclusively proved the fallacy of the theory, this blind adherence shut the eyes of the phlogistians to the merits of the new system, and to the utter falsity of their own. Nevertheless, the theory exercised influence for good; for by its means a certain amount of order was introduced among a vast chaotic mass of chemical facts, and phenomena were classed together, and reasoned upon together, and together submitted to similar processes of mental analysis, after the manner so strongly advocated by Francis Bacon."

"When Mde. Lavoisier, habited as a Greek priestess, burnt the writings of Stahl upon an altar dedicated to the new science, the downfall of the theory of phlogiston was not alone typified; for in that holocaust perished the vast system of empiricism which had pervaded chemistry from the time of its origin until then—relics of Egyptian and Chaldæan lore, of an age of fanaticism, of intellect perverted by a false enthusiasm. Phlogistic chemistry had arisen on the ruins of the older structure of mediæval chemistry, and from it arose modern chemistry. Let us be fain to remember that the mother died in giving birth to the child. The new science was, as Dionysius, born of the dying Semele; and while we worship the son, like the Ancients we have not forgotten to raise a statue to the mother."

Volumetric Determination of Acetic Acid.—G. Merz. The determination of acetic acid by means of a standard solution of sodic hydrate is rendered difficult, by the fact, that sodic acetate imparts a violet tint to litmus, thus obscuring the final change to blue. This inconvenience may be obviated by using curcuma tincture, which remains yellow so long as free acid is present, and changes into brown when the point of neutralisation is reached.—(*Journ. pr. Chem.* ci. 301.)

Determination of Ammonia.—A. Vogel. In cases where ammonic salts have to be determined in presence of albuminoids, the author employs magnesic oxide for the liberation of ammonia in preference to potassic, sodic, or calcic hydrate, which would disengage ammonia from the organic matters. He mixes the substance to be analysed with magnesic oxide, and places it together with a measured quantity of sulphuric acid of known strength under a bell-jar. After about 4 days all ammonia is absorbed by the acid, and is then determined as usual.—(*Z. f. Rübenz, Ind. im Zollv.* 1867, 100.)

ON

HEAT AND COLD;

A COURSE OF

SIX LECTURES*

(Adapted to a Juvenile auditory),

DELIVERED AT

THE ROYAL INSTITUTION OF GREAT BRITAIN,

(CHRISTMAS, 1867—8),

BY

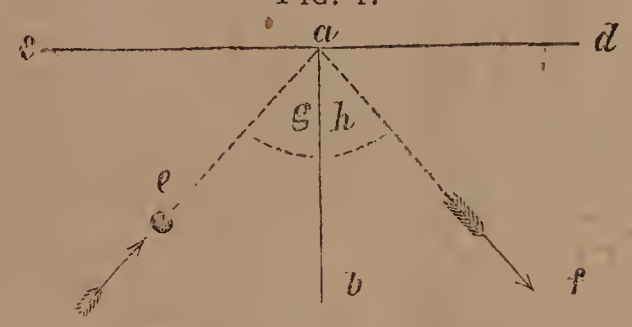
JOHN TYNDALL, Esq., LL.D., F.R.S.

Lecture VI.

Reflection, refraction, and absorption of radiant heat.—The heat of the sun.—Visible and invisible rays.—Extraction of light from the rays of heat.

In our last lecture I endeavoured to explain to you the law according to which radiant heat is reflected. I then made use of some terms which were, perhaps, rather difficult to remember. I explained to you that the angle of incidence was equal to the angle of reflection, so that if you suppose the surface of this table, *c d*, to be a reflecting surface, and this rod *a b*, a perpendicular to the surface, when a ray of light, *c a*, falls upon the surface, striking the bottom of that perpendicular, the ray is reflected so as to lie as far to the left of the perpendicular as the direct ray lies upon the opposite side of it. That is to say, the angle of incidence, *g*, on the one side is equal to the angle of reflection, *h*, which is on the other.

Fig. 1.



And now I have to draw your attention for a moment, not to the reflection of light or radiant heat from planes or flat surfaces, but to the reflection of radiant heat from curved surfaces. I have such a surface here. It is a large concave mirror, as it is called. It forms part of a large sphere of glass: it is, as it were, a slice cut from a large sphere of glass. Now, suppose a sunbeam to come in this direction, and fall plumb upon the mirror: you see that the edges of the mirror are bevelled or slanted off, and the consequence is that that sunbeam striking on it would be reflected in such a way that the reflected rays would converge and form a cone of convergent rays. I want to show you that when light is thus reflected from a concave mirror it is gathered up to a point which is called a focus. We will now throw a beam of light upon it. You can not see light itself, but you can see bodies illuminated by the light; and in this room, and especially in London air, and, indeed, in all air, there is a considerable quantity of common dirt floating in the air, and these dirt particles will be illuminated by the beam of light; and I think this will enable you to see that after reflection the beam of light will be gathered up and brought to a focus. You see the beam is now reflected from the concave mirror and is gathered up in this wonderful way into that convergent cone. If we had time, we might prove that this must be the manner in which the rays would behave after reflection in accordance with the law that the angle of incidence is equal to the angle of reflection.

Now, having shown you this convergence of the rays of light, I want to show you the reflection of the rays of heat; and for that purpose I have not a single mirror, but two mirrors. They are called "conjugate mirrors," and one is suspended over the other. I have here the means of obtaining the beautiful electric light from a battery of fifty cells: if I now place this light in the focus of this mirror the rays will be reflected upwards, and if the mirror were perfectly true they would be reflected upwards in a parallel beam, or, so to say, a solid cylinder of light. Now remember what occurs. The rays of light will fall upon this lower mirror; they will be reflected upwards by it in a straight cylinder; that cylinder of light will strike upon the upper mirror, and will be converged, and reflected again from the upper mirror, and brought to a point in what is called the focus of the upper mirror. You will see these rays of light going upward through the dust of the room when the room is darkened. I intended to have a silver bead in the upper mirror; and if it were there you would see it shining with the brilliancy of the sun, owing to the convergence of these rays of light in the upper mirror. If I put the light in the upper mirror instead of the lower one, the rays would be brought to a focus in the lower mirror. I want to show you this with heat; and for that purpose I will take some boiling water. I lower the upper mirror and hang in its focus a flask of hot water; and now we will examine what occurs with the rays of heat. Having placed the flask in position, I draw the mirror up into its former place near the top of the house; and now the rays of heat are coming down from that hot water. Although you can not see them, they are coming down as the rays of light which were given off from the electric light just now. The rays of heat are striking upon the surface of this mirror, and they are collected and brought to a focus here. I think that by means of our beautiful thermo-electric pile I shall be able to show that this is really the case. I now bring the face of the pile under the mirror, turning it downwards—not upwards, towards the hot water. You observe that the needle very soon moves, in virtue of the heat which is reflected by the lower mirror and collected to a focus in this way. I will now turn the face of the pile towards the cool region of the room, and allow its heat to waste itself; and now for the flask of hot water I will substitute a totally different body—a very cold one. I will, in fact, place a freezing mixture in the focus of the upper mirror, and then operate with the pile exactly as I did when the flask of hot water was there. You will now observe that the needle will move in the opposite direction. It will first come down to zero, and then move up on the opposite side. There will be a very sensible deflection, indeed, if I hit the right point. [The deflection took place as indicated.] Now, I dare say many boys here present think that, as rays of heat issued from the vessel containing the hot water, so rays of cold issue from the vessel containing the freezing mixture. That, however, is not the case. In the case of the freezing mixture our thermo-electric pile is the warm body. It is hot compared with the freezing mixture, and that pile radiates its heat against this lower mirror; the heat is reflected above, is re-reflected against that mirror, and is then absorbed and drunk up utterly by the freezing mixture, so that the pile in this way wastes or loses its heat, and therefore gives that deflection of the needle due to cold. Instead of this freezing mixture or the bottle of hot water, I will now place in the focus of the mirror a body which I hope will be given to me in a bright cherry-red hot state. A copper ball has been placed in the fire in the next room; we will suspend that copper ball when it is red hot in the place which was occupied by the freezing mixture, and see whether we cannot get very visible evidence of its radiation. I do not like to use the thermo-electric pile in this experiment; but I have here some black paper, and sometimes we are able to make paper smoke in the lower focus. I place this paper below in the focus, but I see the ball is not hot enough to burn it; there is no apparent action; but I

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can feel the heat very strongly indeed, through the reflection of the rays, so that my hand can not rest there. Some of this paper smoked freely yesterday when brought within the focus. If I place the face of the thermo-electric pile there for a single moment, you will find what I said to be true. The action of the needle proves that you have there the focal heat I have been endeavouring to describe.

Now we have to pass on to the still further consideration of these rays of heat; and I will first of all try to make plain to you wherein consists this wonderful light that we have been operating with so often. I will take a thin slice of this light and try to unravel it before you. The screen will be lowered in order to enable me to do this, and we will lower the roof so as to darken the room. You will see the beam of electric light making itself evident in the dust of the room; and this lens enables me to obtain a beautiful image on the screen. Now I want to twist that beam aside. That white mass of light which you see, is due to a mixture of lights of various colours. I will twist this beam aside by means of a prism, and separate these colours one from the other. First of all I will send the light through a single prism, thus, which gives this wonderful, rich display of colours upon the screen. Nothing can be more beautiful than this—so rich and lovely. And now I will try and make the band still bigger,—not richer: it is impossible to have it richer or more beautiful than that. For the purpose of increasing the size of this band of colours I will send the beam through another of these prisms, which will pull it aside still farther, and spread these colours still more. You now have the beam passing through a second prism, and when I bring the beam into the field you have this splendid band thrown on the screen. This is called a spectrum. This was the great discovery of Sir Isaac Newton. He found that white light was composed of all these colours; and if it were consistent with our present course of lectures, we could make these colours combine again and form white light. We will now turn up the gas, and you see how dead the spectrum becomes when the light falls upon it. I asked for the gas light in order to choose a boy "ruddy, and of a fair countenance." [The lecturer then selected a boy answering to this description, and led him to the screen. The room was then again darkened.] You will find what happens to the colour of his face when I lift him into the midst of this spectrum. Here [holding the boy's face in the red light] he is blooming like a rose. Now [transferring him to the yellow] he is like something very different from a rose.

Now I want to say a few words upon this wonderful spectrum. You see a great mass of light here, and you might suppose that that is all which comes out of that wonderful electric lamp; but that is, in reality, not at all the case. You have here a certain distance which is rendered visible to the eye by these splendid colours, but there are rays extending about as far on the outside of the extreme red, as the green colour is on the other side of it. The most powerful radiation emitted by the electric light does not fall on any part of the visible spectrum, but it falls as far on one side of the red as the green is from the other. And so also at the other end of the spectrum we have a vast body of rays stretching out beyond the visible portion; but all these ultra-violet rays and the ultra-red rays are perfectly incompetent to produce vision, although a great number of them reach the retina. I now want to make evident to you the prolongation of the spectrum in the direction of the violet, and for that purpose I must make use of a less expansive spectrum. We have produced this by means of prisms of liquid, but I must now make use of a prism of glass, or else have only one of the liquid prisms instead of two. I want to give you an idea of the comparative power of the luminous rays and those dark rays I have spoken of. I have now produced this present spectrum by means of one of the liquid prisms. We might, as Sir William Herschel did when he first discovered the dark rays of the sun, place a thermometer in this dark part beyond the red, and we should find that it would show an augmented temperature because of the heat falling on it from the electric light. Then if we travelled from this red end of the spectrum towards the other, we should find that the thermometer would gradually sink, and if we went back again it would rise gradually through the violet, through the blue, through the green, and the yellow, and the orange to the red, the red being the hottest part of the visible spectrum. But Sir William Herschel did not stop here, but made a further discovery. Far beyond the red he found very powerful rays falling upon the thermometer, and he represented the rise of the temperature by lines of certain length. He represented the least heated part by a short line, and the next by a longer one; the line representing the heat of the green is of a certain length; and the heat of the yellow was marked by a longer line still. The whole visible radiation from the sun was determined in this way by Sir William Herschel; but we have now far finer methods, and with the electric lamp which you now see before you, we went over these colours with a thermo-electric pile. The whole radiation of the visible portion of the spectrum is represented by this small coloured area that you see represented on the diagram; but over and above that, and beyond the red end of the spectrum, you have an amount of heat which is represented by this great mountainous peak. The invisible radiation is nearly eight times the visible; that is to say, only one-eighth part of the rays emitted by the electric light is competent to excite vision, all the rest are rays of heat, and not rays of light.

And now I want to show you the prolongation of the spectrum in the other direction; and for this purpose I will make use of a prism of flint-glass instead of this prism of bisulphide of carbon. I place the prism exactly as in the former case; the display of colours is not now quite so brilliant, but the glass is more transparent to the rays that I want to show you than the bisulphide of carbon is. I have here a certain substance called sulphate of quinine; and I have here also a screen of white paper which was wetted with this substance before the lecture. It was found by Professor Stokes that this substance has the extraordinary power of rendering visible these invisible rays of light beyond the violet. Now, observe this band of light which becomes visible beyond the violet, when I introduce the paper screen which has been spread with the sulphate of quinine. There is darkness when the screen is not there, but when it is held up you see this lovely band of colour produced. If I take the liquid itself and daub it upon a piece of paper, it will render the invisible rays visible. I have here also the means of changing the colour of rays by means of this beautiful violet glass, and rendering rays visible which were hardly visible before. Here is a piece of paper, on which are printed the words "A happy new year." As you look at it you see nothing upon it, by the ordinary light, but if we put up the violet glass observe how beautifully the letters come out.

So much, then, for the existence of rays beyond the red end of the spectrum, and also beyond the violet end, which are incompetent to excite vision. These are what are called invisible rays. Before I proceed farther I should like to show you an experiment by means of these powders. Professor Stokes has called that action which makes the sulphate of quinine visible, "fluorescence." The phenomenon called fluorescence has been known to philosophers a long time. It was observed that certain substances had the power, so to speak, of drinking in light, and then giving it out gradually. M. Edmond Becquerel, of Paris, has rendered himself exceedingly famous by his investigations on this subject, and the powders I have here were selected by him. I am indebted to Sir Charles Wheatstone for them. I will show you that if these powders are shone upon by the electric light, and then the lamp is extinguished, the powders will still retain their luminosity; they will still have the power of giving out light. They, as it were, drink in the light and then give it out slowly

and by degrees. [The powders were exposed to the electric light for a short time, and the light was then extinguished.] There, you see the powders are self-luminous, and emit this beautiful light. I have here a beautiful butterfly formed of these powders. It is painted upon glass. You see the surface of the glass is now perfectly dark. It emits no light; but if I allow the light of the sun or the light of the electric lamp to shine upon it for a short time, you will see that it has the power of drinking in that light, and emitting it gradually. [The surface of glass on which the butterfly was painted with the fluorescent powders was exposed to the electric light. The light was then withdrawn and the butterfly was seen to have become luminous.] This beautiful butterfly is produced by means of these fluorescent powders selected by M. Edmond Becquerel.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, February 20th.

Dr. A. W. Williamson, F.R.S., Vice-President, in the Chair.

At this meeting there was an unusually full attendance of Fellows, and several guests, amongst whom were Sir Roderick Murchison, Professor John Morris, and other distinguished members of the Geological Society. The limited accommodation available in the meeting room was altogether insufficient to provide for the large audience that attended on Thursday evening. This inconvenience has been sorely felt on several recent occasions, and particularly when special evenings have been appointed for lectures. It is to be hoped that the new apartments, about to be constructed for the Society in Burlington House, will meet not only present requirements, but provide for future contingencies.

Mr. Martin Murphy, of the College of Chemistry, Liverpool, was duly elected a Fellow of the Society. The names of candidates read for the first time were—Mr. R. Calvert Clapham, Walker Alkali Company's Works, Newcastle-upon-Tyne; Rustomjee Byramjee, M.D., Assistant-Surgeon in Her Majesty's Bombay Army; and Edward Mensel, Ph.D., recommended by the Council as Associate. For the second time were read the names of Benjamin H. Paul, Ph.D., 8, Gray's Inn Square; Edward Dowson, M.D., 117, Park Street, London; Mr. Thomas William White, Ifield, near Crawley, Sussex; and Mr. Reinhold Richter, of the Rothamstead Laboratory, proposed as Associate.

Mr. David Forbes, F.R.S., &c., then delivered a discourse "*On Chemical Geology*." The lecturer confined himself mainly to the consideration of those parts of the subject which comprehended the period coincident with and subsequent to that stage in the world's history known as the cosmogenetic era. Confessing at the outset that he was neither absolutely Plutonic or Neptunic in his opinions regarding the origin of the oldest rocks, the lecturer argued that combinations of the views held by these rival schools of geology best suited the requirements of modern research. It was neither fire alone, or aqueous agency alone, that accounted for all the natural phenomena observed, but to these forces conjointly must be added the effects of heat, electricity, light, and mechanical pressure, as greatly influencing the consideration of the matter in hand. Referring to specimens on the table, Mr. Forbes showed that silica occurred in nature as an igneous product in recent volcanic lavas; as an aqueous product in different forms deposited from solution; and as a gasolytic product in tubes from the decomposition of the fluoride of silicon. Similar modifications were observed in the case of sulphur, copper, and many other substances. The conditions under which the artificial formation of felspar and zeolites was possible were then alluded to, and stress was laid upon the fact that the production of the latter (hydrous silicates) by fire was consistent with the observation that vast volumes of aqueous vapour escaped together with solid and partially liquefied matters during volcanic eruptions. Igneous action in nature was defined to be volcanic action in which the results were much modified by the presence of steam and gases. Aqueous action also was defined to include the action of dissolved saline matter, gases, air, &c., with or without heat and pressure. Going back to the earliest forms of created matter, the chemist assumes that the elements and their affinities were then the same as now, subject, however, to the disturbing causes due to excessive heat or relative bulk; thus, whilst sodium will at comparatively low temperatures decompose carbonic acid, carbon will, on the other hand, take the oxygen from soda if the heat applied be sufficiently intense. So also with iron, which at a red heat decomposes water, whilst hydrogen at the same temperature effects the reduction of oxide of iron. Claiming a certain amount of latitude in the discussion of the states of combination or balance between the affinities of the earth's contending elements, it was conceived that the first operation of the newly created matters would be to obey the law of gravity, and arrange themselves in zones or strata in and upon the earth according to their respective densities, although modified to some extent by diffusion. Defining the relative position of the silicates, those more basic in character and of greater density underlying the acid silicates, containing probably free quartz, the chloride of sodium and other volatile compounds may be conceived to form a dense vapour or atmosphere immediately surrounding the earth, whilst carbonic acid, and, next, the gaseous constituents of air with aqueous vapour in the upper regions, were the outer zones. Later, when by the abstraction of heat the chloride of sodium was condensed, it formed a solid crust of salt upon the surface of the earth, and by a further reduction in temperature the liquefied water would dissolve the salt to form the ocean. Reasons are given for the hypothesis adopted by the author, which asserts that the central nucleus of the earth must contain an accumulation of the denser metals and their compounds; these considerations are founded upon the knowledge of the mean specific gravity of the earth, about 5·4, and the density of the exterior crust, assumed to be 2·75.

By this solidification of the exterior crust, and its becoming subject to volcanic and other forces irregularly exerted, the physical features of the globe were changed from a true sphere to mountains and valleys, some of which have afterwards been covered by the ocean; and then by the disintegrating action of water, the ingredients composing the first formed rocks may have been sorted into sandstones, derived chiefly from the quartz, and the earthy silicates go to form under great pressure and metamorphic action the numerous class of slaty or argillacious stratified rocks. Metallic sulphides occurring in the silicates would by oxidation furnish sulphates, which would also be formed from volcanic emanations, and pass into the sea. Organic life at this stage came upon the scene, and was instrumental in separating the lime as carbonate from dissolved calcareous salts, thus forming limestones; whilst vegetation proceeded apace, and gradually stored up carbon from supplies of carbonic acid abounding in the earth's atmosphere, and fitted the air for the respiration of animals.

The arguments deduced from the specific gravity of the quartz contained in granite as pointing to its aqueous origin, are shown to be fallacious, and the author finds that recent lavas contain quartz of specific gravity 2·6,

which exactly accords with that of the common hexagonal variety known as rock-crystal.

After duly weighing the conflicting opinions which have divided geologists as to the origin of granites, the lecturer was satisfied from his experience in the field, assisted by the microscope and laboratory, that many of the so-called granites and gneisses are really sedimentary products of the breaking up of true igneous eruptive rocks, stratified by aqueous agency, and subsequently re-consolidated. True eruptive granites of igneous or volcanic origin also undoubtedly exist, and the lecturer replied to the arguments put forward by those who dispute such an origin. The objections were taken *seriatim*—1st, That granite contains free quartz; 2nd, That the specific gravity of the quartz is 2·6; 3rd, That the quartz contains water; 4th. That in granite the more fusible minerals have sometimes become solidified and crystallised before the less fusible ingredients; and 5th, That granite frequently contains hydrated minerals. Reference was here made to Bunsen's experiments on the retention of water by hydrous silicates, and Laurent's observations to the same effect in the case of the fused borates. A specimen of crystallised stilbite, found in the lava current from Etna, in March 1865, was exhibited; and the quartz from the volcanic lavas of Peru, and rocks of Ponza, in the Bay of Naples, were said to contain water. In the lava from Vesuvius crystals of refractory leucite were frequently found sitting upon the easily fusible augite. From the general uniformity in composition and physical characters of volcanic products thrown up in such widely distant localities as Iceland and Terra del Fuego, the lecturer argues that there must still exist a vast reservoir or reservoirs of fluid igneous matter in the interior of the earth, and that volcanic eruptions must have some intimate connection with one another. Volcanic action does not seem to be confined to mere local outbursts, for in the Pacific enormous energy is shown in the numerous volcanic islands lying between 80° and 130° west longitude, which includes a range of nearly one-seventh of the total circumference of the globe. By way of conclusion to his discourse the lecturer divided the forces determining metamorphic action into six principal classes, considered under the following headings:—

1st. Pressure alone.
2nd. Heat alone.
3rd. Heat in conjunction with chemical action and crystallisation.
4th. Aqueous action, assisted by heat and pressure.
5th. Gasolytic action.
6th. Combinations of two or more of the above agencies.

The author's aim was neither the introduction of novelty, nor the rehearsal of published opinions with a statement of authorities; but to bring together a mass of "dissociated data," examine the soundness of the separate parts, and, if possible, build up a structure upon which the criticism of both chemists and geologists could be centred.

The Chairman moved a vote of thanks to Mr. David Forbes for his highly interesting communication, which bore evidence of much study and thought, and invited an expression of opinion from Sir Roderick Murchison and the other geologists whom he saw in the room.

Sir Roderick Murchison said he had listened with great pleasure to the able discourse just now delivered by Mr. Forbes, but felt as yet incompetent to offer any opinion upon the great chemical questions treated of in the paper, particularly those referring to the primitive constitution of our globe, in which the lecturer had grappled so manfully and so successfully with the advocates of the water hypothesis. The facts stated, and inferences deduced from the structure of rocks, go far towards invalidating the opinion of those who assert that the older granites are really sedimentary formations. The igneous origin of at least some of the granitic rocks seemed all but proved, but gneiss may be difficult to determine. With regard to intrusions several interesting examples had been mentioned. When grand ranges of limestone became suddenly changed into gypsum and dolomite, the true explanation could only be furnished by chemical investigation; Daubrée had already done much, and Forbes showed himself ready and willing to go into the arcana of these mysterious regions of speculation, and seek the truth for our science of Geology.

Prof. McDonald acknowledged himself to be a believer in the Neptunian system, and conceived that there was no clear line of demarcation between the granite proper and mica slate; he saw no reason to apply the term "metamorphic" to the latter, since the same ingredients were present in both, and the difference between adjoining portions of rock were often difficult of recognition. If the cavities in the quartz of true granite were carefully examined, they would be found lined with crystals or bounded by plates, and totally different from the hollow amygdaloid spaces occurring in volcanic lavas, and that the fluid contained in them was of an explosive nature.

Professor Morris disputed the accuracy of the inferences drawn from the occurrence of zeolites, which had in the speaker's opinion been crystallised from water. This class of minerals never occurred in modern lavas, but were generally found in the older rocks. Stilbite, chabasite, &c., may be regarded simply as modifications of ordinary felspar. The subject of metamorphism required elucidation from the chemist, and some interesting facts had been brought forward in the paper. Remarkable examples of alteration at the junction of rocks were to be seen in the passage of granite amongst limestone. Where grey granite intruded into mountain limestone, the lime felspar was produced, which segregated out in different forms of metamorphism.

Dr. Hugo Muller said, "I have listened with great pleasure to the very interesting discourse of my friend Mr. Forbes, and in pronouncing my concurrence with most of the views put forth, I cannot help expressing some doubt with regard to the validity of his arguments in favour of the igneous origin of the quartz in some of the granite and similar rocks. Mr. Forbes regards the separation of graphite from pig iron, as shown in the beautiful specimen placed before us, as analogous to the separation of quartz in granite, inasmuch as both substances previous to their separation were in the state of igneous solution. Now it appears to me that this analogy is only very superficial, for in the case of pig iron we see the more infusible graphite separate in a *solid* and crystalline form as soon as the affinity to the iron ceases, whereas, on the other hand, in the case of the granite, we have the undisputed fact that the more fusible felspar has separated and crystallised first; for the quartz surrounds, and, in fact, to a great extent fills up the space between the crystals or particles of felspar. On the other hand, the well defined surface of the felspar and the absence of all indication of partial interfusion on the faces of contact between the two minerals, excludes all probability that these felspar particles could ever have been in contact with fused quartz, leaving untouched the question whether the once fused quartz is capable of passing again into the crystalline state without solidifying. Mr. Forbes, in support of his views, quotes the highly interesting and important researches of Mr. Sorby on the structure of water cavities in quartz and other minerals. If I recollect rightly, the highest temperature deduced from their experiments for the formation of these cavities, is about 400° C., or about the melting point of lead. But surely this is not a temperature which we can call 'igneous,' or associate with plutonic action, it is in fact a temperature which we, if the present theory on the subject is correct, may find anywhere at a depth of about 20,000 feet below the surface of the globe, and to which in the course of time any of the sedimentary formations may have been subjected. The trachytes of Ponza and

Palmarola, a rock of divided volcanic origin contains crystalline quartz, the water cavities of which are, according to Mr. Sorby, formed at a temperature of about 360° C. This fact in itself I consider a proof that their quartz is a secondary product, and could not have crystallised at the time of the eruption of their lava, for it is inconceivable that quartz could remain liquid at the temperature of melting lead. It is hardly necessary to mention that the eruption of these trachyta has taken place in pre-historic times. The fact that quartz and zeolites have been taken from the still flowing lava is not more conclusive, for it seems more than probable that these minerals, along with many others generally named amongst the Vesuvian ejections, are nothing more than particles of the ancient Monte Somma formation, underlying the present volcano, which during the eruption of Vesuvius come occasionally within reach of the lava, and are then ejected from the crater. I have arrived at this conclusion after a personal inspection of the Monte Somma formation, which in reality consists of the lavas, ashes, or tufa, and débris of the ancient volcano mixed up with occasional fragments and blocks of limestone. In the course of time a metamorphic or chemical action has set up in their mineral chaos the result of which are those numerous well crystallised minerals which are found in such positions as to quite exclude the idea of their formation having taken place simultaneously with the Monte Somma itself. If we find these very same minerals, sometimes even in the very same kind of geodes and association in which they occur at the Somma, ejected from the crater of Vesuvius, I think we may safely conclude that they are *not* the products of the active volcano."

Dr. B. H. Paul considered that an effort should be made towards establishing the broad principles upon which chemists were required to investigate geological phenomena. Schistose rocks were found underlying, or formerly did so, other sedimentary strata. This being the case, the examination of the chemical features of difference was a matter of importance, particularly in the event of their becoming crystalline. The speaker could not agree with Mr. Forbes in considering that the uniformity was not so great in sedimentary as in crystalline rocks. The former class were remarkable for their uniformity; thus mica-schist, chloritic-schist, and hornblende exhibited differences only of small degree. For his own part, whilst he abandoned both the plutonic and aqueous theories, he could not adopt Mr. Forbes' reasoning in respect to the quartz in granite.

Dr. A. W. Williamson agreed with the lecturer in most of his arguments, but there was one point in his "chapter of Genesis" which seemed to require further explanation. It had been stated that in the primeval atmosphere the gases would arrange themselves, or be stratified, in the order of their density, but for his own part he should not have expected to find them in this order, but rather obeying the law of diffusion. Some time since, when visiting the blast furnaces of the Cleveland district, he was much struck by seeing a block of slag, weighing perhaps 2 tons, standing upon an iron truck having been just run from the furnace, and whilst cooling a workman perforated the upper crust, when a stream, as of lava, flowed from the aperture, being forced out by the contraction on all sides of the mass. t

Mr. Forbes, in reply, reminded Professor McDonald tha he did not pin his faith to any school of geology, and with respect to the cavities in quartz, had always found them very irregular, and certainly not, as a rule, bounded by plates or lined with crystals. Although admitting that zeolites were usually so formed, he could not agree with Professor Morris in considering that they were, in every instance, formed from solution by subsequent aqueous infiltration; although he was indebted to that gentleman for an admirable illustration in the specimen from the aqueduct of Plombières now upon the table. He once had occasion to send a mass of volcanic lava containing zeolites to a lapidary to be cut across; during the process of cutting, water had been used, and so great an action did it exert upon the mass of the rock itself, that it appeared incredible that the zeolites in its interior had been last formed by aqueous infiltration. Mr. Forbes fully agreed with Dr. Müller that many if not most of the Somma minerals could not be regarded as true volcanic products, but it was far different with many of the great eruptions of quartose lavas of enormous extent occurring in other parts of the globe. For some 600 miles along the volcanic range of the Andes of Chili and Peru, quartz in hexagonal crystals occurred in the volcanic rocks, and the microscopic examination of the quartz of recent lavas by Mr. Sorby, showed abundance of "glass cavities" which could only be the result of fusion. The conjoint influence of heat, water, and great pressure, might bring about results which were impossible with heat alone; and this was in harmony with the known prevalence of aqueous emanations (steam) from volcanoes. Mr. Forbes fully admitted that under such influences, the chemical reactions in such volcanic and granitic eruptive rocks may have taken place at temperatures even below a red heat; yet considers this as no reason for not considering them as igneous, since it must be remembered that in geology the terms igneous and volcanic are synonymous. In answer to Dr. Williamson, the speaker stated that the element, time, must be taken into account in estimating the effects of diffusion; he relied upon the instantaneous production of the gases permitting them to obey the laws of gravity, in the first instance, although he admitted that any such arrangement in the atmosphere would ultimately be obliterated by diffusion.

The meeting was then adjourned until Thursday, 5th March, when the following papers will be read, viz.—"*On the Action of Oxidising Agents on Organic Compounds in the presence of an excess of Alkali*"—*Part I.* "*Ammonia evolved by Alkaline Permanganate acting on Organic Nitro-compounds.*" By Messrs. J. A. Wanklyn and E. T. Chapman; "*Note on Dr. Frankland's Process of Water Analysis*," by Mr. E. T. Chapman; "*On Chloranil*," by Dr. J. Stenhouse, F.R.S.; "*Action of Nitric Acid on Picramic Acid*," by Dr. J. Stenhouse, F.R.S.; "*On the Hydride of Accto-salicyl*," by Mr. W. H. Perkin, F.R.S.; "*On the Crystalline form of Arsenious Oxide*," by Mr. F. A. Claudet; "*On the Detection and Estimation of Nitrates in Potable Waters*," by Mr. E. T. Chapman; "*Action of Zinc Ethyl on Nitrous and Nitric Ethers*," by Messrs. E. T. Chapman and Miles H. Smith.

On Thursday, March 19th, Mr. Henry Chance, M.A., will deliver a lecture "*On the Manufacture of Glass.*"

CORRESPONDENCE.

CRYSTALLOGRAPHY AND THE BLOWPIPE.—LAW OF HORIZONTAL CRYSTALLISATION.

To the Editor of the Chemical News.

Sir,—May I ask you to allow me to add to the paper published in last Friday's Chemical News, that having by the kindness of the Secretary to the R. A. Institution been allowed the use of their splendid compound microscope, by Smith and Beck, I have been able, since that paper was written, to examine the diaphanebulous vesicles whose crystals, appearing at first like a slight cloud, were far too minute to be distinguished by my pocket lens.

Under an object glass, magnifying 1000 diameters, the primary crystals of baryta had that peculiar hieroglyphical appearance which I have termed grammate. Those of silver were like small flowerets, with three petals, and sulphur (whose vesicle was nebulous) appeared in myriads of tripedal marks like "crowfeet." Under the same glass the zones of the magnesian disc I found to consist of innumerable dark, if not black spots, too small for their shape to be distinguished, even by this powerful lens.

It is now, I think, evident, and I think I may fairly claim the discovery of the fact, that—"When the process of crystallisation in nature is confined to the plane of the superficies of the crystal, and not allowed to proceed in a direction either above or below it, as is the case in the thin 'walls' of the borax vesicles made by me, a distinct system of crystallisation is followed, producing forms widely differing from those generated under other conditions,—never geometrical, generally in the shape of flowers, ferns, trees, or stars, and not isomorphous."

I have the pleasure also to inform you that pieces of the crystalline vesicle can be fastened on clean smooth glass merely by the pressure of a finger, so firmly that they cannot be easily rubbed off, and may be carried about, forming excellent slides for the microscope; when no longer required, they can be washed off with soap and water. I tried electrifying the glass previously, but the vesicles being attracted electrically, they were of course soon repelled.

The truth of the above law may be easily demonstrated by an experiment which I have made since the above was written. I placed a solution of common salt in cold distilled water between two plates of glass, under a pressure of 5¾ lbs.; next morning a reticulate crystallisation was observable on the inner side of both plates, while some drops of the solution, left on the platinum spatula, with which I had mixed it, had crystallised in a modification of the cube.

Nitre treated in the same way produced a kind of floral net-work, while outside it assumed the usual prismatic needles. Carbonate of soda crystallised in a very distinct dendroidate form.

It is necessary to use cold water, because if warmed with some substances, as nitre, the secondary or isomorphous crystallisation is set up so rapidly that the primary kind has not space to form.

It would appear from this that although the law of planiform crystallisation, as above demonstrated, holds good, primary crystals from solution by fire are different from those produced by a solution in liquids.—I am, &c.,

W. A. Ross.

Woolwich, 24th February, 1868.

PHOSPHORESCENCE OF POTASSIUM AND SODIUM.

To the Editor of the Chemical News.

Sir,—In your issue of Jan. 31, 1868, is an extract from the *Journal für praktische Chemie* relative to the oxidation of potassium and sodium. It is there stated that "the oxidation of potassium and sodium, when exposed with a clean surface to the air, is accompanied, according to H. Baumhaur, with evolution of light."

Mr. H. Baumhaur thinks, doubtless, that he is the author of this discovery, but his observation is, in reality, about 17 years old. In the year 1851 M. Pétrie discovered that the metal potassium is phosphorescent when exposed to the air, like phosphorus. He covered the potassium with bees'-wax, and then cut it into two. Each segment remained luminous for about half an hour, the light being one-tenth the intensity of that produced by a piece of phosphorus of the same size.

In 1859 Herr Linnemann, ignorant of M. Pétries' observation, published another note (in the *Journal für praktische Chemie*, lxxv.) upon the same subject. He showed that both potassium and sodium are luminous upon their freshly cut surfaces. The light emitted by potassium is of a reddish tint, that of sodium greenish, according to this author. At 60° or 70° C. the light of sodium is quite as intense as that of phosphorus at the ordinary temperature.

In 1859 I also had occasion to examine the same phenomenon, and recorded it in 1862 in a work which has been more than once quoted in your valuable journal. I found the light of sodium to be very feeble at the ordinary temperature of the atmosphere, and that it ceased when the newly exposed surfaces are covered with a layer of soda. The luminosity lasts for a few minutes, and increases in brilliancy as the temperature rises. Potassium also becomes vividly phosphorescent in the preparation of boron.—I am, &c. T. L. Phipson, Ph.D.

The Cedars, Putney, S.W., Feb. 12, 1868.

THE ROYAL SCHOOL OF MINES.

To the Editor of the Chemical News.

Sir,—Now that the subject of technical education is under discussion, I think that perhaps it might not be amiss to say a few words about the Royal School of Mines.

One, and the principal reason why our Royal School of Mines turns out so few scientific men, in comparison with the corresponding French and German Institutions, is because it is so little known, and many who are aware of the existence of it know little or nothing of its mode of working. The School is itself well worthy of a higher reputation than is at present accorded to it; the Professors are among the most eminent men in their several departments; and the course of study prescribed for the students, extending over a period of three years, and embracing several distinct branches of science, seems to demand more general recognition as an efficient and thoroughly practical scientific education. The School at present is merely an appendage of the Geological Museum, instead of being an institution distinct from everything else, as is the case with the French School of Mines.

In 1854 the Ecole des Mines had 600 associates, whereas ours at the present day has only 40; and as it has been established 17 years, this shows an increase of only 2½ per annum.

I have no doubt that were the Institution brought more prominently before the public the number of students would be greatly increased, and this might be done in several ways.

1st. The chemical and metallurgical laboratories, lecture theatres, &c., should be all in one building. The present arrangement involves considerable loss of time and inconvenience.

2nd. There should be a public opening of the school at the commencement of each session, and addresses should be given, the same as in all our medical schools.

3rd. The diplomas of associateship, scholarships, prizes, certificates, &c., should be awarded publicly at the end of the term; this is done, I believe, at every other educational establishment of any standing.

4th. The "prospectus" and calendar (if it be worthy the name) should be printed and kept in a separate form, instead of, as now, being printed on some of the spare pages of a pamphlet belonging to the Geological Museum.

I am, &c., A. L. E.

MISCELLANEOUS.

College of Chemistry.—All chemists who have studied at the Royal College of Chemistry, as well as present students, will be sorry to hear of the death of Richard Coppins, who has acted as porter in that Institution for more than twenty years. He was particularly obliging and ready in attending to the wants of the students. Apart from his other duties he did some business in chemicals and apparatus, which he was always ready to buy or sell; and most who have passed a session at the College have felt the convenience of having Richard's varied stock to select from in an emergency. He died last week of apoplexy.

CONTEMPORARY SCIENTIFIC PRESS.

Under this heading it is intended to give the titles of all the chemical papers which are published in the principal scientific periodicals of the Continent. Articles which are merely reprints or abstracts of papers already noticed will be omitted. Abstracts of the more important papers here announced will appear in future numbers of the "Chemical News.")

December 2, 1867.

Sir David Brewster, "Letter to Chevreul on the Authenticity of the Newton and Pascal Correspondence." Chasles, "Remarks on the preceding Letter, and on one from G. Govi on the same Subject." L. Larroque, "On a Collection of Geological Specimens from Chili." G. Govi, "Remarks on the Letters alleged to have been written by Galileo, which have been published with reference to the Newton and Pascal Correspondence." T. Schloesing, "On the Simultaneous Estimation of Carbon, Hydrogen, and Nitrogen in the elementary Analysis of Organic Substances." Musculus, "On the Hydrates of Stannic Acid." Alvergniat, "An Apparatus for showing that the Electric Spark cannot pass through a Perfect Vacuum." Langlois, "On the Formation of Cyanide of Ammonium by passing the Vapour of Ammonia over Incandescent Charcoal."

Bulletin de l'Academie Royale de Belgique (Classe des Sciences).

October 12.

"Obituary Notice of Michael Faraday." Haidinger, "On the Collection of Metcorites at the Museum of Vienna."

Annalen der Chemie und Pharmacie.

October.

A. Butlerow, "On the Derivatives of Trimethylcarbinol (Tertiary Pseudobutylic Alcohol). On the Isomerism of the Saturated Hydrocarbons C_4H_{10} and of the Butylenes C_4H_8. On Isobutylic Alcohol (Primary Pseudobutylic Alcohol or Pseudopropylcarbinol)." "On the Action of Water on the Chlorides of some Alcohol Radicles." "On the Occurrence of Tertiary Pseudobutylic Alcohol amongst the Products of Fermentation." "On the Action of Hydriodic Acid Gas on the Iodides of the Alcohol Radicles." "On the Crystalline Form of Hexamethylenamine." "On the Non-poisonous Properties of Zinc Methyl." "On the Preparation of Chlorhydrin of Glycol by Carius' Process." A. Butlerow and M. Ossokin, "On Iodhydrin of Glycol and on a new Method of Forming Alcohols by Synthesis." H. Schiff, "On the Ammoniacal Derivatives of Isatin." Schwarzenbach, "On the Mutual Relations between the Equivalents of Albuminoid Substances." P. Lessen, "On some Oxidation Products of Naphthaline." W. Heintz. "Note on the Preparation of Diglycolate of Lime." "On the Action of Dry Carbonate of Soda on Monochloracetic Ether. On Diglycolic Ether and Diglycoldiamide." A. Bettendorff, "On the Allotropic Conditions of Arsenic."

Annales de Chimie et de Physique.

November.

J. Kolb, "Researches on Chloride of Lime, being an Introductory Essay on the Use of that Substance for Bleaching Fabrics." E. G. Lambert, "Analytical Researches on the Nature of the Potable and Mineral Waters of Orizaba, Queretaro, and Monterey, Mexico."

Journal für Praktische Chemie.

November.

D. Huizinga, "On the Detection of Ozone, and on the Presence of this Substance in the Atmosphere." F. Reindel, "On Basic Sulphates of Copper." "On the Production of Prussic Acid from Ferrocyanide of Potassium and Sulphuric Acid." Buchner, "Chemical Analysis of the Mineral Waters of Neumarkt, Upper Palatinate." H. Gruner, "Contributions to the Knowledge of Binitrophenylic Acid." W. Kubel, "On the Estimation of Nitrous Acid by means of Permanganate of Potash." R. Wagner, "On the Solubility of some Earthy and Metallic Carbonates in Water impregnated with Carbonic Acid under Pressure." F. Reindel, "On Soluble Prussian Blue."

NOTES AND QUERIES.

Oxychloride of Magnesia.—If J. E. Hamilton will apply to J. B. Giles, at this address, he may hear of a crude carbonate of magnesia which would doubtless answer his purpose.—Borax Works, Old Swan, Liverpool.

Solvent for Essential Oils.—The only available solvent for your correspondent's purpose is perhaps water itself, in which the oils may be dissolved, or at least mixed in a way which will be equivalent to solution, by the process used by many druggists for making their "aquæ." For every ½ oz. of oil put a good handful of $MgCO_3$ into a mortar and triturate it well with the oil until the latter is thoroughly divided, then add water paulatim, triturating diligently all the time, and filter. The division of the oil is facilitated by previously diluting it with its own bulk of strong spirit of wine, in which case add the water only a few drops at a time at first, but this is not indispensable.—Volta.

Estimation of Tannin.—The most reliable and undoubtedly best mode to estimate tannin is the process devised by Dr. H. Fleck, but a full and complete account of this mode of estimation cannot be given, in N. and Q., since the space cannot be spared. If "Astringent" would give his address I shall be happy to give him a full account of this mode, and also of Müller's mode for estimating tannin. As regards the percentage of tannin in tanning and dyeing materials, the following information may perhaps serve the purpose: oak-bark varies from 10·86 per cent to 15·83 per cent, *Polygonum bistorta* varies from 17 to 21 per cent; sumac, from 12 to 18 per cent; dividivi, from 18 to 25 per cent; nut-galls (Aleppo galls), from 58 to 66 per cent; valonia, from 40 to 45 per cent per cent; catechu, Gambia, from 40 to 50 per cent of a peculiar tannic acid; kino contains from 30 to 40 per cent of the same principle as catechu.—Dr. A. A.

Hofmann's Modern Chemistry.—Lecture Experiments.—Your "Notes and Queries" often have been most valuable to many who seek for information, and cannot find it in books which they consult; for it is a well known saying, that you may have many books and yet never find what you require. The reason I think is, that authors do not trouble themselves about minor points of a subject which perhaps they think only trifles, forgetting all the while that trifles make perfection, and perfection is no trifle. As an illustration of this point I have been reading and closely studying that admirable work "Hofmann's Modern Chemistry," and to make myself master of its contents, I performed nearly all of the experiments, having apparatus made especially for the occasion; it certainly is one thing to read a book of experiments, and another to work them out, as I found when I commenced operation. The first difficulty I encountered was the examination of HCl gas by sodium amalgam. The question I should like answered, is, what per-centage of sodium ought you to use to form the amalgam for this purpose? I have consulted several works, but cannot obtain the desired information. When you have finished the experiment, and emptied the U-tube of sodium amalgam, what is the best method of recovering the pure mercury? I poured warm NH_4Cl upon the very weak amalgam, and let it stand for some days. Is that the best method? My second stumbling-block was in decomposing HCl by the electric current. What ought to be the sp.gr. of the acid? The book says, 1·1. That is very indefinite, because there are about a dozen beginning with 1·1. Does it mean the HCl of commerce, sp.gr. 1·16? In using this said acid I was not successful in performing the experiment, because the liquid became so hot by the action that HCl gas was evolved along with the gases H and Cl. I certainly must admit that these are trifling points, but then they are so necessary for the success of an experiment, and in performing new experiments one desires to know all the little difficulties which may beset them. If some of your corrospondents will help me, I shall feel obliged.—Amateur.

MEETINGS FOR THE WEEK.

Monday.—Royal Institution, 2. General Monthly Meeting.
—— Medical, 8.
Tuesday.—Royal Institution, 3. "On Historical Portraiture," Mr. G. Scharf.
Wednesday.—Society of Arts, 8.
—— Pharmaceutical, 8.
Thursday.—Royal Institution 3. "On Historical Portraiture," Mr. G. Scharf.
—— Royal, 8½.
—— Chemical, 8. "Action of Oxidising Agents on Organic Compounds, &c.," Messrs. Wanklyn and Chapman. "On Chloronil, and on the Action of Nitric Acid on Picramic Acid," Dr. Stenhouse. "On Hydride of Aceto-Salicyl," Mr. W. H. Perkin.
—— Royal Society Club, 6.
Friday.—Royal Institution, 8. "On some of the Conditions of Menta Developement," W. Kingdon Clifford, Esq., B.A. Cantab.
—— Geologists' Association, 8.
Saturday.—Royal Institution, 3. Professor Roscoe, "On the Non-Metallic Elements.

PATENTS.

Communicated by Mr. Vaughan, F.C.S., Patent Agent, 54, Chancery Lane, W.C.

GRANTS OF PROVISIONAL PROTECTION FOR SIX MONTHS.

3518. A. T. Carr, Winkleigh, Devonshire, "An improved manure."—December 11, 1867.

3544. J. H. Johnson, Lincoln's Inn Fields, "Improvements in the manufacture of artificial fuel."—A communication from A. F. P. A. G. Decamps, T. de Joly, and F. F. Chollat, Paris.—December 13, 1867.

3559. J. Hargreaves, Appleton-within-Widnes, Lancashire, "Improvements in the manufacture of soda and potassa."

3566. A. M. Clark, Chancery Lane, "Improvements in the extraction of ammonia from fermented and other liquids, and in the regeneration of the agents used in such extraction."—A communication from A. Coste, and L. T. de Rosnay, Boulevart St. Martin, Paris.—December 14, 1867.

3573. W. Huskisson, Swinton Street, Gray's Inn Road, Middlesex, "Improvements in the manufacture of soda and other aërated waters, and in the manufacture of aërated bread."

3574. J. Dawson, Greenock, Renfrewshire, N.B., "Improvements in treating sugar syrup."

3586. W. Ross, Grove Street, Walworth, and A. Long, Aylesbury Terrace, Walworth, Surrey, "Improvements in means for preventing and removing incrustation in steam boilers."—December 17, 1867.

3614. W. H. Richardson, Glasgow, N.B., "An improved apparatus to be employed in the manufacture of iron and steel."—December 19, 1867.

NOTICES TO PROCEED.

2292. W. R. Dawson, Great Saint Helen's, London, "Improvements in the preparation of titaniferous iron sands for smelting."—Petition recorded August 9, 1867.

2305. R. Girdwood, Edinburgh, Mid Lothian, N.B., "Improved composition to be applied to sheep and other animals, for the purposes of destroying vermin, and parasitical life thereon, and form protecting them therefrom."—August 10, 1867.

2308. C. D. Abel, Southampton Buildings, Chancery Lane, "An improved method or process for removing sulphur, phosphorus, and other impurities from iron, steel, and other metals."—A communication from J. F. Bennett, Pittsburgh, Penn., U.S.A.

2314. A. McDougall, Manchester, "Improvements in the extraction and separation of the sulphur contained in products resulting from the alternate exposure of certain metallic oxides to gases containing sulphuretted hydrogen, and to oxygen."—August 12, 1867.

2344. J. T. Way, Russell Road, Kensington, Middlesex, "Improvements in treating phosphate of lime for the manufacture of manure, and for other purposes."—August 14, 1867.

2377. J. Hooker, Oberstein Road, New Wandsworth, Surrey, and F. Braby, Euston Road, Middlesex, "Improvements in fire lighters, also applicable for fuel generally."—August 19, 1867.

2410. J. G. Marshall, Leeds, "Improvements in solvent or detergent processes."—August 22, 1867.

2436. E. Sonstadt, Manghold, Isle of Man, "Improvements in the treatment of sea-weed for obtaining valuable products from it."—August 26, 1867.

2522. F. Versmann, Ph.D., High Street, Stratford, Essex, "Improvements in the manufacture of varnishes."—September 5, 1867.

2541. J. Whitham, Kirkstall Road, Leeds, "Improvements in machinery for puddling, and in puddling and other furnaces."—September 7, 1867.

2546. W. E. Gedge, Wellington Street, Strand, Middlesex, "Processes of extraction of the colouring matter of indigo from the waste textile fabrics which contain it."—A communication from C. Bernard, P. Scheurer, and J. B. Tempé, Colmar (Haut Rhin), France.

2549. F. Tolhausen, Boulevart Magenta, Paris, "An improved process and apparatus for instantaneously disinfecting fecal and manuring matters, improving the same, and also rendering them fit for feeding domestic animals."—A communication from L. J. B. A. Lemoine, and A. M. Turrell, Paris.—September 9, 1867.

3248. J. Swindells, Kegworth, Leicestershire, "Improvements in the process of, and in apparatus employed in treating and separating minerals, earths, and other substances, when ground or pulverised."—November 16, 1867.

3384. J. Baylis, Durdham Down, Bristol, "An improved chemical preparation or compound to be used in preparing mixed textile fabrics for dyeing or colouring."—November 28, 1867.

3389. C. Albiser, Mincing Lane, London, "Improvements in the preparation of sulphate of magnesia applicable to the treatment of the crude potash salts of Stassfurt, and the refuse from the manufacture of chloride of potassium."—A communication from J. Vorster, and H. Grüneberg, Cologne, Prussia.—November 29, 1867.

3440. J. Gjers, Middlesbrough, Yorkshire, "Certain improvements in the manufacture of cast steel and homogeneous iron."—December 3, 1867.

3469. P. G. L. G. Designolle, Rue de La Steine, and I. Casthelay, Rue Ste. Croix de la Bretonnerie, France, "Improvements in the manufacture of explosive and fulminating powders."—December 5, 1867.

3473. J. Durrans, Thurlstone, near Peniston, Yorkshire, "An improved material or composition to be employed for covering or coating the interior surfaces of moulds, crucibles, or ducts, previous to their receiving the molten metal in the process of casting, and for other purposes."—December 6, 1867.

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THE CHEMICAL NEWS.

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ON SOME POINTS IN CHEMICAL GEOLOGY.

By DAVID FORBES, F.R.S., &c.

No. III. Dr. Sterry Hunt's Geological Chemistry.

In considering the mutual relations of the sciences of chemistry and geology, the student must always bear in mind which of these two sciences is to form the basis or starting-point for his inquiries, since this cannot fail to exercise an important influence on his reasonings and deductions.

In what Dr. Hunt calls my chemical geology, I have taken geology as my basis, and then endeavoured to apply chemistry, especially experimental chemistry, to the explanation of known geological phenomena.*

On the other hand, however, Dr. Hunt, in what may be termed his geological chemistry, starts from data purely chemical, and then looks round for geological instances to which these may be applied; thus, for example, starting from the fact well known to chemists, that a solution of carbonate of soda will precipitate carbonate of lime from a solution of chloride of calcium, he at once asserts that—

> "The whole of 'the calcareous strata, the marble and various limestones which we find on the earth's surface' have been precipitated from the ocean by a solution of carbonate of soda."

Again, observing that in the laboratory the reactions of the compounds of magnesia with carbonic acid under a dense atmosphere of that acid, might be used in facilitating the separations of the sulphate of lime (gypsum), or of the double carbonates of lime and magnesia (dolomites), he at once jumps at the conclusion—

> "That all the magnesian limestones and gypseous strata from the most ancient up to those of the tertiary period, were formed in a dense atmosphere of carbonic acid."

In the face of these assumptions, I contend and feel confident the geological world will support me in believing, that no geologist whosoever if applying the study of chemistry to the explanation of the phenomena of his science could by any possibility ever have arrived at such sweeping generalisations.

When the safety of Rome was endangered by the victories of Hannibal, the advice of Scipio to the Romans was to save Rome by attacking Carthage; and the communications of Dr. Hunt contained in the Chemical News of January 17th, and the *Geological Magazine* of February 1st, evidently prove that he is determined to pursue a similar course, yet I trust with a very different result, since in the present case I imagine that the forces at command will be found quite adequate for offence as well as defence.

In this discussion, however, much more trouble is likely to be caused to me by the method in which Dr. Hunt carries on his scientific warfare, and which seems to partake of the character of the country in which he resides, where the Indian system used to be to worry out the enemy by skirmishing, but never to attack strong points; and the history both of scientific discussion as well as of nations, shows how very effective such a plan of operations may prove even in the defence of a very weak cause.

For this reason, therefore, I have considered it prudent to keep the main points under consideration as prominently in view as possible, and not to allow the discussion to become so diffuse as to risk losing sight of them, which I fear the reader of Dr. Hunt's communications may be likely to do; acting on this determination, therefore, I have in my reply to Dr. Hunt's paper in the Chemical News, given a plain and concise statement of the points (numbered 1 to 9) in which I have presumed to differ from Dr. Hunt's opinions and as I now find nothing in his subsequent communication to the *Geological Magazine* of February 1st, which could in any way tend to shake my previous conviction as to the unsoundness of these points, I must be content to wait until Dr. Hunt may condescend to bring forward further evidence in their defence.

If now, however, after a perusal of Dr. Hunt's paper in the February number of the *Geological Magazine*, it is compared with the text of his previous communication in the Chemical News of January 17th, it will be perceived, as the editor of the *Geological Magazine* has already observed, to be to a great extent the same, and in many parts even *verbatim*; and remembering the puerile accusation brought agalnst me by Dr. Hunt, that I "for some unknown reason withheld from the readers of the Chemical News" matter which I published in the pages of the *Geological Magazine*, it really is amusing to observe that Dr. Hunt in like manner has reserved for the readers of the *Geological Magazine* several most interesting observations which probably he may have considered (and with some reason also) as beyond the capacity of the chemists who patronise the Chemical News; as, for example, the following lucid expositions:—

> "As for the noble metals, whose compounds with oxygen are decomposed at elevated temperatures, their great volatility as compared with earthy and metallic oxides would keep them in the gaseous form till the last stage of precipitation of earthy oxidised matters, when by far the greater part of the globe was probably solidified. Hence we now find them in the earth's superficial crust."

And a little further on, he adds:

> "We cannot conceive anything else than the production of a homogeneous oxidised silicated mass, upon which at a late period would be precipitated the noble metals."

Chemists will not require any comments upon the above, but as they have been accustomed to regard some of the noble metals, platinum for example, as amongst the most refractory bodies known, they will be interested in Dr. Hunt's discovery of their great volatility at heats below which silicates solidify at, as well as the information that the extreme refractory nature of the other metallic oxides had been so completely demonstrated, since some of them at least, as lead, bismuth, antimony, molybdenum, &c., have not been hitherto so considered.

Geologists, however, will not all feel convinced by Dr. Hunt's mere assertion, that the noble metals have from the beginning been in the earth's superficial crust, precipitated on to it from the skies like Jupiter's golden rain, but may also be inclined to believe that they may possibly have been carried up from below.

By a curious coincidence my answer to the criticisms of Dr. Hunt, which appeared in the following week's Chemical News, is also to be found in the *Geological Magazine* of Feb. 1, in which Dr. Hunt's second communication appeared, and I was glad to find that had I even been previously acquainted with the contents of this

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* Here it should be explained that Dr. Hunt, by his having some time back published, both in France as well as in England, an outline of his principles of chemical geology, has fairly laid himself open to having his views both criticised and disputed, whilst, on the contrary, Dr. Hunt's knowledge of my views on this subject appears to have been derived from sketchy allusions to my opinions scattered throughout the two papers in connection with this discussion contained respectively in the *Geological Magazine* of Oct. 1, and the Chemical News of Oct. 4, last year. Although his virulent onslaught might for this reason be considered hardly as altogether fair, I so far from objecting to it, am on the contrary truly thankful to Dr. Hunt for thus enabling me to strengthen any weak points, and for inspiring me with more confidence than before in the views on chemical geology since brought forward by me and now in the press.

latter paper, I would not materially have altered my remarks, although I should have added a few more words in reply to some minor points brought forward by Dr. Hunt. which did not appear in his previous one in the Chemical News.

The only important one now advanced is set forth in Dr. Hunt's courteous request for Mr. Forbes to explain "the intervention of water in all igneous rocks, which, as he declares, are outbursts from the still fluid interior of our globe."

The above words do not exactly express my views, since I maintain that igneous rocks have their source in some *reservoir or reservoirs* of still fluid matter in the earth's interior; and I see no difficulty in explaining, by the action of capillarity and heat, the infiltration of the requisite amount of water for the supply of such a source.

Not wishing, however, to accuse Dr. Hunt of "unfamiliarity with geological literature," to use his own words, I could not suppose him ignorant of the writings of Daubrée, whose labours in the field of experimental geology are well known, and it seemed strange that Dr. Hunt should have overlooked the fact that this question had been fully answered by this gentleman, whose words are—"En résumé sans exclure l'eau originaire et en quelque sorte de constitution initiale, que l'on suppose généralement incorporée aux masses intérieures et fondues, M. Daubrée est porté à conclure de l'expérience ci dessus relatée, que l'eau de la surface pourrait, sous l'action combinée de la capillarité et de la chaleur descendre jusque dans les parties profondes du globe."

Always preferring, when possible, a reference to fact or experiment than to authority, I would advise Dr. Hunt, in order to form a conception of such strange action, to examine a common Gifford or other injector used to supply feed water to a high pressure boiler, and he will soon perceive how it is possible that the very forces which otherwise would prevent the entrance of the water into the boiler can become the very means of forcing it in.

Dr. Hunt next asks me to remember "that the oldest known series of rocks, the Laurentian, consists of quartzites, lime-stones, and gneiss, evidently of sedimentary origin, and derived from still older sedimentary rocks. When I was in Canada, what little I did see of the Laurentian rocks, did not at all prove to me that they had been derived from still older *sedimentary* rocks, but on the contrary, whilst believing that the Laurentian and quartzites were of metamorphic sedimentary origin, and that the lime-stones were of metamorphic organic origin, I inclined to the conclusion that the materials out of which they had been reconstructed had most probably been the debris of still older igneous rocks, a view which I have maintained since 1854, with regard to some of the analogous Norwegian rocks, which I understand Dr. Hunt claims as Laurentian.

To refresh my memory, however, I have read over the description of the mineral characters of these rocks contained in the report of the geological survey of Canada, pp. 24-29, but can find therein no evidence whatsoever to the contrary, and therefore without disputing the correctness of Dr. Hunt's assertions as to points where he ought at least to be at home, I would ask whether this statement is founded on facts or hypotheses.

Dr. Hunt then devotes a whole page to what appears to be an inquiry as to who first showed that water played a part in igneous action, a subject which may be of personal or historical interest, but which is quite unconnected with the questions at issue, for in the consideration of nature all geologists will persist, notwithstanding whatever Dr. Hunt opines to the contrary, in regarding igneous action as volcanic action, and volcanic action as igneous action, nor can they imagine for a moment that any person except one who never had seen a volcano in eruption could be blind to the evidence of his senses, and deny the co-operation of vapours and gases in volcanic action.

That the results of Mr. Scrope's admirable researches should have been discredited, ridiculed, and declared unchemical, should be a warning to chemists in future not to hazard such opinions without having studied them in the field as well as in the laboratory.

As Dr. Hunt now brings forward the question of the density of quartz, it may be as well to remind him that all arguments based upon such data must necessarily be invalidated by the fact that the specific gravity of quartz crystals out of true volcanic lavas is found to be 2·6, or the same as the quartz in granite, whilst Mr. Sorby's microscopical examination of the quartz found in recent lavas conclusively proves that it can have crystallised out of the molten mass, and not necessarily, as Dr. Hunt would have us infer, merely been entangled from the débris of originally sedimentary strata.

Having long occupied myself with the application of the microscope to geology, and having repeated many of Mr. Sorby's experiments relating to this subject, I do not even think it necessary to contradict Dr. Hunt when he accuses me of not understanding Mr. Sorby's views, being quite content with that gentleman having expressed himself most decidedly to the contrary. Whilst I now recommend Dr. Hunt to commence the study of microscopic geology, I can at the same time well imagine his being disconcerted when on opening the February number of the *Geological Magazine*, in which his own paper appeared, he at the same time found a few lines from Mr. Sorby quite sufficient to annihilate the deductions he had so elaborately arrived at from a study of that observer's memoirs with a view to make them serve his own purposes.

All the other points have already been considered in my paper in the Chemical News, and I would only remark with regard to Dr. Hunt's criticisms upon my chemical geology, that it is probable that some of them would not even have been brought forward by Dr. Hunt, had he waited until an outline of these views, now in the press, had appeared instead of selecting for attack disjointed fragments or sentences apart from their context; thus for example, in the case where he accuses me of being ignorant of the laws of diffusion: he would have found my opinion expressed as follows:—

"Whilst on the one hand the zones formed in the earth are considered to have possessed a somewhat stable or permanent character, those present in the atmosphere would on the contrary be the reverse, for no sooner had the gasiform products forming them, by in the first instance obeying the impulse of gravity, and so overcoming the counteracting tendency of the laws of the diffusion of gases, than these latter would assert themselves, and in process of time entirely obliterate this arrangement."

And again:—

"As before stated, this arrangement would gradually be obliterated by diffusion, but as the element of time is one of vital importance in considering the effects of diffusion, it is imagined that before being obliterated, this arrangement may still have had considerable influence in modifying the chemical reactions which took place at this period in the earth's history."†

Dr. Hunt, whose knowledge of the laws of diffusion does not seem to include any appreciation of the importance of the element of time in their consideration, might just as well maintain that a lump of sugar could not reach the bottom of a tumbler of water because sugar will dissolve in water.

As Dr. Hunt seems to have great respect for authorities on each subject, I will have great pleasure in submitting the question as whether my proposition is invalidated under these circumstances by the action of laws of diffusion to Mr. Graham, the great expounder of these laws, and abide by his verdict.

In the discussion of new views, more, however, is

† It must be remembered that these great bodies of gases and vapours are supposed to be the results of a general and simultaneous act of chemical combination *in situ*, and not to have been slowly gathered together from the realms of space.

required than mere quotations from old authorities, what is specially required are facts and experimental evidence; it must also be remembered that much depends upon the mode in which authorities are made use of in such discussions, since it is often an easy matter to select passages or disjointed fragments from the published works of authorities which may appear to support almost any view which may be taken of a subject under consideration.

Dr. Hunt, whose papers consist in greater part of a compilation of references to numerous authorities, from the time of Thomas à Kempis down to that of Sterry Hunt, seems to be quite aware of this fact, as an instance or two will testify.

Thus, when Dr. Hunt quotes Hopkins in support of his views as to the consolidation of the molten sphere, he takes good care not to inform his readers that Hopkins distinctly declares his opinion that the exterior was not the last to solidify, but would have consolidated and formed a crust before the interior had become entirely solid, a view which I have adopted on his authority, and which is diametrically opposed to Dr. Hunt's opinion, that—

> "The surface of the earth immediately previous to its entire solidification was "a liquid bath of no great depth surrounding the solid nucleus."

Again, although he finds it convenient to quote Forchammer in reference to some minor points quite beyond the limits of the present discussion, he seems to be quite unaware of the fact that the idea of the saline crust of chlorides, &c., which he ridicules my having adopted, was long before propounded by Forchammer, who first made the calculation that the quantity of chloride of sodium in such a crust would have been sufficient to have clothed the entire sphere with a coating of salt some 10 feet in thickness.

And yet, again, when he refers to Sorby's experiments as corroborating his views, as for example that quartz cannot be a volcanic product, *i.e.*, a product of igneous fusion in nature, his deductions are at once put to rout by the few lines from Sorby himself brought forward in my last communication to the Chemical News.

On the other hand, after a full consideration of the various memoirs of Hopkins, Forchammer, and Sorby, along with a careful repetition of many of their experiments, I have failed to discover any point inconsistent with the views I have advanced, and I am further enabled to find much evidence in their favour in the writings of Daubrée, Durocher, Bunsen, Phillips, and other eminent scientific men whose opinion Dr. Hunt evidently considers as quite beneath his consideration.

To prove that it is better to stay at home in one's laboratory than to travel wide and far in order to study nature's operations in the field, as is considered necessary to the geologist by Sir Charles Lyell and other eminent men, Dr. Hunt quotes, from Thomas à Kempis, "the wise saying passed into a proverb among churchmen"—that "those who make many pilgrimages rarely become saints."

In this we are quite of accord, since it is well known that a knowledge of the world acquired by travel is the best antidote to bigotry or one-sided opinions. What we require are geologists, not saints, and although it may be that in Canada geologists are esteemed in proportion to their saintly pretensions, experience on this side the Atlantic does not tend to prove that any of the natural sciences have been as yet much advanced by the labours of the would-be-saintly portion of the community.

As I have previously explained, I was induced to enter into this discussion (which I am still confident will do good to science by energetically ventilating some obscure points) by the special invitation conveyed in writing from Dr. Hunt "to have a friendly fight;" but I now find, if I may judge from the style of that gentleman's communications to the Chemical News and *Geological Magazine*, that his idea of scientific warfare consists in an attempt to overwhelm and crush his opponent with sneers and countless accusations of incompetency and ignorance :‡ ignorance of chemistry, of geology, of petrology, mineralogy, microscopy, literature of the subject, &c., &c., &c., whilst at the same time he does not fail to herald in his own views as what might be termed the quintessence of the combined "results of modern investigations in physics, chemistry, mathematics and astronomy." Would it not have been more prudent, as well as more becoming, to have left to our readers the task of forming their own judgment upon the evidence on both sides brought before them in the course of this discussion.

Having no pretensions, either to being a saint, nor like Dr. Hunt, to be versed in saintly lore, I cannot quote Thomas à Kempis, yet I can nevertheless follow his example and wind up with an old quotation; for even at the risk of appearing still more uncourteous, I really cannot resist the temptation to remind him of the old saying, passed into a proverb amongst laymen, that "curses, like chickens, come home to roost."

ON THE USE OF THE SPECTROSCOPE AND MICROSPECTROSCOPE IN THE DISCOVERY OF BLOOD STAINS AND DISSOLVED BLOOD, AND IN PATHOLOGICAL INQUIRIES.

By W. BIRD HERAPATH, M.D., F.R.S.

The discovery of and recognition of blood stains, and more especially of human blood, has been a problem which has long baffled the skill of the chemist and the more highly trained medico-legal eye of the microscopist, and any means by which our medical jurists can lessen the difficulties, and facilitate the inquiry, must be hailed as a boon by all scientific observers. Hitherto the chemical difficulties of the question have been the greater in the inverse proportion to the quantity of stain, and many minute and perfectly evident spots would even evade recognition by the test tube in consequence of the smallness of their size, or the disadvantages of their position. Whilst the microscope would also fail in their detection if from any peculiar circumstances the globules could not be safely and securely removed from the tissues which were under careful examination; independently of which the chemical and physical changes induced in the characters of the blood globules by the various menstrua employed in their removal, rendered recognition by micrometrical admeasurement a very doubtful and uncertain operation.

When blood globules are to be discovered floating in a saline fluid, such as urine, saliva, or the generality of mucous discharges, the microscope will readily detect their presence; and should the density of the fluid be very closely equal to the specific gravity of the serum of the blood, scarcely any change in their physical characters would occur, and it would then be possible to determine their exact form and size, and render their probable source a question of easy solution; but when the blood has been dried and long exposed to the air, it is no longer easy to reproduce the blood globules in their pristine form and optical characters, as the various media employed to

‡ Dr. Hunt does not content himself with mere accusations of ignorance, for when disputing my statement that the reactions of the compounds of magnesia with carbonic acid in an artificially compressed atmosphere of that acid, had long been employed in manufactures,—he uses the words, "here it becomes difficult to admit the plea of ignorance which suggests itself for most of Mr. Forbes's errors and mis-statements." I may merely add, that since the appearance of Dr. Hunt's paper in the Chemical News of Jan. 17th, I have received various communications from chemists and others connected or acquainted with this manufacture, not only offering to supply facts in full corroboration of the truth of my assertion, but also directing my attention to a long expired patent (No. 9102. 1841) of the late Mr. Pattinson, of Newcastle, in which these very reactions are distinctly embodied.

dissolve the clots act on the globules with more or less celerity. It is usual to employ either solutions of cane or grape sugar, or mixtures of glycerine and distilled water having a density of 1·030°. Some observers have employed saline solutions for this purpose, others have used a strong solution of arsenious acid in distilled water; the objects which each have in view being the removal of the blood discs, and the non-alteration of their physical characters. If the action of the solvent or medium, from its deficient density or peculiar chemical properties, has resulted in a destruction of the blood globules and a solution of the colouring matter, the *microscope* would no longer either recognise its mammalian character or even assure us of the presence of blood.

In this condition of the enquiry the chemist alone, by the employment of tests, could decide the question of the presence of dissolved hæmatine, and would chiefly rely on the action of heat in coagulating the albumen and destruction of the colour; whilst upon another portion of the fluid he would assure himself that ammonia would not produce any great change of colour, thus deciding the non-vegetable character of the colouring matter. But during the past two or three years an addition has been made to our optical instruments in the aid which we have obtained in the recognition of various substances by the effects which they have on the absorption of different portions of the spectrum, and we have various forms of spectroscope according to the purposes for which they were intended.

The first invented spectroscope was a very efficient but cumbrous instrument, and astonished the world by the discovery of four new metals in consequence of the remarkable peculiarities of their coloured flames, and thallium, cæsium, rubidium, and indium have been thus isolated from other bodies, and added to the list of elementary bodies. Shortly afterwards Professor Stokes introduced a modification of this instrument, which enabled liquids and coloured fluids to be submitted to the same mode of testing them by their absorptive spectra, and on the table is one which I have long employed for this purpose, a more powerful and efficient instrument than that recommended by him for these experiments. In this instrument, essentially a direct vision or Hofmann's spectroscope, the liquid to be examined is placed in a small test-tube, and that is held in a clipped spring, which supports it during the examination, whilst a bright light is transmitted through the liquid previous to its analysis in the spectroscope,—the spectrum showing various bands of absorption in well-marked optical liquids, some of the most beautiful of which are certainly weak solutions of permanganate of potassa, and dilute solutions of cruorine and hæmatine. In the first case five dark bands are seen in the green part of the spectrum, and in that of blood two sharply defined black bands are seen, one in the green, and another on the border of the orange ray. The intensities and positions of these bands vary according to the age of the blood stain, and result from the alteration in the colouring matter of the blood, from the effects of drying, and from exposure to air.

The stains when old have a much less decided or evident absorption, the bands are weaker, and an additional band of a diffuse character is found in the red ray. But it does not appear to be possible to form any positive, or accurate, opinion on the age of blood; from various observations it has been ascertained that these changes take place with more rapidity under some circumstances than in other apparently similar cases.

In all optical experiments on blood, it is necessary to use *excessively* dilute solutions of the colouring matter; otherwise the fluid is absolutely opaque to light—or if it transmit any light at all, nothing but the extreme red rays are observed. When still more dilute, the blue end of the spectrum is quite absorbed. and so are two bands in the green, and occasionally, also, one in the red. These optical properties of blood were first pointed out by Hoppe (*Virchow's Arch.*, 1862, vol. xxiii. p. 446), and subsequently by Professor Stokes (*Proceedings of the Royal Society* 1864), and Mr. Sorby (*Quarterly Journal of Science*, 1865.)

Professor Stokes has investigated the effects of different chemical reagents upon the colouring matter of blood (*Proceedings* of the Royal Society, 1864, p. 355, *et seq.*) and he has arrived at a very intelligible solution of the various phenomena exhibited by this remarkable substance, and has come to the conclusion that arterial and venous blood in the recent fluid condition contains a substance called cruorine, which like indigo is capable of existing in two different states of oxidation and colour. That in arterial blood, scarlet cruorine is the form in which it is found, and in this condition very dilute solutions produce two very sharply defined black bands of absorption, one close to and parallel with the sodium line D, and is the more intense of the two; whilst the second is found in the green ray about the breadth of the previous band distant from it.

On submitting this scarlet cruorine to deoxidising agents to a moderate extent, the cruorine becomes the deoxidised or purple cruorine, and then the solution is light or deep purple according to its degree of concentration.

Examined by the spectroscope in this condition, only one deep broad band of absorption is found, which commences about the solar or sodium line D, and then passes onwards to the green, absorbing the whole of the yellow and part of the green band of the spectrum. It is remarkable that upon shaking up a dilute solution of scarlet cruorine with an atmosphere of carbonic acid the fluid does not exhibit the appearance, or spectrum of venous blood. It is evident therefore that the blue colour of venous blood is not produced by the presence of carbonic acid in solution, but to a *reducing action* in the capillaries analogous to that of other reagents, whilst the influence of sulphide of ammonia, sulphuretted hydrogen, or the hydrated protoxide of iron, or the protochloride of tin, or the peculiar deoxidation of arterial blood due to the changes going on in the systemic circulation, are all instances in which such a deoxidation or reduction has been in action. A dilute solution of scarlet cruorine set aside in a full closely corked phial, or with very little air, will shortly pass by spontaneous deoxidation to the purple cruorine, and will then exhibit all the optical phenomena of this peculiar substance, but resumes its scarlet colour by agitation with air. Professor Stokes says that of all reducing agents, an ammoniacal solution of protochloride of tin, (previously treated with sufficient tartaric acid to prevent the precipitation of oxide of tin), is the most efficient reducing agent, and as it is colourless it does not interfere with the spectroscopic appearances. He says that when a few drops of this solution are added to a solution of scarlet cruorine, the latter is presently reduced and we have the spectrum of purple cruorine. If the solution be now shaken up with air, the cruorine is reoxidised to the scarlet form. On standing a few minutes it again becomes reduced, and the solution may be made to go through these changes repeatedly until all the tin has passed to that of complete oxidation.

But when blood or scarlet cruorine is treated with an *excess* of deoxidising material, or has become changed by long exposure to air or by drying, or by the effect of sulphurous and some other acids, the colouring matter becomes brown and more insoluble in water. It is in fact then changed into *brown hæmatine*, which has very different optical properties from that of either scarlet or purple cruorine. The two dark bands of absorption are still found, but have become very faint, and much less sharp in outline, whilst a third dark band is seen in the red ray; of course, on the less refrangible side of the solar or sodium line D. This change, when produced by age and exposure, is sometimes months in being fully accomplished, and is only well marked on the appearance of the dark band in the red ray.

Now just as cruorine can exist in two different forms, so can hæmatine; and we have either the brown or the red

hæmatine, the latter being produced by deoxidising the brown hæmatine by some reducing agents, as hydrated protoxide of iron. Then two bands of absorption occur, as in scarlet cruorine, but capable of being readily distinguished from those by their position and different degrees of intensity.

(To be continued.)

METHOD FOR THE

DETERMINATION OF SILICON IN IRON AND STEEL.*

By V. EGGERTZ,
Professor at the School of Mines, Fahlein, Sweden.

(Concluded from p. 101.)

The oxide of iron is easily dissolved in the heat of a water-bath. The silica is again thrown on a filter, washed, dried, ignited, and weighed. 0·016 gramme of silica answers to each 0·001 gramme of silica when 3 grammes of iron have been used in the analysis. To ensure the purity of the silica, it may be mixed in a platinum crucible with ten times its weight of pure fluoride of ammonium, diluted with water to the thickness of syrup. The water must be evaporated on a water bath, and the crucible heated, with a cover on it, by a gradually increasing heat over a spirit-lamp to a full red. If nothing is left in the crucible, the silica was pure, and has passed off as silicon fluoride; but, if any thing remains, the operation with fluoride of ammonium must be repeated until a constant weight is obtained. When iron contains tungsten, for instance, some tungstic acid is formed, and this accompanies the silica for the most part, being dissolved by the soda solution, but not volatilised by the use of fluoride of ammonium. Vanadic acid also accompanies the silica, behaving as tungstic acid. Instead of using fluoride of ammonium, it is preferable to use hydrofluoric acid, with which the silica is moistened, and the evaporation is conducted on a water-bath. 0·1 gramme of pure silica obtained from analysis is easily dissolved by 2 c.c. of hydrofluoric acid (of the strength that 2 c.c. of it are neutralised by 1·5 c.c. of a saturated solution of ammonia 0·95 sp. gr.). When using hydrofluoric acid, getting it on the hands or exposure to the evaporating gas must be carefully avoided. The mass left on the filter from the soda solution may be composed of—besides graphite—slag, oxide of iron, oxide of titanium, &c. (but not copper, at least when the iron does not contain more than 1 per cent); this is dried, ignited, and weighed. The method of separating oxide of iron and slag, when the iron or steel contains both these, is not yet known. If the composition of slag were always alike (which it is not), it would be easy to calculate its amount from either the silica or oxide of iron obtained in the analysis. In a piece of Bessemer iron very red short (that is, it could not be bent at a white or yellow heat without being broken), which contained no sulphur, by several experiments 0·3 per cent of oxide of iron has been obtained, and only traces of silicon. After ignition, the oxide of iron may possibly be found as sesquioxide. The amount of oxygen, in case that the red shortness is due to this, as it probably is, amounts to less than 0·1 per cent.

When the iron or steel for analysis contains titanium, a part of this substance follows the slag in the form of titanic acid. If such is the case, this must be melted with ten times its weight of acid sulphate of potash, by which it is dissolved; the mass is dissolved in cold water, and the solution precipitated by boiling; the weight is determined, and subtracted from that of the slag. This ingredient has not, however, been found in bar iron or steel in such a quantity as to merit special attention.

Regarding the determination of silicon in cast steel where only a trace of slag is found, the method given below for cast iron may be employed; but 3 grammes at least ought to be taken for each experiment, and the acids for solution in proportion.

In experiments conducted at the Mining Institution for the determination of silicon and slag in bar iron and steel, the amount of silicon has generally varied between 0·01 per cent and 0·1 per cent; but in two sorts of good cast steel from Krupp's it has amounted to about 0·3 per cent. Slag in cast steel has been found only in traces, but in another case it amounted to 0·2 per cent; in good iron wire, prepared from bar iron, converted in a refinery hearth, from charcoal pig iron, 0·33 per cent; in puddled iron (armour plate), from 0·75 to 3 per cent; and in an English iron rail, to 4 or 5 per cent.

For the determination of silicon in cast iron, in which no finery slag is found, and only exceptionally blast-furnace slag, the following method has proved suitable; 2 grammes of iron, which has passed a sieve with holes of a diameter of $\frac{1}{60}$ in. at the most, is put into a beaker of 100 c.c. capacity, containing 30 c.c. of hydrochloric acid, sp. gr. 1·12. The beaker is covered with a close-fitting watch-glass, heated without delay, and the liquid kept at a gentle boiling for half an hour.

All the carbon chemically combined with the iron is separated from the liquid in the form of an ill-smelling hydrocarbon gas, by which operation a disadvantageous formation of humus and oxide of iron is prevented (in case that the solution should be required for further researches).

If the carbon formed in the solution is left in contact with the air some minutes before the boiling is begun, it undergoes such a change that it cannot afterwards be decomposed into gas and evaporated; if necessary, some hydrochloric acid is added, and the solution evaporated on a water bath, until the smell from hydrocarbon gas has ceased. If the graphite in the iron is to be determined at the same time, it is placed, as well as the silica, on a filter previously dried at 95° or 100°, and weighed; well washed with hot water containing 5 per cent nitric acid, again dried, weighed, and ignited in a porcelain crucible. By deduction of the weight of silica and of the filter ash, the amount of graphite is determined. (It must be observed that the silica dried on a filter contains 6 per cent water, or only 94 per cent silica.) If, for instance, the filter weighs 0·125 gramme, and its ash 0·001 (the combustible substance being 0·124 gramme) and the filter + graphite + silica 0·182 gramme, and the residue after ignition 0·025 gramme (0·001 gramme of this is filter ash), the weight of the silica (0·024 gramme) is, after the drying, 0·0255 gramme, and thus the weight of the graphite becomes $0{\cdot}182 - (0{\cdot}124 + 0{\cdot}001 + 0{\cdot}0255) = 0{\cdot}0315$ gramme. To the solution, after the separation of graphite and insoluble silica, is added 4 c.c. nitric acid, 1·2 sp. gr.. and evaporated to dryness. The further proceedings are the same as previously described for the determination of silicon. If it is intended to determine only the silica, the whole solution is evaporated to dryness immediately after boiling. When the silica is red, strong hydrochloric acid is added, as previously described. If the silica is contaminated with titanic acid, vanadic acid, or tungstic acid, it is operated upon with fluoride of ammonium or hydrofluoric acid, as previously mentioned, whereby the silica is evaporated and calculated by loss. By the above method of dissolving iron in hydrochloric acid, the silicon changes, without evaporation, for the most part, to insoluble silica, which may be filtered and determined. Sometimes a very unimportant part is dissolved, especially if the boiling has been short.

When iron is dissolved in hydrochloric acid without heating (white cast iron is very difficult to dissolve in this way), a still less portion is dissolved, and generally so little that it may be neglected for practical purposes.

* From *Engineering*, July 24, 1868. Translated by C. P. Sandberg.

The washing is performed with hot water containing nitric acid, as previously described.

When the iron is dissolved in nitric acid, a great deal of silica enters into solution.

The different sorts of cast iron appear to be slightly different in this respect. In dissolving cast iron with heat, in very diluted sulphuric acid, a great deal of silica is dissolved, but very little when the water is the least possible; as the water evaporates, the silica settles and becomes insoluble. The method given below rests upon these circumstances, and has proved very satisfactory, and by this the taking away of the acid is avoided, which is both necessary and troublesome when using hydrochloric acid with heat. The amount of silicon has, according to both methods, turned out alike. Traces of silica are always found left in the solution and wash-water. Regarding the determination of silicon in iron, it should be observed that only such vessels may be employed as are unacted upon by the reagents used in the analysis, as otherwise an undue proportion of silicon may be obtained.

Two grammes of cast iron which have passed a sieve of 0·2 of a line, are shaken by small portions at a time into a beaker of 100 c.c. capacity. In this beaker has been previously put 18 c.c. of water with 3 c.c. pure sulphuric acid of 1·83, or 15 c.c. sulphuric acid of 1·23 p. gr. with 6 c.c. of water.

The beaker is covered with a watch-glass, and placed on a water-bath; if the graphite rises on the sides of the beaker, it is pushed down into the liquid by a glass rod. When the iron is dissolved, the watch-glass is changed, after being washed, for a paper cover, and the solution evaporated on a water-bath until no condensation occurs on a watch-glass held over the beaker; 30 c.c. of water are then added, and it is frequently stirred with a glass rod, whilst on the water-bath, until the white iron salt has completely dissolved. The insoluble mass is then thrown on a filter, washed with hot water containing 5 per cent nitric acid, 1·2 sp. gr. (in order to dissolve all compounds of iron) as long as an iron reaction is given with ferrocyanide of potassium. The filter, with its contents, is placed in a carefully tared porcelain crucible; it is then cautiously dried, ignited, and weighed. The silica contains 48 per cent of silicon, and its purity is examined by the method previously mentioned, when such is considered necessary. If the cast iron contains vanadium, this is obtained for the most part as a yellow-brown vanadic acid with the silica, from which it may be extracted by warm hydrochloric acid or ammonia.

When intending to determine at the same time the amount of graphite in the cast iron, the solution is treated, after the separation of the chemically combined carbon, by boiling, as previously described when dissolving the iron in hydrochloric acid. In determination of graphite the use of hydrochloric acid is preferable.

The greatest amount of silicon which has been found here in grey charcoal pig iron was 2·7 per cent., and in white (spiegeleisen) 0·8 per cent. The amount of silicon in pig from coke blast-furnaces is rarely more than 4 per cent. The least quantity of silicon in grey cast iron has been 0·2 per cent, and in white (spiegeleisen) it has not been less than 0·01 per cent. The amount has usually been from 1 to 2 per cent in cast iron suitable for the Bessemer process, about 1 per cent in good Franche Comté, and in pig iron for puddling about 0·5 per cent.

From many iron works have been obtained pig iron suitable for refining on the charcoal hearth, which contained about 0·2 per cent silicon; but from others a greater amount of silicon has been found in the same sort of pig iron, and it is generally presumed that different quantities of silicon require a different construction of the furnace, and a different method of working the refinery. It has been clearly proved by numerous experiments what a great influence the amount of silicon has upon the nature of cast iron, in being more or less easily refined, &c., and at the same time the great importance of paying more attention to its manufacture than has hitherto been done, in order to obtain cast iron, which, with regard to its silicon, may be suitable to the purposes for which it is to be employed.

The amount of silicon in iron of different degrees of hardness from the same charge of the blast furnace ought to be pretty well valued by the fracture, after some determinations have been made by analysis.

ON

HEAT AND COLD;

A COURSE OF

SIX LECTURES*

(*Adapted to a Juvenile auditory*),

DELIVERED AT

THE ROYAL INSTITUTION OF GREAT BRITAIN,

(*CHRISTMAS*, 1867—8),

BY

JOHN TYNDALL, Esq., LL.D., F.R.S.

Lecture VI.

(Concluded from page 105.)

Reflection, refraction, and absorption of radiant heat.—The heat of the sun.—Visible and invisible rays.—Extraction of light from the rays of heat.

I have had occasion to say to you once or twice in these lectures that no body in nature is absolutely cold. All bodies are more or less hot. Even ice itself is a hot body compared with solid carbonic acid. In fact, ice would be quite competent to make a mixture of solid carbonic acid and ether boil, it being hot in comparison. All bodies are warm, and all bodies are emitting rays of heat. Here is a platinum wire in front of the table, such as we have already operated upon. At the present time that platinum wire is emitting rays of heat of a perfectly definite character. If I connect this wire with our battery you will observe our old experiment. You see the wire is heated to redness; it emits rays of heat, and also to some extent rays of light. Before the electric current passes the wire emits rays of heat which are incompetent to excite vision; but when I raise the temperature of the wire thus, by sending the electric current through it, what becomes of its old rays of heat which it emitted in this invisible state? They still maintain themselves, and they become much stronger, but they are still obscure. We mix, with the luminous rays of that wire, the obscure radiation that issued from it before the current made it incandescent. If I go on shortening the wire, as in an experiment we made in an early portion of these lectures, we find it gets brighter and brighter, but the rays it emitted before it became red hot at all are still mingled with the visible radiation. They exist, but they exist greatly intensified; so that the rays which issued from that wire before it became incandescent, are present, as well as the visible rays, but they are raised to a thousand times the intensity which they first possessed. They are still obscure, and have no power to excite vision, but they are, nevertheless, there with a thousand-fold their first intensity. Now I must try to separate before you these luminous rays from the obscure rays; and I must endeavour to operate upon the obscure rays so as to show you some effects that they can produce. I think you will understand the process by which this can be done. I have here a small concave mirror, and this I will place behind the electric lamp. We shall have an image of the carbon points of the lamp produced in that

way, and I will throw that image upon the screen. We have now thrown upon the screen an image of the carbon points, whence issues the electric light. If I take another mirror, and converge the rays by it, I can give you a larger image, which, perhaps, will be better seen. Here is now a larger image of the carbon points produced in that way. The image is inverted. You see a considerable amount of light there, but Mr. Cottrell will now fill a vessel with an opaque liquid. The liquid which we use to obtain the opaque solution is called bisulphide of carbon: it is perfectly transparent; and here is the substance called iodine—very well known to many people. This bisulphide of carbon dissolves the iodine with great freedom, and the consequence is the production of this dark liquid, which is so wonderfully opaque that it would cut off the light of the sun at noon-day. Strange to say, it is the quality and property of this wonderful substance to entirely cut away the luminous or visible rays upon which depend the colours you saw on the screen, whereas it allows all the rays of heat to pass through. The liquid is opaque to light, but perfectly transparent to radiant heat. Mr. Chapman will place a lens in front of the electric lamp; and thus we obtain this beautiful convergent beam or cone of light tracking its way through the dust of the room towards the thermo-electric pile. Mr. Chapman will, when I tell him, place the cell containing this opaque liquid in front of the electric light. That will cut off bodily all the light, but still the spot where the pile will be placed will remain very hot. [The cell and pile were then placed in position.] You see that all the light is cut away; but you observe that the needle at once marches away, thus proving that although the light is cut off, the heat rays are left behind.

I want now to try and make these heat rays more evident to you still, and for that purpose I have placed within this camera an electric lamp similar to what I have just used; and behind the electric lamp I have placed a silvered mirror. This mirror will reflect the rays of light from the electric lamp, and will cause them to issue through this window which you see in front. This window is formed of rock salt. Rock salt is exceedingly transparent to the rays of heat, and also to the rays of light; and it is for that reason that I use that substance. I now obtain a convergent beam from the electric lamp. You see a brilliant cone of rays. Mr. Cottrell will now place the opaque solution in front. There it is, cutting off all the light, so that you see nothing. But now I bring this piece of platinum opposite the dark liquid; and observe what occurs. The platinum is raised to a red heat, in perfectly dark air. If, instead of platinum, I take some dry paper, and hold it in the focus of the dark rays, you see I can ignite that paper. The paper is set on fire. This ignition is caused by the invisible rays of heat issuing from the electric lamp. I now take a thick piece of metal, and hold it in the dark rays of heat: you see it is melted by the radiant heat, and drops down in a liquid state. I will now burn a piece of zinc here. There, you see the zinc is actually set on fire in a place where there was perfect darkness. The air where this zinc is set on fire is perfectly unwarmed. Nothing would be easier than to ignite a cigar in this way in perfect darkness. For instance, here is one which I will ignite. You see it is instantly set alight in a place where there is absolutely no light. You might put your eye where that platinum was raised to red heat. I have cautiously approached my eye to that burning focus that you saw there, and allowed the rays bodily to enter the eye, and could neither see light nor feel heat. The retina was perfectly dead to those very powerful rays. Sometimes we obtain the combustion of magnesium by these rays. Here you see we have that beautiful metal set on fire in a place where there were no light whatever—a space of utter darkness. I might set London on fire by means of these dark rays. I have here a glass jar containing oxygen gas, and into this jar I dip a piece of charcoal. I now bring the charcoal into the focus of the invisible rays of heat, and you see the charcoal is ignited by these dark rays, and burns brilliantly in this gas.

I want now to make one or two more experiments in connection with this subject. For this purpose I will take the same mirror which I have just used, and employ another camera which is at the end of the table. The mirror will be placed behind the light, and will reflect a beam of light along the table. Instead of allowing this beam to fall upon the audience and annoying you, I will catch it upon another mirror just as I caught the ray of light by the mirror near the ceiling in an experiment early in the lecture. I daresay many of you see the intense reflection here. There is a focus which would burn your fingers most fearfully if you put them there. I dare say we shall be able to inflame paper at that focus. There you see the paper instantly set in a blaze; and this blaze is produced not by the luminous rays, but by the dark ones. You might put a sensitive thermometer there, and have no result. It is only when the heat falls upon this paper that the heat is produced. We can burn zinc here as I did in the dark rays. You see the zinc is set on fire, and blazes up almost like a piece of paper. Here is a small vessel containing water, and I will place that in the focus of the rays. I now place another vessel of water in such a way that the light has to pass through it. This will intercept the dark rays which give the heat, though it does not sensibly interrupt the rays of light. At the present time the focus of rays falls upon the former vessel of water, without any effect whatever being produced upon it. I will now withdraw the vessel of water through which the beam passes before it reaches the mirror, and so allow the heat rays to pass, and you see the water in the vessel at the focus of the rays immediately begins to boil. After a time this water will be thrown into a state of violent ebullition. It is already boiling. This action is due not to the rays of light, but entirely to the dark invisible rays of heat of which I have been speaking.

I make these experiments for the purpose of bringing home to your minds the fact that we owe all our rivers, all our glaciers, and all our snow, entirely, or almost entirely, to these dark rays. The luminous or bright rays of the sun fall upon the tropical ocean, and pierce it to great depths: they are not absorbed; but the non-luminous rays—the heat rays of the sun—strike upon the tropical ocean, and they are absorbed very near its surface. It was by the absorption of the dark rays that the water was boiled in the last experiment. These dark rays of the sun which strike upon the tropical ocean, and are then absorbed, heat the surface of the ocean, and thus it is that all the moisture or evaporation is produced.

And now, I am sorry to say, we have come to the end of our task. I told you in the beginning that I wished very much to transfer the task of giving these lectures to somebody else, as I was so occupied that I could not make them what I wished to make them, and still I am not sorry that I undertook them. I am glad that I have come here, for it has given me great pleasure to meet you from day to day. You have made up by your attention for my defects in lecturing; and I have only to add that I thank you for that attention, and wish you from my heart a happy new year.

Preparation of Bromides.—A. Faust describes the following method for the preparation of bromides: Bromic sulphide is first prepared by mixing together 2 parts of sulphur (flower) and 24 of bromine; this is added to calcic hydrate, suspended in water, when the following reaction takes place:

$$2\,S\,Br_6 + 8\,Ca\,O = 6\,Ca\,Br_2 + 2\,Ca\,S\,O_4$$

The filtrate is saturated with carbonic anhydride, concentrated, and mixed with twice its bulk of alcohol. After a few days the solution containing pure calcic bromide is filtered off from the calcic sulphate, and evaporated. From this salt any other bromide may be obtained by mutual decomposition.—(*Arch. Pharm.*, clxxxi., 216.)

PROCEEDINGS OF SOCIETIES.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, January 21st, 1868.

Edward Schunck, Ph.D., F.R.S., &c., President, in the Chair.

"*Some Remarks on Crystals containing Fluid*," by J. B. Dancer, F.R.A.S.

The author gave a brief history of the discovery of fluids in crystals, including Sir H. Davy's chemical experiments on the fluids and gases obtained from the cavities in quartz crystals; Sir David Brewster's discovery of the pressure cavities in the diamond, ruby, emerald, amethyst, chrysoberyl, &c.; the existence of minute crystals in these cavities and the two new and remarkable fluids, which are immiscible, but sometimes found together in the same cavity—one a liquid hydrocarbon, named Brewstoline, the other Cryptoline; his experiments and examination of artificial crystals deposited from aqueous solutions; his examination of the Koh-i-noor diamond and others in the East India Company's museum; and the geological speculations to which these discoveries gave rise. Mr. Dancer mentioned the experiments of his late father and others in producing artificial gems by intense heat, and stated that his own attention was drawn to this subject some twenty-four years since, by Sir David Brewster presenting him with a specimen of topaz containing fluid. Since that time he had examined a large number of crystals, of various kinds, from the collections of friends; and had found fluid in quartz from South America, Norway, the Alps, Ireland, Snowdon, and the Isle of Man; and in fluor spar from Derbyshire; this latter specimen contained a considerable quantity of fluid, which burst the crystal at 180° temperature.* He suggested the employment of the microscope as a valuable assistance in detecting spurious from real gems; very few of the latter are perfect, and the flaws and cavities are so distinct in character from those which are so abundant generally in artificial gems that very little experience is sufficient for the purpose. This mode of testing of course is limited to transparent crystals, but might be employed when the usual methods are not practicable. He also mentioned Mr. Sorby's (F.R.S.) discovery of fluid cavities in the quartz of granite, in the quartz of volcanic rocks, and also in the feldspar ejected from the crater of Vesuvius, and Mr. Sorby's method of determining the temperature at which various rocks and minerals are formed. At the conclusion of the meeting, crystals containing fluid were exhibited under the microscope, and the expansion of the fluid by elevating the temperature of the crystal whilst under examination.

Ordinary Meeting, February 4th, 1868.

Edward Schunck, Ph.D., F.R.S., &c., President, in the Chair.

Among the donations announced were several bottles of chemical products for the Society's collection, from Dr. F. Crace Calvert, F.R.S., &c.

The thanks of the Society were voted to Dr. Calvert for his valuable donation.

"*On Some Constituents of Cotton Fibre*," by E. Schunck, Ph.D., F.R.S., &c., President.

It is generally supposed that cotton, when quite pure, consists entirely of woody fibre or cellulose, and that its composition is consequently represented by the formula $C_{12}H_{10}O_{10}$. It is certain, however, that in the raw state, as furnished by commerce, it contains a number of other ingredients, some of which occur so constantly that they may be considered essential constituents of cotton, viewed as a vegetable product. The object of the bleaching process to which most cotton fabrics are subjected is to deprive the fibre of these other ingredients and leave the cellulose behind in a state of purity. Notwithstanding the importance of an accurate knowledge of everything relating to cotton from an industrial point of view, the substances contained in it along with cellulose have never been subjected to a special chemical examination, and very little is consequently known about them. Persoz, in his *Traité de l'Impression des Tissus*, says that the woody fibre constituting the tissues of cotton, hemp, linen, &c., is not pure; it contains—1st, a certain quantity of colouring matter, which is more or less shielded from the action of decolourising agents by the bodies which accompany it, naturally or accidentally; 2ndly, a peculiar resin, natural to the fibre, insoluble in water and soluble with difficulty in alkalies, which plays the part of a reserve and protects the colouring matters inherent in the fibre from the action of the agents which ought to destroy and remove them; 3rdly, a certain quantity of fatty matter, of which a very small portion is peculiar to the fibre, the greatest part being derived from the operations of spinning and weaving; 4thly, a neutral substance, either flour, starch, or glue, which has been introduced by the weaver in sizing his warp; 5thly, inorganic saline matters, some of which belong to the fibre, while the others are derived from the water and the matters employed in the dressing of the warp. In the excellent article on Bleaching in the new edition of Ure's *Dictionary of Arts* there is a full account of these and other impurities of cotton fabrics, comprising all that was known at the time when the author commenced his examination.

The object which the author had in view in undertaking his investigation was to endeavour to throw a little more light on the nature of those substances which are contained in or attached to the framework of cellulose, of which cotton fibre mainly consists, and which are together with the latter produced by the plant. All foreign and extraneous matter introduced during the process of manufacture was therefore left entirely out of consideration. The author has further confined his attention to those constituents of the fibre which are insoluble in water but soluble in alkaline lye, and are afterwards precipitated by acid from the alkaline solution. Whether cotton contains naturally any substance soluble in water, or which being originally insoluble is rendered soluble therein by the prolonged action of alkalies, is a question on which the author pronounces no decided opinion.

For the purpose of obtaining the substances which he proposed to examine the author employed cotton yarn, which he preferred to unspun cotton for several reasons, the principal being that yarn is comparatively free from mechanical impurities, such as fragments of seed-vessels, &c., while on the other hand, if proper care be taken, no impurity is added to those previously existing during the process of spinning. The yarn was boiled in an ordinary bleacher's kier for several hours with a dilute solution of soda ash. The resulting dark brown liquor, after the yarn had been taken out, drained, and slightly washed, was removed from the kier into appropriate vessels, and mixed with an excess of sulphuric acid, which produced a copious, light brown, flocculent precipitate, while the liquid became colourless. This precipitate was allowed to settle, the liquid was poured off, and after being washed with cold water to remove the sulphate of soda and excess of acid it was put on calico strainers and allowed to drain. A thick pulp was thus obtained, which when dried assumed the appearance of a brown, brittle, horn-like substance, translucent at the edges. In one experiment 450 lbs. of yarn, made from East Indian cotton, of the variety called "Dhollerah," yielded 0·33 per cent of the dried precipitate. In another experiment made with

* After this Paper was written, Sir David Brewster informed the author that the fluid contained in crystals of fluor spar was water, and that the cavities burst at a temperature of 150°.

500 lbs. of yarn, spun from American cotton, of the kind called in commerce "middling Orleans," 0·48 per cent was obtained. The total loss sustained by yarn during the bleaching process amounts to about 5 per cent of its weight. Only a small portion of the matter lost is therefore recovered by precipitation of the alkaline extract with acid.

This precipitate formed more especially the subject of the author's investigation. It was found to consist almost entirely of organic substances, and of these the following were distinctly recognised :—

1. A species of vegetable wax.
2. A fatty acid.
3, 4. Colouring matters.
5. Pectic acid.
6. A trace of albuminous matter.

The author described the method employed by him for separating these substances from one another and obtaining them in a state of purity ; and he then gave an account of their properties and composition.

The waxy matter is by far the most interesting of these substances. It is insoluble in water, but soluble in alcohol and ether. If a concentrated solution in boiling alcohol be allowed to cool, the greatest part is deposited, causing the liquid to assume the appearance of a thick white jelly, consisting of microscopic needles or scales. When this jelly is filtered off and dried it shrinks very much, and is converted into a coherent cake, which has a waxy lustre and is translucent, friable, and lighter than water. Its melting point is between 83° and 84° C. At a higher temperature it is volatilised. When heated on platinum it burns with a very bright flame. The author thinks it probable that this substance covers the cotton fibres with a thin waxy film, and thus imparts to them their well-known property of resisting water. In its properties and composition it approaches very nearly the better known vegetable waxes, such as that obtained by Avequin from the leaves of the sugar cane, and that which is found on the leaves of the Carnauba palm. The author thinks that the name *cotton wax* is sufficient to distinguish it from these and other nearly allied bodies.

The fatty acid has the properties and composition of margaric acid. It is white and crystalline, fuses at 53° C., and gives with alkalies compounds soluble in water which are true soaps. It is, however, probably not a natural constituent of cotton fibre, but rather an impurity derived from the oil of the seed which escapes and diffuses itself among the cotton before or during the process of ginning. It might also have had its source in the oil and fat, used for greasing the cotton spinning machinery, since the author employed yarn in all his experiments. Persons practically conversant with cotton spinning affirm, however, that if ordinary care be taken, it is impossible that the cotton can become contaminated with anything of a fatty nature, during its conversion into yarn.

The colouring matters obtained in these experiments are without doubt the substances to which raw cotton owes its yellowish or brownish colour. The author was able to distinguish two bodies of a dark brown colour, which occurred in all kinds of cotton examined by him. Of these one is easily soluble in cold alcohol, and is left, on evaporation of the solution, as a dark brown, shining, brittle, amorphous resin, which is transparent in thin layers. In boiling water it softens and melts to a pasty mass, which becomes hard and brittle again on cooling. When heated on platinum foil it burns with a bright flame, leaving a very voluminous coal. It is nearly insoluble in ether. It dissolves easily in concentrated sulphuric acid and glacial acetic acid, with a brown colour. It also dissolves with ease in caustic and carbonated alkalies, giving dark, yellowish-brown solutions, from which it is re-precipitated by acids in light brown flocks. The other colouring matter resembles this in most of its properties. It is, however, much less soluble in alcohol. Cold alcohol, indeed, dissolves only a trace, but in boiling alcohol it dissolves with tolerable facility, being re-deposited, on the solution cooling, in the form of a brown powder. This powder, when filtered off and dried, forms coherent masses of a colour varying from light to dark brown, which are easily broken, showing a dull earthy fracture. Both colouring matters contain nitrogen, and they differ therefore in constitution from true resins, which they resemble in many of their properties. The peculiar colour of the so-called "Nankin cotton" is probably due to a great excess of these colouring matters existing in the fibre. It is certainly not caused by oxide of iron.

The purification of the pectic acid contained in the brown precipitate produced by sulphuric acid was not effected without difficulty. The best method, according to the author, consists in submitting it to a simple process of bleaching with chloride of lime, by which means the impurity, consisting of brown colouring matter, which adheres to it with great pertinacity, is destroyed. When pure it has the properties and composition ascribed to pectic acid by Fremy. The cotton itself probably contains pectose or pectine, which is converted into pectic acid by the action of the alkaline lye. About three-fifths of the brown precipitate consists of pectic acid. Of the remaining two-fifths the colouring matters constitute by far the largest part, the wax and fatty acid being present in very minute quantities. The albuminous matter was not isolated, but its presence was indicated by the formation of a small quantity of leucine, which took place when the brown precipitate was submitted to the action of hydrate of soda. A large quantity of oxalic acid was formed at the same time, no doubt from the pectic acid.

In conclusion, the author makes some remarks in regard to the part which these bodies may be supposed to play during the process of manufacturing gun-cotton. It has been asserted that the instability occasionally observed in gun-cotton is to be attributed to the impurities in the raw fibre, forming, by the action of nitro-sulphuric acid, bodies which decompose spontaneously at the ordinary or a slightly elevated temperature. The author's experiments do not support this view, since the substances described by him, when submitted to the action of the mixed nitric and sulphuric acid of the strength employed for making gun-cotton, do not yield explosive compounds.

FOREIGN SCIENCE.

PARIS, MARCH 3, 1868.

Estimation of titanic and niobic acids.—Process for bleaching palm oil by chromic acid.—Solubility of ether in solutions of sugar.—Alcoholate of baryta. ACADEMY OF SCIENCES, Feb. 10: On the heat set in motion during chemical combinations.—A new body analogous to diastase.—Nitrous fermentations.—Influence of light on vegetation.—Electro-capillary actions.—On the part played by electricity in muscular contraction.

THE processes proposed up to the present time for the estimation of titanic and niobic acids in mixtures have not succeeded very perfectly. M. Marignac, after testing the capabilities of the processes already known, has adopted the following. He takes 5 grammes of the metallic acid, and fuses it with 1·5 gramme of hydrofluorate of fluoride of potassium, first heating this salt gently until it is in a state of fusion (this fusion might almost be called aqueous), and then adding the niobic and titanic acids. By allowing the salt to fuse first, loss by decrepitation is avoided; afterwards a powerful heat is applied, and in a minute there is complete fusion, and the metallic acid is completely dissolved. It is not well to prolong the operation, as the mass has a tendency to creep up the sides of the crucible; for this reason a deep crucible should be chosen. The cooled mass is dissolved out by digesting with warm

dilute hydrochloric acid, employing for this purpose 20 c.c. of pure concentrated hydrochloric acid and 230 c.c. of water, and the solution collected in a flask. When it is quite cold, a bar of distilled zinc is introduced, sufficiently long to reach the bottom of the flask, and then to protrude at the neck. The flask is closed by a cork carrying a bent glass tube, by which the hydrogen is disengaged under water; the temperature of the solution must never be allowed to rise sensibly. At the end of twenty-four hours, the reduction is terminated, and it is only necessary to remove the cork, and withdraw the bar of zinc, and then to proceed immediately to determine the sesquioxide of titanium by a titrated solution of permanganate of potash. In default of rods of zinc, fragments of the metal may be employed, but they must not be too small; the zinc in this case need not be removed before making the titration. M. Marignac has obtained in the majority of cases very accurate results with the process. In some cases, however, a partial reduction of niobic acid took place, and in others incomplete reduction of titanic acid. The error in the first case can never be great without a brown colouration being manifested. M. Marignac has not been able to convert this quantitative method into one by which the titanic acid can be *separated* properly from the niobic acid. This chemist has also experimented upon the composition of the two other oxides of niobium. The brown oxide appears to be formed by niobic acid losing one-third of its oxygen, being thus intermediate in composition between niobic acid and the blue oxide (which passes so readily into the brown oxide, that no determinations could be made), and possessing the rather complicated formula Nb_3O_5.

The following is a detailed account of the process used by M. Engelhardt, of Leipzig, for bleaching palm oil by means of chromic acid:—A convenient quantity of palm oil is placed in a cauldron and heated to about 62° C., and allowed to repose during a whole night; the next day it is poured into a perfectly clean vessel and cooled down to 40° or 37° C. While this operation is being performed, a certain quantity of water is heated to ebullition; for 1000 parts by weight of palm oil, 45 of water, in this quantity 15 parts of bichromate of potash are dissolved. As soon as the solution has cooled a little, 60 parts of hydrochloric acid are added, and it is then mixed with the palm oil, which is vigorously agitated during the mixing. Five minutes suffice for the oil to be coloured a dull green. By continuing the stirring, the oxide of chromium becomes completely separated; the oil becomes clearer and clearer, and finally quite limpid; when this point is arrived at it is washed with hot water: the product is perfectly white. If the palm oil has not been thoroughly bleached by the treatment, the process is repeated with 23 parts of bichromate of potash, and 1 of hydrochloric acid. This method is capable of rapid execution, and gives very good results.

MM. J. Regnauld and Adrian have examined the solubility of ether in solutions of sugar. They have made experiments in order to solve the following questions:—What is the solubility of pure ether, and of ether more or less alcoholised, in aqueous solutions of sugar of 1·321 density? What augmentation in the solubility takes place when the density of the aqueous solution is changed to 1·261? These and several other questions have been closely examined to obtain data which will be used in revising the Codex.

M. Berthelot has examined the alcoholate of baryta. He has observed in making this compound by reacting, with carbonic oxide in the cold, upon an alcoholic solution of baryta, that the elements of water are separated from the alcohol by the baryta, at the moment of combination. The alcoholate can be obtained by boiling an alcoholic solution of baryta; it separates as an insoluble powder. The supernatant fluid is decanted, and the precipitate dried rapidly at 100° in a current of absolutely dry hydrogen. In the compound thus obtained, M. Berthelot has determined the baryta and alcohol regenerated by the action of water. The numbers obtained by him made a sum nearly 1 per cent in excess of that required by theory, a result which he explains by the elimination of the elements of water already referred to. Alcoholate of baryta, left in contact with a small portion only of its mother liquor, re-dissolves during cooling, yielding a solution much more concentrated than the original one.

At the meeting of the Academy held on the 10th of February, M. Favre contributed a memoir on researches on the heat set in motion during chemical combinations and decompositions.

Three memoirs were sent by M. Dubrunfaut; the first related to a new body analogous to diastase; nitrous fermentations, lately the subject of examination by M. Reiset, formed the subject of the second, and the influence of light upon vegetation the third.

M. Becquerel communicated a further account of his researches in capillary chemistry, and M. Marey brought before the Academy his researches on the part played by electricity in muscular contraction.

M. Favre has studied the compounds formed by the union of sulphuric acid, considered as a hydrogen salt, with zinc, iron, copper, and cadmium. He measured first the heat produced by the oxidation of the metal, then that produced by the combination of the acid with the oxide, and finally that caused by the hydration of the salt. He found that the heat produced by the simultaneous combination of the different elements represented exactly the sum of these quantities. M. Favre adduced new facts in confirmation of a result already announced by him,—that the heat produced by the combustion of a body, or absorbed during reduction, does not represent the whole of the heat put in motion. A certain amount of heat is necessary to prepare the body for combination or decomposition. The general result of M. Favre's work is that in the case of the salts with which he has experimented, these are more correctly represented as direct combinations of SO_4 with the metal, than as sulphates of oxides.

The matter analogous to diastase, described by M. Dubrunfaut in his first memoir, possesses the same property as diastase, but it is distinguished from it in having less saccharifying power, and by the property of rendering fluid 1,000 or 2,000 times more starch. The second memoir was chiefly a criticism of M. Reiset's recent communication on nitrous fermentations. M. Dubrunfaut has obtained in recent experiments confirmation of the theory formerly proposed by him to explain these phenomena. The following is the theory referred to:—Lactic acid is developed, and this decomposes the nitrate of potash of the saccharine juice, liberating a certain quantity of free nitric acid, which, by contact with organic matters is reduced to the state of binoxide of nitrogen; then the binoxide of nitrogen, by contact with the atmosphere, becomes nitrous acid. In M. Dubrunfaut's last memoir, a study of the influence of light on vegetation, he indicates a method of valuing the mechanical work performed by the light in the decomposition of carbonic acid.

CORRESPONDENCE.

THE ROYAL SCHOOL OF MINES.

To the Editor of the Chemical News.

Sir,—Will you kindly allow me to make a few remarks on some of the statements, made by A. L. E. on the School of Mines, which appeared in the Chemical News of last week?

A. L. E. says the School of Mines is an appendage to the Geological Museum; this is quite a mistake,—if the School had 200 to 300 students, the Museum would seem

just as much an appendage to the School, as the School now seems to be to the Museum.

"The prospectus or calendar, if it be worthy of the name" writes A. L. E.; now, no one dreams of calling the prospectus a calendar, or wishes to do so (always excepting A. L. E.): as to its being printed "on the spare page of a pamphlet belonging to the Geological Museum," this is not correct. If A. L. E. will look at the prospectus of the present session, he will find that it consists of 42 pages: the first page is the title-page; the next contains a table of contents; pp. 3, 4, and 5 contain a short account of the origin of the School of Mines; pp. 6, 7, and 8 give an account of the Geological Survey Mining Record Office and Library; the rest of the prospectus, pp. 9 to 42 (incl.), *i.e.*, 34 pages, are devoted to the School of Mines; *ergo*, the prospectus is printed on 34 pages, being spare pages of a pamphlet, consisting of three pages, belonging to the Geological Museum.

It would be better, no doubt, if the chemical laboratory was under the same roof as the Geological Museum; but this has very little influence on the number of students, its present position really producing but little practical inconvenience. The real reason of the small number of students is, in my opinion, that it is so little known to the general public. I am sure the names of the lecturers are far more widely known, than the Royal School of Mines with which they are connected. If, as A. L. E. suggests, the session was opened publicly, and the reports of the examiners, at the end of the session, were read out publicly to the council, an account of the proceedings would appear in the papers, and thus the public would be informed of the existence of an English School of Mines far more effectually than in any other way.

If the council would adopt this plan for two or three years, long enough to give it a fair trial (and there is a theatre luxuriant in cushions ready built for the purpose), I can not but think the number of students would be considerably increased.—I am, &c.,

A Student at the Royal School of Mines.

Jermyn Street, March 3rd, 1868.

To the Editor of the Chemical News.

Sir,—I notice with pleasure a letter in your last week's number calling attention to the present condition of the Royal School of Mines. It is indeed to be regretted, that this, which should rank foremost amongst our scientific training institutions, and which has abundantly proved its utility, should be compelled to exist under so many disadvantageous circumstances. In a silent and unobtrusive manner it has already done much; but there can be no doubt, that by a more judicious arrangement of details, and by the infusion of more energy and spirit into its management and maintenance, its sphere of usefulness might be considerably enlarged. Undoubtedly, bringing the School before the notice of the public, will have the effect of inducing a greater nnmber of students to avail themselves of the excellent opportunities it affords for obtaining scientific instruction: but this is not all; publicity will render more important service by contributing to give the Associates, who must necessarily share the obscurity of the School, a better status in scientific circles than, by virtue of their title, they have hitherto possessed. The course of study which must be pursued in order to obtain the distinction of "Associate," is sufficiently arduous to make the title, were the School better known, one of considerable merit. Candidates are obliged to have a knowledge, not superficial, but somewhat extensive and practical, of chemistry, physics, geology, and mineralogy. These studies occupy the first two years, after which the student may confine himself to that division in which he desires to take his Associateship; thus, if he wishes to pass in the mining division, the third year's subjects are mining, assaying, and applied mechanics; if in the metallurgical division, metallurgy, (theoretical and practical), and applied mechanics; if in the geological division, natural history and palæontology. It appears that about one-third of the Associates have taken up all three divisions; and I should mention that it is necessary to pass in the first class in the third year's subjects. After having completed this course, which it must be acknowledged represents a fair acquaintance with the principal branches of science, the students are sent out into the world with a title which is both little known and recognised, for it is not very flattering to be superciliously asked, "Where is the Royal School of Mines?" "What is the meaning of an Associate?" and whether it is an honorary title. In Dublin what used to be the Mining School is now called the Dublin College of Science, and perhaps if the School of Mines were called the London College of Science, it would be more appropriate, as comparatively few of the students ever have any thing to do with mines. But, apart from that, it is really desirable that the title should at least rank equal to the Associateship of King's College, and that the Associates should be allowed to make use of the initials A.R.S.M., or L.C.Sc. (Licentiate, College of Science), as indicative that the title is by no means an honorary one.

The state of things which renders some such public explanation as this necessary, is probably attributable to the manner in which the School has allowed itself to be tacitly ignored; it has no outward sign of existence, because there is no building which bears its name; it has never, in fact, assumed that position amongst educational institutions which, as a State school, it is entitled and privileged to occupy.

It is reported that the School does not pay its own expenses; if this be so, the authorities must have been strangely blinded to their own interests to have omitted such simple remedial measures as those suggested by your correspondent, for I feel convinced that were its existence and benefits more universally known, and if the title it confers carried with it any adequate amount of social standing, there would be an increase in the number of students, and consequently the School would no longer prove unprofitable, either in a pecuniary sense to the Government, or, in an intellectual sense, to the people. Apologising for intruding so much upon your space. —I am, &c. Delta.

March 3rd, 1868.

MISCELLANEOUS.

The Chemical Society.—We understand that Professor Kolbe, of Leipzig, is expected shortly in London, and that he will make a personal communication to the Chemical Society, on the direct transformation of carbonate of ammonia into urea. He will also be present at the President's soirée on the 11th instant.

Lectures at the School of Gunnery.—Mr. E. O. Brown is now delivering a course of lectures "On Gunpowder and its Substitutes," including gun-cotton and nitro-glycerine. The modes of firing by electricity and special fuzes will also be described. A similar course of six lectures "On the Chemical History and Military Applications of the Metals," was delivered before Christmas, at Shoeburyness, by Mr. J. Spiller.

Separation of Niobic and Titanic Acid.—C. Marignac. The great difficulties attending the estimation of titanic in presence of niobic acid have been overcome by the invention of the following method: 0·5 grm. of the mixed acids are fused with 1.5 grm. of potassic fluorhydride. The mixture after cooling is dissolved in about 250 c.c. chlorhydric acid of 1·015 sp. gr., and reduced by plunging a rod of zinc into the solution, precautions being taken to prevent access of air. Under these conditions niobic acid remains unchanged, and only titanic acid is reduced to sesquioxide. After 24 hours the zinc is removed, and a standard solution of potassic permanganate added.—(*N. Arch. ph. nat. August*, 1867.)

NOTES AND QUERIES.

Hofmann's Modern Chemistry. Lecture Experiments.—Having repeatedly performed all the experiments described in this book, I may be able to remove some of the difficulties which your correspondent "Amateur" has encountered. First, with regard to the percentage of sodium in the sodium amalgam to be used for the decomposition of hydrochloric acid gas. This is quite immaterial, provided that sufficient sodium is contained in the amalgam to decompose all the hydrochloric acid in the U-tube, and not so much as to render the amalgam solid. The U-tube is not likely to contain more than 100 c.c. of gas, which would weigh 0·16352 grm., and would require about 0·103 grm. of sodium to decompose them, so that if your correspondent dissolves about one gramme of sodium in the quantity of mercury necessary to fill up the U-tube, he will probably have a sufficient excess of sodium to ensure the decomposition of the gas; but as the experiment, as far as I am aware, has never been performed with an amalgam of a known composition, it would be advisable to try it in this manner before positively affirming that the quantities above given would produce the desired result. Next, as to recovering the mercury from the amalgam. If "Amateur" had reflected, he would probably have come to the conclusion that, as he was using sodium amalgam to decompose hydrochloric acid, so hydrochloric acid would be the best reagent for decomposing sodium amalgam; and he will find that if he places the amalgam into dilute hydrochloric acid, the sodium will be removed in a few minutes, and quite as efficiently as by the circuitous process of digesting with a solution of ammonic chloride. "Amateur" complains that the direction to use hydrochloric acid, of the specific gravity 1·1, is indefinite; perhaps 1·10000 will convey a more distinct impression to his mind—for this is what is meant. The object of employing acid of this strength is to avoid the inconvenience of which your correspondent speaks; the liquid not fuming in the air, and not evolving hydrochloric acid when heated.—H. M.

Estimation of Free Sulphuric Acid in Superphosphates.—Permit me to express my astonishment at the Chemistry of "Dr. A. A." as displayed in his replies to "V. C.," on the Estimation of Free Sulphuric Acid in Superphosphates; and to "F. W. W.," on the Determination of Sulphur in Pyrites. "Dr. A. A." informs "V. C." that by first ascertaining the entire amount of sulphuric acid in a hydrochloric acid solution of the superphosphates, and then the quantity of sulphuric acid left, after igniting another portion of the sample, the difference will be free acid. This looks remarkably simple and straightforward on the face of it. But "Dr. A. A." ignores the fact that most superphosphates contain sulphate of ammonia, and no inconsiderable proportion of organic matter. It is not difficult, then, to conceive how these would militate against the accuracy of the results by "Dr. A. A's." process. To say nothing of the entire volatilization of the sulphate of ammonia, or how the sulphate of lime would stand affected by ignition in the presence of carbonaceous matters, a considerable quantity of the free acid would in all probability, under this exciting condition, combine with the lime of some of the undecomposed phosphates. Should "V. C." require a more ready, though not nearly so accurate a process as the admirable one volunteered by Dr. Moffat, Glasgow, I may say, that an aqueous solution of the manure, treated with a very dilute normal solution of ammonia, gives tolerably fair results. Again, as regards "Dr. A. A's." expeditious mode for determining the sulphur in pyrites,—that is a more extraordinary outrage on chemical science than the former one, on which I have just commented. However he gets even "an approximate estimation" by submitting a sample of pyrites (FeS_2) to "strong incandescnce for two or three hours in an open capsule"; producing, of course, an indefinite mixture of sesquioxide and protoxide of iron ($Fe_2O_3 + FeO$), and afterwards reckoning the loss of weight as sulphur, is best known to himself: but to me is an inexplicable enigma. I have already, I apprehend, trespassed too much on your space, and will only therefore refer "F. W. W." to almost any handbook on analysis for a speedy way of arriving at the value of pyrites.—S. A. S.

Sulphur in Pyrites.—In answer to the query of your correspondent F. W. W., I beg to offer the following remarks. The method of analysis of sulphur ores usually adopted for commercial purposes is in substance this: a known weight of the ore reduced to fine powder is oxidised (best in a small flask with a funnel placed in the mouth to avoid loss by spirting, and heated on a sand-bath) either by strong nitric acid or nitro-hydrochloric acid (aqua regia), perfectly free from sulphuric acid; after the oxidation is complete, the liquid is evaporated down as far as possible to expel the majority of the remaining nitric or hydrochloric acid; the residue is boiled with a little water, and almost but not quite neutralised by ammonia; a solution of barium chloride of known strength is then added until no further precipitate is produced, the exact point being found by filtering off a little of the liquid after each addition of barium chloride and adding to it a few more drops of the standard solution, care being always taken, in case of a further precipitate being thus produced, to add this filtrate to the original solution, and mix well before filtering a second time. In case of overstepping the mark, it is convenient to have at hand a solution of sodium sulphate of strength precisely equal to that of the barium chloride; this solution may then be cautiously added with repeated filtration and examination of the filtrate with the sulphate solution, until the point is just reached, when addition of sulphate solution produces no further precipitate: by subtracting the volume of sulphate solution thus used from the total volume of barium solution added, the exact quantity of this latter consumed is known. If 1 gramme of sulphur ore be taken, and 32·5 grammes of pure anhydrous barium chloride be dissolved to a litre of fluid, each cubic centimetre of barium solution used will represent $\frac{1}{2}$ per cent of sulphur in the ore examined; 22·19 grammes of anhydrous sodium sulphate being dissolved to a litre for the second solution. In case of lead being contained in the ore, an error is introduced from the formation of insoluble lead sulphate; as lead, however, rarely occurs in any perceptible quantity, this error is negligible, the process only giving approximate results. Where greater accuracy is required, it is advisable to precipitate the sulphuric acid formed from the original liquid (filtered from insoluble residue) by barium chloride or nitrate, and to weigh the barium sulphate produced. Instead of oxidising by acids, the powdered ore may be suspended in caustic potash (free from sulphate), and oxidised by passing washed chlorine into the liquid; lead being converted into dioxide, is thus rendered non-injurious; the alkaline liquid obtained is acidified and precipitated by barium chloride as before. Or the finely powdered ore may be mixed with 7 or 8 times its weight of a mixture of equal parts of sodium carbonate and potassium nitrate, and heated cautiously in a capacious crucible; the mass being finally boiled with water, and the sulphate estimated in the filtrate; in practice, however, it is somewhat difficult thus to avoid loss by a deflagration, especially with rich sulphur ores. In the volumetric determination usually pursued, a curious circumstance is occasionally observable when much free acid exists in the solution, viz., that a point may be reached when the filtered liquid is clear, and remains so even on standing for a short time, but yields a cloud, or even a precipitate, on the addition either of barium solution or sulphate solution; this source of error is mostly avoidable by nearly neutralising the free acid with ammonia. Another convenient process consists in fusing the weighed ore with a weighed quantity of anhydrous sodium carbonate, twice as much potassium chlorate as ore, and 12—20 times as much sodium chloride (added to moderate the action); CO_2 is expelled, KCl formed, and all the sulphur converted into Na_2SO_4; by dissolving the residue in water and estimating alkalimetrically the unaltered sodium carbonate by a standard acid solution, the portion converted into sulphate, and hence the sulphur in the ore, is known. Besides the difficulty of preventing loss by deflagration, this method is open to the small errors caused by the reckoning all arsenic present to be sulphur—usually, however, of no moment for commercial purposes; any calcium carbonate in the ore may, if required, be previously dissolved out by dilute hydrochloric acid.—C. R. A. Wright, B.Sc.

TO CORRESPONDENTS.

R. H. R.—"Liebig's Agricultural Chemistry;" "Johnstone's Chemistry of Soils."

P. Holland.—Received with thanks; too late for insertion this week.

Capt. W. A. Ross.—We shall be glad to see the crystals, if you will send specimens.

R. Broadhurst.—Use hydrofluoric acid gas to obscure the glass globes. Those parts which are required bright must be coated with wax.

Errata.—In Dr. Müller's remarks on Mr. Forbes's lecture on Chemical Geology, reported in our last number, the following corrections are requested to be made:—Page 106, line 14 from bottom, for "*without* solidifying," read "*when* solidifying;" page 107, line 1, for "*divided*," read "*decided*;" line 23, for "*their* mineral chaos," read "*this* mineral chaos."

W. Schofield.—The letter has been forwarded.

W. Skey.—Communications have arrived, and shall be inserted. You will observe that the notes named in your letter have already appeared.

G.—Why will not chalk answer? It will satisfy your conditions.

A. G. S.—The *Bulletin de la Société Chimique de Paris* is published monthly. It can be procured by order through any foreign bookseller.

Solutus.—The normal sulphate of cerium is much more soluble in cold than in hot water, and is precipitated when a cold saturated solution is boiled.

Communications have been received from Dr. Wood; Miss Chapman; J. B. Giles; J. Davies; R. Eaton (with enclosure); J. T. Brown, F.C.S.; Bollman Condy & Co.; W. Field; Dr. W. Bird Herapath, F.R.S.; C. F. Burnard, F.C.S.; F. C. Calvert & Co.; Jones & Co.; P. Dorn; Capt. W. A. Ross, R.A.; H. B. Ingram; Rev. Richard Kirwan; J. Muspratt & Sons; A. Payne; B. Wheeler & Co.; W. Kellner; T. L. Patterson; Dr. E. W. Davy (with enclosure); Dunn & Co.; C. M. King; C. Mitchell & Co.; Baron von Seckendorff (with enclosure); Dr. Forbes Watson; John Knight & Co.; Kingsbury & Co.; Fletcher & Co.; John Davies, M.D.; E. Brooke & Co. (with enclosure); L. Demuth (with enclosure; J. R. Irvine; Reginald Petre; R. Broadhurst; C. Steer; Philip Holland (with enclosure); F. J. M. Page; Dr. H. Müller, F.R.S.; W. Schofield (with enclosure); H. Merivale; F. Scott; W. Skey, New Zealand (with enclosure); W. Cortis, jun.; T. A. Readwin; J. Spiller; D. Forbes, F.R.S. (with enclosure); Dr. Odling, F.R.S.; Professor Heaton; H. Bailliere; Dr. F. B. Courteney; G. H. Mann, Troy, United States; F. Sutton; W. Bywater; Dr. Adriani (with enclosure).

Books received.—"The Illustrated Photographer;" "Dictionary of Chemistry," by Henry Watts, B.A., F.R.S., F.C.S. Part XLIV. Types, Water. London: Longmans, Green, & Co.; "Hardwicke's Science Gossip;" "Pharmaceutical Journal;" "American Journal of Pharmacy," November, January; "The Scientific Review;" "La Meraviglie Della Scienza;" "Zeitschrift für Chemie;" "Faraday as a Discoverer," by John Tyndall. London: Longmans & Co.

THE CHEMICAL NEWS.

VOL. XVII. No. 432.

ESTIMATION OF NITRITES IN WATERS.

By PHILIP HOLLAND.

DR. MILLER, in his paper "On some Points in the Analysis of Potable Waters,"* alludes to a reaction for the detection of nitrites, viz., the property these salts have of liberating iodine from an acidified solution of iodide of potassium.

Dr. Angus Smith† asserts that an amount of nitrous acid, so small as 1 in 3¼ millions of water, may easily be discovered in this manner. In spite of its extreme delicacy, I am not aware that any process for the quantitative estimation of nitrous acid has been founded on the reaction. Most, if not all water analysts, unless they be recent converts, are inclined to be satisfied with permanganate indications, the presence or absence of nitrous acid being assumed according as the permanganate is quickly or slowly decolourised.

The process I am about to describe is one in which the colouration imparted by the free iodine is taken as the measure of the nitrous acid present. For a "colorimetric" standard, I know of nothing better than a solution of iodine in iodide of potassium; about 4 grms. is dissolved in excess of iodide, and made up to the volume of a litre.

In the next place, it is necessary to prepare a pure salt of nitrous acid; for this purpose, commercial nitrite of potassium is precipitated with $AgNO_3$, the resultant silver salt washed by decantation, re-crystallised, and dried in vacuo.

To ·3276 grm. of the silver salt, dissolved by heat in water, is added a slight excess of pure NaCl, and the liquid, when cold, made up to the volume of 1000 c.c.

10 c.c. = 1 mlgrm. HNO_2,

The iodine solution is "titrated" as follows:—A permanganate burette divided in $\frac{1}{10}$ths of a c.c., and fitted with a float, is filled with it. Two narrow white glass jars are placed on a white slab; on each is marked the point at which a volume of 200 c.c. of water stands. Into one, A, is put an amount of the standard nitrite equal to 1 mlgrm. of HNO_2, together with 6 c.c. of iodide of potassium (1 to 10 of water), then distilled water nearly to the mark, and lastly dilute H_2SO_4. The whole is to be mixed and allowed to stand until the colour is fully developed; when that point is reached, the second jar, containing an amount of iodide of potassium and acid equal to that in A, is filled to within a short distance of the volume mark with water, and placed under the burette; the iodine solution is then cautiously delivered into it, until the depth of colour is judged to be equal in intensity to that in A. The iodine solution should be of such a strength that 10 c.c. have a colouring power equal to that possessed by 1 mlgrm. of HNO_2 in the presence of iodide of potassium in a volume of 200 c.c. of water. It is unadvisable, when making the comparison, to add the standard nitrite from a burette to an acidified solution of iodide of potassium, for an obvious reason. It may, however, be suggested that a definite quantity of nitrite should be added together with iodide to the water in the jar, and lastly the acid. Such a method is tedious, in that it would be necessary to make several assays before attaining the desired shade.

* Journal Chem. Society, May, 1865.

† "Estimation of Organic Matter in Waters, with reference especially to sanitary purposes," page 20.

The following determinations of nitrous acid were made:—An amount equal to 1 mlgrm. was evaporated with a litre of spring water to the volume of 100 c.c., the residue was filtered into the cylinder, and the filter washed; when cold some iodide of potassium was added, then distilled water to within ¼ inch of the mark, and lastly dilute H_2SO_4. After thoroughly mixing, the contents of the cylinder were left undisturbed, for the colour to become fully developed; when that stage arrived it was found that 11·5 was the number of c.c. of iodine requisite to impart the same colour to an equal volume of water. 10 c.c. should only have been required; the excess, therefore, of 1·5 c.c. is the measure of the HNO_2 in the water employed. In order to justify this assumption I evaporated two separate quantities of a litre.

	Iodine, c.c.
No. 1.	1·2
,, 2.	1·4

These figures give ·12 mlgrm. per litre as the amount of HNO_2 in the spring water. Artificial waters were made by adding known amounts of HNO_2 to common water; the quantity of HNO_2 already existent therein being deducted.

Amount HNO_2 added in mlgrms.	Iodine required in c.c.	Mlgrms. HNO_2 found.
·43	4·5	·45
·86	8·4	·84
1·52	14·8	1·48

The following natural waters were examined:—

A. A well water.

B. A well water, in which nitrates were found in some quantity.

C. A brook water containing some sewage matter.

A. ·23 mlgrm. HNO_2 per litre.
B. ·27 ,,
C. ·63 ,,

The process is not suitable when the quantity of nitrous acid is large; whilst it ranges below and up to 1 mlgrm. corresponding results can be obtained.

Some precautions are necessary in certain cases. H_2S and sulphides if present must be removed; the former escapes during the evaporation of the water; the latter may be decomposed by a metallic oxide.

Organic colouring matter can be precipitated by means of chloride of calcium, carbonate of sodium, and a few drops of hydrate of potassium, as suggested by Dr. Frankland. Kaolin could perhaps be employed for the purpose.

Chorley, March 4, 1868.

ON THE DETERMINATION OF TARTARIC ACID.

By GEORGE H. MANN,

Polytechnic Institute, Troy, New York, United States.

TEN grammes of the crude tartar are mixed with a sufficient quantity of pure hydrate of potassa, free from carbonate, in order to neutralise the free tartaric acid present; the mixture is evaporated to dryness in a water bath, and heated to redness in a closed porcelain crucible. The tartaric acid is thus decomposed into carbonic acid, which is determined in the following manner: place the warm mixture of the carbonates and carbon in any of the various forms of the apparatus designed for this purpose, and illustrated in Fresenius' "Quantitative Analysis," fill the upper bulb with strong sulphuric acid, tare the apparatus on the balance, admit the acid into the lower part, when carbonic acid will be evolved, and the amount present may be estimated from the loss of weight. Then we shall have the proportion CO_2 : $C_8H_4O_{10},2HO$ :: the amount of carbonic acid found: the amount of crystallisable tartaric acid present in 10 grammes of crude tartar. Or expressed in numbers, 22 : 150 :: α grammes of carbonic acid : χ grammes of crystallisable tartaric acid.

ON THE USE OF THE

SPECTROSCOPE AND MICROSPECTROSCOPE

IN THE DISCOVERY OF

BLOOD STAINS AND DISSOLVED BLOOD,

AND IN PATHOLOGICAL INQUIRIES.

By W. BIRD HERAPATH, M.D., F.R.S.

(Concluded from page 115.)

In red hæmatine an interval exists between the sodium or solar line D and the margin of the first band of absorption, which in red hæmatine is less distinct or *sharp* than that in scarlet cruorine, or than its fellow in the same spectrum; both of which therefore are in red hæmatine found in the green rays. Most chemical reagents convert scarlet cruorine into brown hæmatine without any previous passage through the stage and properties of purple cruorine. The effects of reduction on brown hæmatine are evanescent, and the solution rapidly becomes deficient in optical power; it does not assume the former properties of brown hæmatine. The absorption bands fade away and disappear, the one nearer to the sodium line D being the more persistent, and remaining sharp during several days; this takes place even if the bottle be air-tight. Re-treatment with protoxide of iron will again reproduce the two absorption bands as before. It is brown hæmatine which is usually discovered in old blood stains. but red hæmatine in dry and more recent blood clots. A specimen on the table six months old still shows the two bands of red hæmatine, whilst one of two years old shows the spectrum of brown hæmatine.

It may be as well to state, that these specimens of dried blood had been kept in a moderately dry room, powdered, and in a paper pill-box for some portion of the time, but during the major part of the period on cloths exposed to the air without any care, and in the ordinary atmosphere of an inhabited room, with gas burning nightly. Solutions of these specimens of blood in distilled water, after having been kept closely corked for some days, underwent a peculiar change. Both the brown and red hæmatine were *spontaneously changed* into purple cruorine, and now show only the one broad band of absorption due to that colouring matter. This effect is probably due to reduction by sulphuretted hydrogen. In *no specimen* would it be possible to detect *any globules by the microscope*, as the colouring matters have been all dissolved in distilled water, and the globules destroyed. The microscope would therefore fail in detecting blood in all of them, whilst the chemist might probably recognise it in most of the fluids, and so will the spectroscope. When an old blood stain has been so changed by exposure to air that the hæmatine gives but very faint and indistinct absorptive bands, it is possible by deoxidising the solution of hæmatine by means of a little recently precipitated hydrated protoxide of iron to reproduce the bands in nearly the same intensity, though slightly differing in position, than they were in blood stains of a very recent period of their formation, when scarlet cruorine would of course be the colouring material.

This experiment is readily made by adding a few drops of a weak solution of proto-sulphate or proto-chloride of iron, and then a few drops of liquor ammoniæ; the green hydrated protoxide of iron resulting has a great affinity for oxygen, and at once reduces the hæmatine and restores its colour and optical properties.

It is essential that putrefaction should not have actually destroyed the hæmatine. The spectroscope which has been mounted upon this stand is so perfect in its action that it readily exhibits Fraunhofer's lines in solar or lunar light, and it divides the bright yellow sodium line D into two when properly adjusted. It therefore gives great accuracy of observation, and the stronger dark lines of the solar spectrum become so many fixed points for the comparison of the position of the dark absorptive band of various coloured solutions when they are observed by daylight.

One disadvantage attendant upon the employment of this form of spectroscope is the quantity of material necessary to be employed. Several drops of blood must be at the disposal of the operator to get a sufficient coloured solution to fill the little test-tubes used in the optical examination. By modifying the instrument, and introducing a larger tube for containing the liquid to be examined; say a column of six or eight inches in length, it would be possible to discover one drop of scarlet cruorine in a pint of distilled water without much difficulty, a quantity so minute that no perceptible colour could be visible to the unaided eye, and no other method of analysis would be capable of detecting it. Quantities like these are not always to be had, and a recent well-known case in which I had the opportunity of first using the microspectroscope in a medico-legal inquiry would have altogether failed if I had depended alone upon this "fluid spectroscope," as nearly all traces of blood stains had disappeared from the weapon employed by the murderer in consequence of the hatchet having been left exposed in the woods near Mountain Ash for several weeks after the deed was accomplished (case Reg. *v.* Robert Coe, Swansea Spring Assizes, 1866). It was only on the removal of the head of the hatchet that any appearances of blood were to be obtained from the surface of the handle, which had been protected by the iron ring, and on carefully making thin sections of these stained portions of wood, and treating them with distilled water, a few drops only of a brownish coloured fluid were obtained, which coagulated and became discoloured on boiling; also another drop when placed in a very minute tube, about half an inch long, and the $\frac{1}{8}$th of an inch in diameter, the total contents of which tube were one grain and $\frac{1}{3}$rd of distilled water, gave the optical absorptive bands due to old blood.

This little drop of bloody-coloured fluid was placed on the stage of the microscope, and examined with an inch Ross objective, illuminated by an achromatic condenser, and the microspectroscope was inserted into one of the tubes of a binocular microscope as an ocular lens would be employed. This form of instrument is that known as the Sorby-Browning spectroscope, and it admits of great precision, as it has a lateral spectroscope as well as a terminal one. These two spectra appear side by side in the field of view, and being pefectly parallel, admit of examining substances by two sources of light at the same time, or enable us to make comparisons between two different or similar substances at the same time and by the same kind of illumination—the two spectra being both visible with the same eye. This form of instrument is very sensitive to small quantities of blood; and it would be perfectly easy to detect and ocularly examine the blood contained in the stomach of a rascally "flea," and even dilute it with a teaspoonful of water without losing its properties; especially if he had made anything like a decent forage upon some sanguineous individual. But other forms of spectroscope have been adapted to the microscope by different opticians or inventors. One great objection to the other forms consists in the greater complexity of the arrangements and the variety of adaptations to be made, rendering the observations both difficult and troublesome, involving great loss of time, and, of course, greatly multiplying the chance of failure. But to show how small a quantity of blood is really necessary for recognition with this instrument, Mr. Sorby has distinctly obtained the absorptive bands in a single half globule of dried blood; in order to obtain this result the object was illuminated by a powerful achromatic condenser, and one of Smith and Beck's new $\frac{1}{25}$ objectives was employed.

However, without having gone as far as this, my own observations have proved that it is possible to obtain very evident results from less than one-thousandth of a grain of dried blood, the colouring matter of which had been dissolved out by one drop and a half of distilled water. In fact, comparative experiments proved that in the Mountain Ash case the quantity of blood experimented on, and productive of conclusive results, did not

exceed one-thousandth part of a grain; and the justness of the sentence was afterwards proved to the satisfaction of all parties by the confession of the prisoner previous to his execution.

However, this optical or micro-spectroscopic result was in this case also confirmed by the microscopic examination and detection of the blood globules, as well as by the chemical testing of the solution of hæmatine, or rather red hæmatine, for it had not passed to the extreme state of change visible in old dried blood.

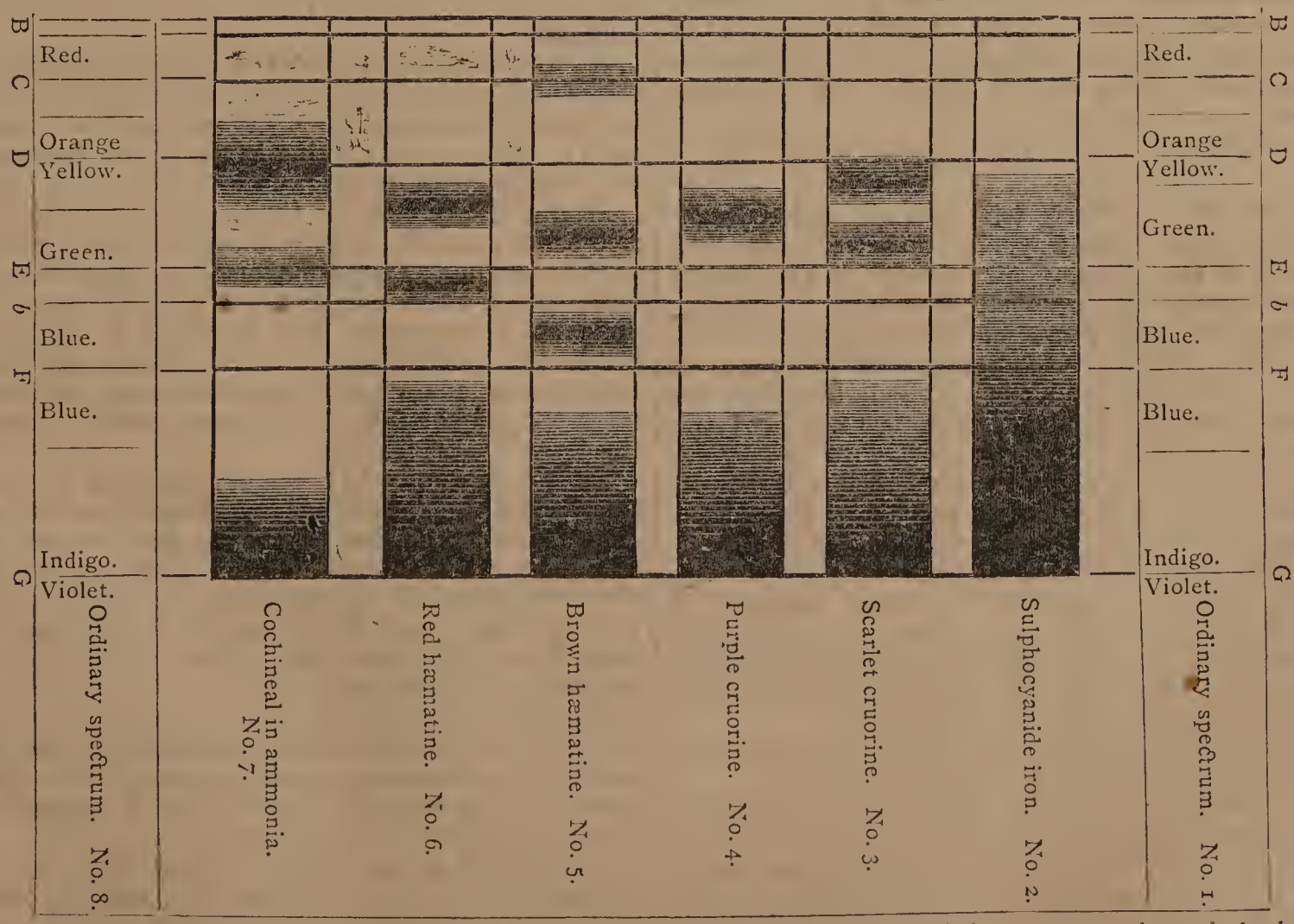
It is somewhat remarkable that though various other bodies have, to the eye, all the appearance and colour of blood, yet none of those usually met with have any spectra to be mistaken for those of the various forms of blood colouring matters herein described. The generality of soluble red colouring matter absorbs more or less of the violet, blue, green, or yellow, and even orange, rays of the spectrum *continuously*. Some wholly absorb the spectrum with the exception of the red ray which they transmit.

The well-known sulphocyanide of iron often called (and used by conjurors and by chemists) artificial blood, is strikingly wanting in those optical absorptive powers or bands so indicative of cruorine or hæmatine in their various forms. Spectrum analysis is capable of rendering great service in chemical and pathological enquiries, as by means of the optical spectra blood may be easily recognised in urine, and detected in some forms of albuminuria, even if it be also charged with the colouring matter of bile. Highly jaundiced urine absorbs all the blue end of the spectrum, but as the green, orange, and red rays are unaltered, the two bands of scarlet cruorine are readily seen. The recent menstrual fluid, when dissolved and properly diluted, gives the spectrum of scarlet cruorine; so does urine mixed with menstrual fluid even if highly bilious. Two substances only have been found comparable in their optical effects to those of hæmatine,—a dilute ammoniacal solution of carmine, and a similar solution of cochineal in ammonia, which colouring matters are very unstable and fade quickly. In both these liquids the same colouring matter exists, as carmine is produced from cochineal. The two absorptive bands are much broader and more diffuse than any of the optical appearances due to the colouring matter of blood, though most like those of brown hæmatine, and only a novice in spectrum analysis could possibly mistake the one for the other, whilst the least attempt at chemical investigation would pronounce them different, the action of heat alone being sufficient to coagulate the colouring matter of blood, whilst the cochineal and carmine would remain unchanged. Reducing agents would also settle the question definitely.

Acids immediately change the colour of cochineal solutions to a reddish orange, deficient in absorptive, and even a spontaneous coloration will take place in the ammoniacal solution of cochineal. Solutions of carmine are more permanent. Acetic acid produces no change at first, but eventually the colouring matter is precipitated. Sulphide ammonium does not alter it in the least, nor does the alkaline solution of protochloride of tin. On adding protochloride of iron and ammonia to any solution of carmine, the colouring matter is immediately precipitated in combination with the oxide of iron as a brown or maroon coloured compound.

But one great safeguard in medico-legal enquiries will be the absence of cochineal or carmine from those positions in which blood may by any possibility be found, some cloth fabrics alone being dyed by a mordanted cochineal. Some scarlet clothes also are of this character, and the carmine colour being fixed by alumina, would be insoluble in cold water; whereas the cruorine or hæmatine would dissolve with more or less facility according to its age. It is evident, therefore, that all these considerations render the detection of blood stain by spectrum analysis a matter of but little doubt or difficulty, even when in minute quantities; and, in conclusion, although spectrum analysis does not go one step farther than we were before in our powers of discriminating human blood from that of other mammalian, or red-blooded creatures, yet it gives us greater facilities of demonstrating the presence of the colouring matter of blood, even in inconceivably minute and almost invisible proportions, whilst the facility with which the observations are made is a great, if not the greatest recommendation to the employment of this method whenever practicable.

Chart illustrative of Dr. Hcrapath's Paper on the Spectroscope.



In this Chart eight spectra are exhibited, two being the ordinary solar spectra, with seven of the more strongly marked solar lines drawn perpendicularly through all the other spectra as indices for the real position of the various absorption bands. The strong black bands towards the violet ends of the spectrum (which are also absorbed) show the amount of the usual absorption of red fluids of that end of the spectrum; but all the other bands are indicative of the fluids examined.

ON THE VENTILATION OF SEWERS.*

By DR. W. ALLEN MILLER, V.P.R.S.

Determination whether the charcoal injuriously impedes the ventilation.

This could be determined by two methods, viz.—*a.*, by ascertaining the ordinary draught of air in the sewer when there was no charcoal, and then ascertaining the amount of draught after the charcoal had been introduced; or *b.*, by analysing the air before and after the introduction of the charcoal, and ascertaining whether any serious diminution of oxygen or increase of carbonic acid had occurred.

a. The variation of the draught of air in the sewer was examined, as follows:—A puff of smoke was produced by firing a little gunpowder, and ascertaining the time occupied by the smoke in travelling up or down the sewer for a known distance. This method answered its object sufficiently well, but it was found that such slight causes interfered with the strength of the draught, that much less information of value was obtained than had been anticipated, since it was found that the direction of the draught and its amount were liable to be interfered with by local accidents, such as the imperfect closure of a trap, the variable force of the wind, and the opening or shutting of a side entrance: so that the act of entering or leaving the sewer for the purposes of experiment, was more than once found to reverse the direction of the current of air in the body of the sewer during the observations.

So far as the observations go they, however, show that the introduction of the charcoal into the boxes produces a sensible retardation of the current of air. Indeed any other result would be impossible.

Three sets of experiments were made upon this plan; one set before the charcoal was introduced, and two other sets after the boxes had been charged with charcoal.

The average rate deduced from six experiments *without the charcoal* showed a current moving at the rate of 4,254 feet per hour.

The first set of trials, consisting of three experiments, *after the introduction of the charcoal*, gave a current moving at the average rate of 3,263 feet per hour, and the second set of trials, also, an average of three experiments, showed a current of 2,005 feet per hour, in the same part of the sewer.

b. Effect of the charcoal on the chemical composition of the air.

It was ascertained by direct trial that air passed freely through the charcoal in the trays, but no sewer odour was ever perceived in the escaping air; though if the box of charcoal were purposely removed from the ventilating shaft, an immediate and powerful odour of sewage was perceived. The charcoal, therefore, did its work in absorbing the offensive products. It had, however, no direct action upon the atmosphere in the body of the sewer; but, indirectly it might be expected to impair its quality by detaining it for a longer period within the sewer, thereby causing it to lose a larger portion of its oxygen than if a freer current of air was maintained. The oxygen during its detention would combine with part of the decomposing refuse, whilst an increased amount of carbonic acid was to be looked for as one of the products of decomposition; besides which a small quantity of carburetted, and occasionally of sulphuretted, hydrogen might occur in the more stagnant parts.

Samples of air were therefore collected for analysis both before and after the introduction of the charcoal.

From an average of eighteen experiments upon the quantity of carbonic acid, made during the month of May, before the charcoal was introduced, and before the lateral sewers had been provided with flaps, the average quantity amounted to 0·106 parts per cent, while in the open air, the average may be taken at 0·040 parts. The mean temperature of the air within the sewer during this period was 50°·8, ranging between 48° as a minimum and 56° as a maximum.

The mean amount of oxygen in the air of the sewer was for the same interval, from an average of six experiments, 20·71 per cent, the proportion in the open air being 20·96.

After the introduction of the charcoal, the quantity of carbonic acid was found to have risen to 0·132 parts per 100 of air, the mean temperature in the sewer having risen to 56°·2, with a minimum of 52°, and a maximum of 61°·5.

The average amount of oxygen was 20·79. No sulphuretted hydrogen was present.

As a general rule it was found that the quantity of carbonic acid in the air within the sewer increased in proportion as the temperature rose in the sewer, and it declined again as the temperature fell.

No connection between the fluctuation in the amount of carbonic acid and the variation of the barometer could be traced.

On the whole, the air of this sewer was not seriously altered by the obstruction occasioned by the charcoal.

But though this sewer in the Avenue Road offered great mechanical facilities for carrying out experiments of this nature, there were circumstances which detracted from the value of the conclusions to be drawn from the results obtained there. The flow of water was rapid (four feet per second), the sewer itself a clean one, and the openings for ventilation numerous; so that, though it was quite certain the use of charcoal in such a case would occasion no difficulty, it did not indicate whether, in a fouler sewer with fewer air outlets, it would still be proper to use the charcoal ventilators.

It was therefore arranged in July that a second sewer should be placed under experiment; and for this purpose the Great Smith Street sewer was judged by Mr. Lovick to be particularly suitable.

The register thermometers were suspended in this sewer in the Brompton Road on the 3rd of October, and samples of the air in its ordinary condition were taken on the 4th and 5th of the month. On the 6th of October charcoal was placed in all the boxes in the ventilating openings, and samples of the air of the sewer collected as before, for analysis.

An average of six samples of air before the charcoal was introduced showed that the air in its normal state contained the large proportion of 0·307 per cent of carbonic acid, with a temperature ranging in the sewer between 57° and 60°, with a mean of 58°·2.

During the months of October and November, twenty-four samples of the air were examined after the introduction of the charcoal, giving a mean of 0·251 per cent of carbonic acid, with a mean temperature of 53°·2, ranging between 49°·5 as a minimum, and 57°·5 as a maximum. The diminution in the proportion of carbonic acid is in no way to be accounted for by the use of charcoal, but is occasioned, no doubt, by the fall in temperature which took place as the days became shorter.

So far, therefore, as these experiments go, even in this tide-locked and imperfectly ventilated sewer, no serious obstruction to the flow of the air through the ventilators was produced by the charcoal. It should be added, that the proportion of oxygen after the introduction of the charcoal was found, on an average of four experiments, to amount to 20°·7 per cent. This sewer was free from sulphuretted hydrogen.

The introduction of charcoal ventilators not only will require a large outlay, but it involves a possible risk to the safety of the men who work in the sewers. The results appear to me to be such as warrant a systematic trial of the method over a limited area.

The second point for inquiry was: *the frequency with which the charcoal will require to be changed* in the ventilating boxes.

As yet the charcoal put into the ventilators in the Avenue Road sewer continues in a perfectly efficient

* Abstract from the Report to the Metropolitan Board of Works.

condition. I examined some of the charcoal which had been in the Park Street ventilator (the lowest point in the sewer from the Avenue Road) for nearly six months. It contained nearly one-fourth of its weight of moisture, but appeared as though dry when handled. This moisture had been condensed in the pores of the charcoal, and had not penetrated the box from the road. 100 parts of the damp charcoal gave off, when heated, 19·7 parts of water, and a small quantity of an offensive ammoniacal liquid. Nitrates were also found in small quantity in the products retained by the charcoal.

The charcoal in the Great Smith Street sewer has been in use only about ten weeks. It is a much fouler sewer, but the results of the examination show that it is still efficient, although in one or two instances, where the traffic is considerable, the mud has worked through the covering, and gained access to the upper layer of charcoal. The lower part of the charcoal was, in each case, clean, and though saturated with condensed moisture, it still effectually purified the air from the sewer, which, as direct trial showed, passed freely through the layer of charcoal.

I examined a portion of the charcoal after ten weeks' exposure in the ventilator in the King's Road. It had absorbed 37·2 per cent of moisture, and the water which was distilled off had the peculiar and offensive odour of the sewage gases.

The charcoal appeared dry to the touch, although it had condensed so large a proportion of water. The important practical conclusion is, that charcoal, though thus saturated with moisture, does not obstruct the escape of air, which it still effectually purifies.

3. *The mechanical arrangements best adapted to the employment of charcoal air filters.*

In the earlier trials, three layers of charcoal, each about two inches thick, were employed. The charcoal was broken into fragments about the size of cob-nuts, ordinary wood charcoal being used. A bushel of this charcoal weighed 15lbs. 2½oz., and the average weight of the charcoal in the three trays together was 6½lbs.

Experience showed that some modification was necessary to prevent the road drift from working in; and in the Great Smith Street sewer a single tray of charcoal, 6 inches in depth, was substituted for the three shallower trays with good effect.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, March 5th.

Dr. Warren de la Rue, F.R.S., &c., President, in the Chair.

The minutes of the two previous meetings were read and confirmed, and a long list of donations to the library announced. Dr. Schenk, Mr. Vosper, and Mr. Gilbert W. Child, were formally admitted as Fellows of the Society, and the following gentlemen were duly elected, viz., Dr. Benjamin H. Paul, 8, Gray's Inn Square; Mr. Thomas W. White, Ifield, near Crawley, Sussex; Edward Dowson, M.D., 117, Park Street, London. Mr. Reinhold Richter, of the Rothamstead Laboratory, was also elected as an Associate. The candidates proposed for admission were John Tyndall, LL.D., F.R.S., Fullerian Professor of Chemistry in the Royal Institution of Great Britain; Frederick Guthrie, Ph.D., F.R.S. Edin., Lecturer on Chemistry in the College of Mauritius; William Brantingham Giles, Chemist at the Borax Works, Old Swan, Liverpool. For the second time were read the names of Mr. R. Calvert Clapham, Walker Alkali Company's Works, Newcastle-upon-Tyne; Rustomjee Byramjee, M.D., Assistant-Surgeon in Her Majesty's Bombay Army; and Edward Menzel, Ph.D., recommended by the Council to become an Associate.

The names of officers and other members of Council proposed for election at the anniversary meeting on the 30th of March were announced. For President, Dr. Warren de la Rue, F.R.S., F.R.A.S, &c. For Vice-Presidents, Dr. E. Frankland, F.R.S., and Dr. J. H. Gilbert, F.R.S. For Foreign Secretary, Prof. F. A. Abel, F.R.S. New Members of Council:—Dr. E. Atkinson, Dr. E. J. Mills, Mr. W. H. Perkin, F.R.S., and Mr. John Williams.

Professor J. A. Wanklyn read a paper "*On the Action of Oxidising Agents on Organic Compounds in Presence of Excess of Alkali*," of which Mr. E. T. Chapman and himself were joint authors. Part I. "*Ammonia evolved by Alkaline Permanganates acting on Organic Nitrogenous Compounds.*" It has already been shown in previous communications that albumen evolves ammonia when submitted to the action of alkaline permanganates, and, further, it is asserted that this ammonia is perfectly constant in quantity, and always proportional to the amount of albumen employed, although the whole of the nitrogen does not take this form. The authors have now extended this enquiry to organic nitrogenous substances in general, and find the action to be definite, yielding proportions of ammonia varying with the nature of the body acted upon, and sometimes the entire quantity was obtained. A number of typical substances were selected for examination, with the following results:—

Class I. Bodies furnishing the *whole* of the nitrogen in the form of ammonia: amylamine, di-amylamine, asparagine, piperine, piperidine (sulphate), narcotine, diphenyltetramide, and hippuric acid.

Class II. Organic bodies which evolve *half* the contained nitrogen in the form of ammonia: morphine, codeine, strychnine, methyl-strychnine (iodide), papaverine, brucine, quinine (sulphate), cinchonine (sulphate), nicotine, naphthylamine, toluidine, and rosaniline (acetate).

Class III. Creatine, which gives off *one-third* of its contained nitrogen in the form of ammonia upon distillation with the alkaline permanganate, is conceived to contain the remaining two-thirds in the form of urea, which by previous experiment was found to give only nitrogen and nitric acid. It is, therefore, concluded that sarcosine, which, together with the elements of urea, make up the original creatine, will furnish the whole of its nitrogen as ammonia,—a supposition which awaits the confirmation of direct trial.

Class IV. Theine, gives off *one-fourth* of the nitrogen.

Class V. includes bodies which evolve various proportions of nitrogen in the form of ammonia. 100 parts of uric acid give about 7 parts of ammonia; caseine 7·6 parts, dry albumen about 10 parts, and gelatine 12·7 parts of ammonia. Picric acid, as the type of a nitro-compound, gives no ammonia on treatment with alkaline permanganate.

The authors finally conclude that nitro-nitrogen does not give ammonia; that amidogen, imidogen, and perhaps nitrogen, are evolved in the form of ammonia from organic bodies derived from marsh gas and its homologues. Compounds derived from hydrocarbons below marsh gas do not give up the whole of the nitrogen as ammonia. The anomalous results obtained in the case of urea are explained by the circumstance, that no oxidation of this substance is possible without destruction of the amidogen. The authors reserve the full consideration of the residual nitrogen (in cases where there is any), and of other complementary products of the oxidation.

A short discussion then took place, having reference to the means of purification of distilled water and the application of the Nessler test, in which Messrs. Dugald Campbell, Wanklyn, Chapman, and Thorp took part.

The President exhibited some interesting examples of phosphorescent salts, arranged in series so as to imitate

the colours in the solar spectrum. A butterfly also, with gorgeous wings extended, was constructed by placing the various salts in patches against a glass plate. These illustrations were the work of M. Gaiffe, and were said to have been prepared from the sulphates of barium, calcium, &c., reduced by heating with carbon to the state of sulphides. To start the phosphorescent activity of these salts the frames were exposed to the intense light given out during the combustion of about six inches of magnesium ribbon. [The phenomenon was exhibited with good effect in the meeting-room after the gas-lights had been lowered.]

A "*Note on Dr. Frankland's Process of Water Analysis*" was read by Mr. E. T. Chapman. The author calls in question the accuracy of the method lately proposed for the destruction of nitrates in natural waters, by evaporating with aqueous sulphurous acid, even when perchloride of iron or phosphoric acid is added to the portion of liquid under treatment. The The objections raised were, firstly, that the decomposition was incomplete ; and, secondly, that the sulphuric acid formed, or other free acids present in solution, would decompose a portion of the organic matter and entail a loss of carbon.

Mr. CHAPMAN then proceeded to read a "*Note on the Estimation of Nitric Acid in Potable Waters.*" This analytical method depends upon the reduction of the nitric acid to ammonia by the action of nascent hydrogen —a condition practically ensured by distilling the water with pure caustic soda, to expel, in the first instance, any ready-formed ammonia, then cooling the contents of the retort and introducing aluminium foil, which, during solution, effects the reduction of the nitrates to ammonia. After standing for several hours heat is again applied, and the distillation proceeded with until no more ammonia comes over. The amount of the latter is ascertained either by the Nessler test, or by the method of titration with standard acid. The possible occurrence of nitrates in the caustic soda employed is to be guarded against, and some special precautions are taken for the purpose of avoiding loss of ammonia by diffusion of air into the apparatus.

Dr. WILLIAMSON inquired whether, in proceeding to effect the destruction of nitrates by sulphurous acid in the presence of proto-salts of iron, Mr. Chapman had exactly adhered to the proportions of these re-agents prescribed by Dr. Frankland ?

Mr. CHAPMAN said he had used even double the quantity of sulphurous acid without completely decomposing the nitric acid.

Dr. ODLING, in rising, said that he was not about to offer any testimony as to the truth or inaccuracy of either of the proposed methods of determining nitric acid ; that point would be ascertained, not by discussion, but by patient investigation. It appeared to him possible that both methods of operating would give successful results if all the prescribed conditions were fulfilled. On referring to Dr. Frankland's published paper, which he held in his hand, he found recorded a specific experiment in which a water containing nitrate was treated with sulphurous acid, evaporated to dryness in vacuo, and the residue afterwards burnt. As the result of the combustion, no trace of nitrogen was obtained, and this evidence seemed to be perfectly conclusive. He admitted that apparently trifling modifications in the mode of conducting an experiment might sometimes influence the result, and this showed the necessity of thoroughly working out a process before submitting it to public criticism. For his own part he could not help saying that the new methods lately proposed by Messrs. Wanklyn and Chapman would have met with a more welcome reception at the hands of chemists, if the results, instead of being published piece-meal, had been kept back until a complete and thoroughly verified system of analysis had been worked out.

Professor WANKLYN repudiated the accusation of having brought forward his method of analysis prematurely, by asserting that all the details published in June last had been confirmed by evidence since obtained. He used now, instead of a litre of water, only half a litre for the distillation, and in other trifling respects had improved the process. He would, however, insist upon the truth of the leading axiom, that the amount of ammonia obtained was always proportional to the "badness" of the water, and the method was, therefore, in all cases, applicable.

Professor ABEL said that, upon the first appearance of Messrs. Wanklyn and Chapman's method of analysis, he had felt it his duty to inquire into the applicability of the new process ; but he soon found that modifications were to be introduced, and that statements made in one paper were contradicted in the next. He had failed in arriving at such definite results as would enable him to adopt the process in its present form, and he entirely agreed with Dr. Odling in considering that the authors had laid themselves open to the accusation of having published their paper somewhat prematurely.

After a few words from Mr. CHAPMAN, the President invited Mr. W. H. PERKIN to read his paper "*On the Hydride of Aceto-Salicyl.*" In the present communication, the author gives a fuller account of the hydride of aceto-salicyl, the formation of which was mentioned in a previous paper when treating of the artificial production of coumarine. The body in question was prepared by acting with acetic anhydride upon the hydride of sodium-salicyl in the state of fine powder suspended in pure ether. After twenty-four hours' contact the etherial fluid was evaporated, and furnished a white satin-like crystalline mass upon cooling. It is possessed of aldehydic properties, and has the following composition :—

$$C_9H_8O_3 = \left\{ \begin{matrix} COH \\ C_6H_4 \\ C_2H_3O \end{matrix} \right\} O.$$

By digesting this product with more of the acetic anhydride, another crystalline body is formed, which fuses at 100°—101°C., and contains the elements of the two substances which give rise to its production. Its formula is then—

$$C_{13}H_{14}O_6 = C_9H_8O_3, C_4H_6O_3.$$

The rest of Mr. Perkin's paper is devoted to an account of the formation of coumarine, from which it appears that this body is only formed when acetate of sodium is present, together with the other ingredients used in its production ; this apparent anomaly is explained by assuming the formation, in the first instance, of the sodium-salt corresponding to Gerhardt's "anhydrous biacetate of potassium."

A paper "*On the Absorption of Vapours by Charcoal*," by John Hunter, M.A., of Queen's College, Belfast, was read by the Secretary. This communication resumes the subject of a previous experimental enquiry, and describes the amount of mixed vapours, as well as of a number of new substances in a state of vapour absorbed by a known volume of cocoa-nut charcoal, under varying circumstances of temperature and pressure, Among the substances examined were the following:—Ethylamine, iodide of ethyl, acetate of methyl, oxalic ether, hydride of salicyl, salicylic acid, iodide of amyl, naphthaline, camphor, nitro-benzol, bisulphide of carbon, alcohol, acetone, and methylic alcohol.

The next paper read was "*On the Occurrence of Prismatic Arsenious Acid*," by Mr. FREDERICK CLAUDET. The dimorphism of arsenious acid was discovered by Wöhler, who found in the flue of a reverberatory furnace crystals of this substance, produced by sublimation, which were not octohedral. The author exhibited a fine sample of a product naturally formed in fissures of an arsenical pyrites ore, occurring in the San Domingos mines, Portugal. This proved on analysis to be pure arsenious

acid in the form of thin plates, similar in appearance and structure to the mineral selenite, and having a beautiful pearly lustre. The specific gravity was found to be 3·85, and hardness 2·5. An analysis of the ore is given, from which it appears that arsenic occurs to the extent of 0·47 per cent, and copper about 3 per cent, with a host of other metals in smaller quantity, including a trace of thallium. There are indications in the mine of a slow process of oxidation having been going on for many years, and the mineral from which the arsenious acid was taken remained quite hot to the touch after it had been raised from the workings. The substance in question is believed to be the result of a very slow process of sublimation, and the form is probably modified by the sulphurous atmosphere pervading those parts of the mine.

Professor Warington Smyth, having been invited to speak, said he was not conversant with the locality where these crystals were found, but from the full description given by the author he thought it probable that his speculations regarding the origin of the substance, were correct. It was interesting as furnishing evidence of the dimorphism of arsenious acid, since two kinds of oxide of antimony were previously known.

The next paper was by Dr. Stenhouse, on the "*Action of Nitric Acid on Picramic Acid.*" The author reconciles the conflicting statements regarding the nature of the products of this action by showing that, according to the degrees of concentration of the acid employed and temperature reached, the products are a mixture in variable proportions of picric acid, and the diazodinitrophenol of Greiss. When much nitrous fume is evolved the latter substance is the principal product.

An important communication "*On Chloranil.*—Part I.," by the same author, was then read. In following the best method described for the preparation of chloranil from phenol, that of A. W. Hofmann,—the substance is never produced in the theoretical proportion, but by superadding a treatment with chloride of iodine, the red oil and terchlorquinone become almost completely converted into chloranil, which can then be purified by solution and crystallisation from warm benzol. The action of hydrochloric acid and chlorate of potassium upon picric acid gave but a small proportion,—one-eighth of the theoretical quantity—of chloranil, the chief product being chloropicrin. For the preparation of Städeler's chlorhydranil Dr. Stenhouse directs that the chloranil should be treated with moderately strong hydriodic acid, and about one-tenth by weight of ordinary phosphorus for about half an hour, the product being washed with cold water, and crystallised from boiling alcohol. By the action of sulphurous acid on chloranil suspended in boiling water, the author remarked that other products besides chlorhydranil were formed. Free acids (sulphuric and hydrochloric) remained in solution, which were separated or neutralised with carbonate of lead, and sulphuretted hydrogen passed through the liquid precipitated the heavy metal, and left still in solution an organic compound, which was recovered by evaporation to dryness and sublimation from a paraffin bath heated to 120° C. Lustrous crystals were thus obtained which proved on analysis to be terchlorhydroquinone, $C_6Cl_3H_3O_2$. This treated with nitric acid furnished terchlorquinone $C_6Cl_3HO_2$. A modification of this process will, it is expected, give the bichlor- and chlor-quinone in a state of purity. The author concludes with some observations on terchlorquinone and the bromo-derivative, which, not yet analysed, is believed to be $C_6Cl_3BrO_2$.

Time did not permit of the reading of the next paper, but Mr. Chapman made a short verbal statement of its contents. It is entitled, "*Action of Zinc Ethyl on Nitrous and Nitric Ethers*," by E. T. Chapman and Miles H. Smith. The authors compared the action of zinc ethyl to that of metals, and show that when operating upon nitrites the residual products are nitrogen, nitric oxide, or the intermediate oxide N_2O. When acting with sodium upon nitrite of amyl a black substance is formed, which has but a transitory existence, and is believed to be the nitride of sodium, NNa_3. Undiluted zinc-ethyl attacks nitrite of amyl with great violence, bursting into flame when placed in contact with it, but if previously diluted with ether its action may be controlled, and gives rise to the production of nitric oxide gas and a honey-like solid mass believed to have the following composition :—

$$\left.\begin{matrix} Zn C_2H_5 \\ C_5H_{11} \end{matrix}\right\} O + \left.\begin{matrix} C_2H_5 \\ C_5H_{11} \end{matrix}\right\} O.$$

This decomposed by water gives hydrated oxide of zinc, hydride of ethyl, and amylic alcohol. If the zinc ethyl be used in excess, there is formation of Frankland's dinitro-ethylate of zinc. In proof of this the barium, and subsequently the copper salt, were prepared and analysed. When only a small proportion of ether was employed to moderate the action, the following change occurred :—

$$N''' \left\{\begin{matrix} O'' \\ OC_5H_{11} \end{matrix}\right. + 3\left(\frac{Zn}{2} C_2H_5\right) = \frac{ZnO}{\frac{Zn}{2}OC_5H_{11}} + N(C_2H_5)_3.$$

The triethylamine was recognised by its power of neutralising acids, and of giving by limited oxidation only acetic acid, with consumption of the proper amount of oxygen. The reaction between pure zinc ethyl and nitrite of amyl was characterised as giving a mixture, which, when heated only to 40°C., suddenly exploded with a degree of violence unsurpassed by any fulminating compound of which the authors had previous experience.

The Secretary gave notice that on the next occasion, 19th instant, Mr. Chance will deliver a lecture "*On the Manufacture of Glass ;*" and that Professor Kolbe, who will then be in London, will give a short account of "*The Direct Transformation of Carbonate of Ammonia into Urea.*"

A vote of thanks was passed to the authors of the several communications read, and at an unusually late hour the meeting was adjourned.

ROYAL SOCIETY OF EDINBURGH.

Relation of the Chemical Constitution and Physiological Action of Medicine.—Addition of Iodide of Methyl to Vegetable Alkaloids.

At one of the recent meetings of the Royal Society of Edinburgh, a very interesting paper was read by Drs. Crum Brown and T. R. Fraser, upon the influence of direct chemical addition upon the physiological action of substances. This paper is the first of a series which may be expected to throw great light upon one of the most interesting questions which can suggest themselves; viz., the relation existing between the chemical constitution and the physiological action of medicinal and poisonous substances. That such a relation must exist, we can have no doubt; and, indeed, attempts have been made by some to establish the relation in certain cases. Hitherto, however, the subject has not received that systematic investigation which it is now receiving at the hands of the authors of the paper.

In order to arrive at any accurate knowledge as to the influence which chemical constitution exerts upon physiological action, it would appear to be desirable to take substances having a very definite and energetic physiological action, and then to perform upon them a chemical operation, having for its object the promotion of a definite change in the constitution, and to examine the modification which the physiological action has undergone. Such has been the plan which the authors have pursued; the bodies which they have chosen for examination are the more

active of the vegetable alkaloids, and the chemical operation, of which they have studied the effect, has been the direct addition of iodide of methyl. It was shown by How that, when iodide of methyl acts upon strychnia, brucia, morphia, and other alkaloids, it adds itself to them, and beautiful crystalline bodies are produced which differ considerably in character from the salts of the alkaloids. The authors have already examined the physiological action of the bodies produced by the addition of iodide of methyl to strychnia, brucia, morphia, thebaia, codeia, and nicotia.

The iodide of methyl-strychnium is prepared by first treating finely pulverised strychnia with a solution of carbonate of potash in dilute alcohol, and then adding an excess of iodide of methyl mixed with about its own volume of rectified spirit, and digesting in a flask for twenty-four hours. The spirit is thereafter distilled off, the residue dissolved in water, and crystallised. It is well known that doses of strychnia, varying from one-twentieth to one-thirtieth of a grain, rapidly produce in rabbits most violent convulsions, and in a few minutes kill the animal; the phenomena produced being due to a localisation of its action on the cord. It was found that twelve grains of iodide of methyl-strychnium, when administered (by subcutaneous injection) to rabbits weighing three pounds, produced no effect whatever. Fifteen grains produced symptoms, and twenty killed; but the animal died with symptoms altogether different from those produced by strychnia. In place of violent and spasmodic convulsions and muscular rigidity, the appearances were those of paralysis with complete general flaccidity. The spinal motor nerves were either paralysed, or speedily became so; and, instead of the speedy occurrence of muscular rigidity, the muscles remained flaccid, contractile, and alkaline for several hours. In short, by the addition of iodide of methyl to strychnia, the toxic properties of the latter are diminished about 140 times; and the body produced possesses the physiological action of curare; viz., paralysis of the end-organs of the motor nerves.

Similarly, Brown and Fraser have discovered that the toxic properties of brucia, thebaia, and codeia are immensely diminished by the addition of methyl; and that the bodies produced, instead of being, as all three of these alkaloids are, strongly convulsent, possess, on the contrary, the physiological action of curare. Morphia, as is well known, possesses both soporific and convulsent properties; its toxic action is much diminished by the addition of iodide of methyl; its convulsent action is destroyed, but its soporific action remains. The above are amongst the chief results which have been obtained by the authors, and appeared to possess such interest as to warrant my drawing the attention of your readers to them.—*British Medical Journal.*

FOREIGN SCIENCE.

PARIS, MARCH 10, 1868.

ACADEMY OF SCIENCES:—Death of M. Foucault.—Products of the distillation of beet-root.—Oxygenated water, not the cause of colouration in ozone test-papers.—Employment of salts of potash in agriculture.—Examination of flour.

On the 17th of February the Academy was informed, by the President, of the loss it had sustained in the death of two distinguished savants, Sir David Brewster and M. Leon Foucault.

M. Pasteur presented a pamphlet, entitled "A Study of Vinegar: its fabrication, accidents, and the means of preventing them. New observations upon the preservation of wines by subjecting them to heat."

MM. Pierre and Puchot presented a memoir, entitled "Experimental researches on the products of the distillation of beet root."

M. Chacornac addressed a note concerning the intimate constitution of light and the formation of nebulæ.

M. Phipson sent a note on some luminous phenomena which accompany meteoric showers.

M. Houzeau sent a note on oxygenated water considered as not being the cause of the alterations induced by the atmosphere in litmus, and iodide of potassium and starch paper as ozone tests.

Experimental researches on the employment of salts of potash in agriculture was the subject of a note by M. Deherain; and there was a note on the qualitative and quantitative examination of rye flour; and of alcoholic liquids by means of chloroform, from M. Rakowitsch. These are all the communications relating to chemistry brought before the Academy at this *séance*; there were several physiological papers, among others one upon the nature of vaccine virus, which elicited considerable discussion.

The memoir of MM. Pierre and Puchot, communicated by M. Wurtz, is an account of a research which has occupied them more than three years. It relates, as has already been mentioned, to the products of the distillation of beet root. When the ordinary rectification of the fermented liquor is observed with attention, a disagreeable penetrating odour is perceived in the first portions which come over. These products often cause liquids with which they are mixed to colour spontaneously. The disagreeable odour of the products manifests itself a considerable time, to the annoyance of the distillers, since they are obliged to keep the portion contaminated separate from the alcohol of good flavour which follows, and sell it at a reduced price. MM. Pierre and Puchot have observed the presence of a vinic aldehyd, of which they have separated, though not without considerable trouble, several litres. The aldehyd thus separated, boils a little below 22°; a specimen made three months ago is still perfectly clear.

If, towards the end of the distillation, a sample be collected and examined, the odour of amylic alcohol will be perceived; at the end of the operation, this alcohol distils over almost free from other alcoholic products. Examined more closely at this stage, the distillate is found to contain other definite substances, such as butylic and propylic alcohols. To test the nature and purity of these two alcohols, MM. Pierre and Puchot have prepared the corresponding iodides and acetates. The butylic alcohol boiled at 107·5°, its iodide at 122·5°, its acetate (isomeric with ethylic butyrate) at about 116°. The propylic alcohol boiled at about 98·5°, its iodide about 104·5°, and its acetate (isomeric with methylic butyrate) about 105°. In conclusion, they remark upon the deceitful nature of the appearances manifested when investigations of bodies such as they have been working upon are made with restricted quantities of material. It is not easy, they say, to distinguish always mixtures possessed of a relative stability from definite and true compounds. Elementary analysis, too, cannot always solve the point, and many examples are cited to show how mixtures of two alcohols may yield the same percentage results as a third pure alcohol.

Some of the other memoirs deserve more than a passing notice, and your correspondent will introduce accounts of them hereafter.

M. Dehérain has arrived at a certain number of conclusions regarding the employment of salts of potash in the cultivation of wheat, potatoes, and beet-root. He finds the salts of potash have generally augmented the wheat crops, which have been augmented still more when ammoniacal salts and phosphatic manure have been also added. Pure potash manures have not increased potato crops; when ammoniacal salts and phosphates have been added as well, a slightly greater yield has been obtained, but not sufficiently to make the employment of these manurial agents profitable. In the cultivation of the beet-root the facts are precisely the same.

The experiments upon which these conclusions are based were made upon a large scale in a part of the domain of l'Ecole de Grignon.

M. Rakowitsch proposes a method of examining flour by means of chloroform. The following are the results which he says may be gathered from an experiment capable of being made in a few minutes :—The amounts of bran, the moisture between 10 and 25 per cent, the damaged flour, the mineral matters, the ergot of rye, and other impurities. The whole of these are determined by the relative specific gravities of the different substances in chloroform. The flour is simply placed in a tube and mixed with chloroform ; the chloroform is enabled to hold in very thorough suspension the pure flour, while the other materials are not thus suspended. By adding spirits of wine of 95°, the flour is precipitated to the bottom of the tube. The more humid the flour, the more spirits of wine must be added, and thus the amount of humidity in the flour is arrived at.

NOTICES OF BOOKS.

Boiler Deposits : On the Chemical Nature of the various kinds of Boiler Incrustations, and on some of the methods which have been proposed to remedy them. By Dr. T. L. Phipson.

In the form of a small pamphlet, the author reproduces the text of a paper read at a meeting of the Inventor's Institute in December last. The chemical nature and circumstances of formation of boiler deposits are fairly discussed, several proposed means of prevention described, and a few practical remarks appended which are likely to prove useful to engineers. In the manufacturing districts of Lancashire and of the midland counties many disasters must have been prevented by the system of periodical inspection carried out during the last few years by the so-called Boiler Associations ; but much yet remains to be done towards extending their operation in new localities, and particularly in order to guarantee the safe working of boilers attached to portable engines, which are now so commonly placed in the hands of agriculturalists. The greatly extended use of small steam lifts and cranes, and of boilers constructed around the flues of forges and reheating furnaces, appears likewise to increase the risk of danger from explosion—and many sad calamities have been reported during the past year. Apart from the question of personal safety, there is a wide margin for the exercise of economy in the working of a steam boiler, and Dr. Phipson assures us that the loss of heat, or of combustible, in a thickly-incrusted marine boiler, may amount to as much as 40 per cent ! In all cases where an adherent mineral scale is permitted to form, the consumption of fuel for all constructions of boilers must necessarily be somewhat augmented, and the operation of chipping tends certainly to loosen the rivets and weaken the plates.

The composition of boiler deposits is considered under three heads, viz. :—Fresh water deposits consisting chiefly of carbonate of lime ; salt water deposits invariably composed of a mixture of sulphate of lime and hydrate of magnesia ; and abnormal deposits containing incrusting matters derived from mine waters, canals, or streams contaminated with metallic salts and other kinds of refuse from chemical works. Impure waters of this class are known to be especially liable to induce corrosion of the boiler-plates, and a red deposit analysed by Dr. Phipson contained more than 9 per cent of peroxide of iron, due to the rusting of the metallic surfaces of the boiler. Two years ago Dr. Voelcker* gave an account of marine-boiler incrustation, and called attention to the large amount of magnesia contained in it. Dr. Phipson now appends a further analysis which, from its interest, is here subjoined :—

* Report of the British Association, 1865 ; p. 39.

Marine Boiler Incrustration.

Sulphate of Lime	65·00
Magnesia	19·00
Water	13·50
Oxide of Iron and Alumina	0·85
Chloride of Sodium	0·70
Sand	0·45
	99·50

The author proceeds to remark—"This analysis shows that marine incrustation consists chemically of an atom of sulphate of lime united to two atoms of hydrate of magnesia ; and that the sulphate of lime is combined here with *one* atom of water, instead of two as in ordinary gypsum." This statement is not, however, in harmony with his own results (quoted above), which demand, on the contrary, the following formula :—

$$2(MgO,HO)+HO,2CaOSO_3.$$

It will be perceived that *dihydrated* sulphate of lime—a compound described by the late Professor Johnston under the name of "boiler sulphate,"—is really deposited, together with the hydrate of magnesia, at the increased temperature of water boiling under considerable pressure.

Dr. Phipson then enumerates several of the ingredients which have been proposed as means of prevention. Of boiler compositions which act only mechanically, the author mentions scrap-iron, clay, starch, and potato-parings. A varnish for coating the interior of boilers, is compound of equal parts of black-lead and suet, with small quantities of charcoal-dust and coal-tar oil. As an objection to this lacquer, it is said to require frequent renewal. Professor Chandelon, of Liège, recommends a mixture of—

Bullock's blood	5 parts.
Starch	2 ,,
Carbonate of soda	2 ,,

The use of sal-ammoniac is condemned on account of its rusting the plates. The most efficacious remedies are the carbonate and hydrate of soda, and the author proposes to feed the boiler with water from which the lime and magnesia salts have been previously precipitated by addition of alkali to the contents of the supply tank. Dr. Phipson calculates that 24 grs. of the carbonate of soda, or 14 grs. of the hydrate, should be allowed for every gallon of water, and now that a sufficiently pure quality of the hydrate can be procured for 3d. per lb. (misprinted 3s.) the use of soda is recommended as being both effectual and economical with all kinds of fresh water ; but for marine boilers so large a quantity would be required, that the author prefers to combine this alkaline treatment with the system of frequent "blowing off," whereby much of the calcareous refuse can be ejected in a sandy or granular form.

CORRESPONDENCE.

NESSLER'S TEST.

To the Editor of the Chemical News.

Sir,—I have used the following modification of Nessler's process for the estimation of small quantities of ammonia, by which considerable expedition is gained without, as far as I am aware, sacrificing accuracy. It is based on the fact that a solution of iodine, dissolved in iodide of potassium, imparts to water a colouration very similar to that produced by the Nessler test with ammonia. Where the quantity of ammonia was small, say ·4 mlgrm. in 200 c.c. of water, I could perceive no difference in looking *down* the two cylinders standing on a white surface ; the real difference, however, becomes quite apparent if viewed laterally. I conduct the process as follows :—A per-

manganate burette, divided into ·1 of c.c., and fitted with a float, is filled with a solution of iodine dissolved in excess of iodide of potassium; the strength is best determined by experiment.

Into one of the cylinders is put an amount equal to ·4 mlgrm. of NH_3, together with 200 c.c. of water, and the Nessler test, taking care to mix thoroughly. As soon as sufficient time has elapsed to allow of the colouration attaining its darkest shade, a second cylinder, containing a volume of water equal to that in the first, and to which has been added a little solution of iodide of potassium (1 to 10 of water) is placed under the burette, from which is delivered the solution of iodine, until a point occurs when, on looking *down* the cylinders, the shades of colour in each are judged to be alike. From this it follows that, provided the "tinctorial" power, and consequently "titre" of the ammonia standard be correctly ascertained in terms of the iodine solution, the former may be dispensed with.

This modification, it will be observed, obviates the numerous assays it is necessary to make when the ammonia standard is retained, besides saving no inconsiderable time in the execution of an analysis.—I am, &c.

PHILIP HOLLAND.

Chorley.

WATER ANALYSIS AT THE CHEMICAL SOCIETY.

To the Editor of the Chemical News.

SIR,—During the discussion at the last meeting of the Chemical Society, Mr. Abel, the chemist to the War Department, took occasion to remark, in reference to our method of water analysis, that our paper describing it contained many things that were incorrect, and on my asking him to be kind enough to name some of those things that were incorrect, I could get no answer from him.

* * * * * *

I have made an examination of our paper, and find absolutely nothing which I can say is incorrect; the most that I find is a single expression in a parenthesis, which appears to be doubtful. We have used the word "usually" instead of "sometimes" in this parenthesis. The paper is distinguished by extreme accuracy and fidelity to the facts, but is in parts unartistic, and gives an undue degree of prominence to certain results. Owing to this circumstance, notwithstanding its accuracy, it is calculated to lead those persons into error who glance at a paper without reading it.

There is much misconception current respecting our method—far greater misconception than could possibly have arisen from this cause. Indeed, it seems as if the current notions about it have been derived, not from our paper describing it, but second-hand from the writings of our detractors. A *bon mot*, by a Fellow of the Chemical Society when our method was brought out, will serve to characterise much of the causes which have produced these misconceptions:—"The worst fault of your water-analysis," said this gentleman, "is that it proceeds from you."

One of the most curious of these misconceptions is that we direct people to determine the urea in a water by taking a litre of it, adding carbonate of soda and distilling off 300 c.c., and then measuring the ammonia in the 300 c.c. of distillate.

Our directions given last June were, "go on distilling until no more ammonia can be detected in the distillate." So far from, in any way, leading people to believe that the first 300 c.c. will contain all the ammonia proceeding from the urea, we warn people against expecting to find it all there; we talk about the necessity arising for the addition of ammonia-free water, so as to fill up the retort, and finally we give examples where the urea-distillate reaches 600 c.c.

I understand our detractors to say that our process for the estimation of urea does not give the whole of the ammonia derived from it, but only a fraction of it. Such a notion is intelligible enough in conjunction with the belief that our method consists in seeking for urea-ammonia only in the first 300 c.c. of distillate. I understand that some of them are anxious to construe the remark made in my paper on "the verification, &c.," that the estimation of urea is not so sharp as the estimation of albumen, into a round-about way of saying that the decomposition of urea into ammonia is incomplete. Let them be undeceived. There is no failure of the reaction. The want of sharpness in the estimation is due only to a manipulatory difficulty, which might be got over if necessary. It arises from the division of the ammonia into too many portions, and the consequent accumulation of experimental errors.

Those gentlemen to whom the *bon mot* is applicable, are jubilant over the fate of the determination of albumen. Say they, you promised us a complete conversion of the nitrogen of albumen into ammonia, and you give us some kind of an incomplete transformation. We have given them everything we promised, and much more too. If our detractors will not merely glance at our paper, but read it carefully, they will find a warning that the first third of the ammonia derived from albumen is only to be got when the potash treatment is pushed to dryness, and after getting that first third in that manner, the remaining two-thirds are to be had on using the permanganate. It was in that manner that the total ammonia was got from albumen.

In our paper, however, we say in effect, if you have to make a water analysis do not distil to dryness. When we wrote our paper last June, it was for us an undecided point whether or not the neglect to evolve the first third of ammonia by means of potash would involve the ultimate loss of it—whether, in fact, there would be more ammonia on the employment of the permanganate to make up for absence of ammonia during an imperfect potash-treatment.

This question, although interesting on scientific grounds, and the answer to which will go a long way towards the disclosure of the rational constitution of albumen, was in my opinion of comparatively little consequence in reference to water analysis, and we adjourned the consideration of it to a more convenient season. In our opinion it did not matter at all whether the modification we recommended on the score of practicability involved the representation of albumen by a fraction instead of the whole of its ammonia. The essentials for water analysis were that the method should really cope with the infinitesimal quantities of organic matter existing in water, and that its indications should be reasonably parallel with the degree of badness of the water, and that it should be practicable. The possession of these qualities is characteristic of our method, and will, we believe, secure its general acceptance. The absence of these qualities is one of the most prominent features of the method which at the present moment is the rival of our own.—I am, &c.

J. ALFRED WANKLYN.

London Institution, March 9, 1868.

ON THE REDUCTION OF CARBONIC ACID TO OXALIC ACID.

To the Editor of the Chemical News.

SIR,—It is due to ourselves, as well as to the chemical public, to throw some light on our relations to a discovery which is claimed by a chemist who worked with us in the same laboratory. It is the synthesis of oxalic acid from carbonic acid and potassium, a fact which is decidedly of great interest, as it shows the direct transformation of carbonic into oxalic acid, and is a new proof of the truth of the views which reign at present on the constitution of the latter body.

Occupied during the course of last summer with similar synthetical experiments, we were, by a very simple reflection, led to the idea of preparing directly oxalic acid from carbonic acid, an idea the execution of which was clearly pointed out by the successful experiments of Mr. Wanklyn on the formation of propionic acid.

The reflection that the group CO,HO, common to all organic acids, and already prepared synthetically in combination with alcohol radicals, could exist independently as a molecule, only doubled, that is to say, as oxalate of potassium, leads, consequently, to the synthesis of the acid.

We began our experiments in the Liepsic Laboratory, in presence of a considerable number of chemists, who took a lively interest in the proceedings of our research. Amongst them we observed Dr. Drechsel.

The first we tried was the action of liquid carbonic acid on potassium. We were not disturbed by the observation made by Dr. Drechsel on this occasion, that an English chemist had tried already the same experiment (executed only in order to state the solubility of potassium in liquid carbonic acid), without mentioning the formation of oxalic acid, for which he evidently was not looking.

The experimental difficulties, namely, the execution of the reaction in a Faraday tube, as well as the close of the laboratory by the holidays, obliged us to interrupt our research, the continuation of which was clearly indicated at the occasion of the lecture experiments on liquid carbonic acid with Natterer's apparatus in the course of the following winter.

It is easily explained that our experiments in that direction escaped the memory of our highly esteemed teacher, Professor Kolbe, who at this time was fully occupied by the new building of his laboratory, and by other official occupations, so that he, half a year afterwards, proposed to Dr. Drechsel the same research which had led us, six months ago, quite independently, without any impulse, to the above-mentioned experiments.

* * * * * *

We are, &c., J. Wischin and Th. Wilm.

Berlin, March 8th, 1868.

THE ROYAL SCHOOL OF MINES.

To the Editor of the Chemical News.

Sir.—"A student at the Royal School of Mines" states in the Chemical News of last week, that I am in error when I look upon the Royal School of Mines as an appendage of the Geological Museum (when I say the Geological Museum, I include the Geological Survey). Notwithstanding what he has said, I still think that I am right in so regarding it. To support me in this opinion I will for a moment refer to the "pamphlet." On page 3, paragraph 1, we are told that the Museum and the School have arisen from the wants of the Geological Survey; this is substantially repeated in paragraph 5; and in the following paragraph we are distinctly told that the School "has grown out of, or engrafted itself upon, the Geological Survey." So that the Geological Survey is the trunk, and the School is one of its branches, and therefore an appendage.

I perhaps made use of too strong an expression when I said that the prospectus was printed on the spare pages of a pamphlet belonging to the Geological Museum; what I meant was that the School does not occupy the position of primary importance in it which it should; the accounts of the Geological Survey, of the Museum, and of the Mining Record Office, all take precedence of it; and this may be seen from a glance at the title-page,—thus, "Geological Survey of the United Kingdom, Museum of Practical Geology, and Royal School of Mines," whereas those departments, being quite distinct from the School, should have a prospectus of their own, while the School should have its calendar printed in a separate form. The pamphlet is neither wholly a prospectus nor a calendar, it is nothing but an economical make-shift, attempting to perform the functions of both; advertisements of maps occupy one cover, whilst the "Arrangement of Lectures," as if of no importance, is ignominiously consigned to the inside of the other. I am rather surprised that a student of the Royal School of Mines should have raised a discussion about such a triviality as this is when compared with the general welfare and position of the School.

I cordially agree with "Delta" in his remarks respecting the Associates, although I must say that the term "Associate" is often used as an honorary one, and is generally regarded as such. I think that the excellent course of study now marked out might be somewhat improved by adding mathematics to the list of subjects; and it would be a great help to the students in the Geological department were the course to include botany; both these subjects are taught in the Dublin School. I may perhaps suggest that the certificate of proficiency would derive an additional value were they to be signed by the director, Sir Roderick Murchison, as well as by the Professor.—I am, &c. A. L. E.

MISCELLANEOUS.

Test for the Presence of a Free Acid.—Dissolve chloride of silver in just sufficient ammonia to make a clear solution. If a little of the test be added to ordinary spring water, the carbonic acid present in the latter will neutralise the ammonia and precipitate the chloride. The above forms a good lecture experiment, the test being a very delicate one.—Edwin Smith; *Nottingham, March* 5, 1868.

The Soiree of the Chemical Society.—On Wednesday evening the President and Mrs. Warren de la Rue gave an entertainment at Willis's rooms, which was one of the most successful ever known in connection with this Society. The rooms were filled with chemical and philosophical apparatus, and the walls were hung with pictures by Landseer, Turner, Gainsborough, &c., some excellent chromo-lithographs by Hanhart, and a fine collection of photographs. General Lefroy, R.A., sent some ancient fire-arms from the Rotunda collection. The War Office contributed an illustrative series of the Ordnance projectiles and rockets now used by the service. A specimen of meteoric iron from Cranbourne, Australia, with the surface etched by acid to show the structure, and a remarkable example of the formation of blisters, due to the evolution of hydrogen beneath the lead coating of Armstrong shot; an illustration of metal-work cut with the band-saw, as used in the Royal Carriage Department, Woolwich; an Abyssinian 7-pounder double shell, whole and in section, with bursting charge of 1 lb. of powder; and an explosive shell, made to resemble a piece of coal, used on board the blockade-runners to effect the destruction of the boiler, when the vessel was about to be captured by the Americans, were kindly lent by Professor Abel, F.R.S., Chemist of the War Department. Dr. Hugo Müller, F.R.S., and Dr. Warren De la Rue, F.R.S., exhibited two hundred cells of their chloride of silver battery; Mr. Ladd exhibited the electric light from it, and its power of decomposing water. Messrs. J. J. Griffin and Sons showed Dr, Russell's apparatus for gas analysis, Tate's air pump, Leslie's freezing apparatus, and their hot-air bath. A large series of metallurgical preparations, consisting of oxides and salts of platinum, gold, silver, palladium, &c., by Messrs. Johnson and Matthey; some magnificent samples of thallium and its salts, by Messrs. Hopkin and Williams; a new electrical pyrometer, by C. W. Siemens; a polarising instrument, by Mr. J. Huggins; a colorimeter, and a number of other instruments, by Mr. W. Ladd, were all much admired. About 800 visitors were present, and the entertainment was continued till a late hour.

NOTES AND QUERIES.

Carbonic Acid.—Can either of your correspondents suggest any crude native, earthy, or metallic carbonate which parts with its carbonic acid, pure and undecomposed, at a moderate red heat?—G.

Prussian Blue Paste.—Would any of your kind correspondents inform me how the bronze is brought out on this article?—J. R. J.

Microscopic Detection of Flour.—Could you oblige by informing me if there is any, and what, method for detecting pea or bean flour when mixed with common wheat flour, except from the appearance of the starch granules under the microscope?—Q.

Estimation of Phosphoric Acid.—Will you oblige a subscriber to your journal, by informing him where he will find the best and simplest means for ascertaining the quantity of phosphate in artificial manure? Would it be too much to ask you to give the best method in your journal? I observe, in your number for February 28th, a valuable contribution from Mr. Burnard, but it is not sufficiently explicit. Our manure dealers tell us that we cannot judge of the quality of their manures by the soluble phosphate alone, as they contain also a considerable quantity of insoluble, or neutral phosphate. I do not care to know what other matters their manures contain, such as ammonia, soda, potash, &c., &c.; I simply want to know their phosphatic value.—AGRICOLA.

Estimation of Phosphoric Acid.—Reading in your last week's paper an article "On the Volumetric Estimation of Phosphoric Acid," by C. F. Burnard, F.C.S., I venture a few remarks upon this subject. The author uses the process of precipitating the phosphoric acid as phosphate of uranium by a solution of nitrate of uranium, and applies it principally to the analysis of phosphatic manures. Several attempts have been made to introduce the above-mentioned process, but they have failed until now, and principally because of the presence of iron in very nearly all manures. To determine the phosphoric acid in manures, a quantity of acetate of soda has to be added, since a precipitate of phosphate of uranium will only appear when the solution has been made acid by acetic acid. The iron present is precipitated as phosphate of iron. Had this precipitate the same composition in every case, the phosphoric acid in it could easily be determined; but this not being so, a more complicated method has to be adopted. I have tried to substitute citric or tartaric acid, to prevent the precipitation of phosphate of iron, but they unfortunately hinder also the precipitation of phosphate of uranium. Perhaps Mr. Burnard has found some more simple way to overcome this difficulty, and if so, I am sure that all chemists would be very glad to hear an account of it. —V. C.

Estimation of Phosphoric Acid.—Having seen some remarks in your last issue on the above subject by Mr. Charles F. F. Burnard, I should feel greatly obliged if he will inform me what is the reaction of the soluble phosphates with caustic soda, also the strength of his standard solution. On reading his article, it occurred to me that it might answer for the determination of $3CaO.PO_5$ in bone-ash and other commercial phosphates. I therefore prepared a normal solution of caustic soda. I then dissolved 10 grammes of bone-ash, which I knew to contain 75 per cent of $3CaO.PO_5$, in HCl, neutralised the free HCl, made up to 400 c.c., and proceeded with the standard solution of NaO, of which 50 c.c, were required to make the solution alkaline. Now 50 c.c. $= 50 \times \cdot031 = 1{\cdot}55$ grammes of NaO required for $3{\cdot}75$ grammes $3CaO.PO_5$, or 2 equiv. NaO are equal 1 equiv. of $3CaO.PO_5$, and not 3 equivalents NaO to correspond with the same number of equivalents of CaO in $3CaO.PO_5$.—J. J. K.

TO CORRESPONDENTS.

G. Kellog.—No report has yet been made on the alleged discovery It is not generally credited. Our neighbours do not hurry themselves.

Librum.—Dr. Marcet's is the most suitable of the books you name; then Dr. Hassall's.

H. Fisher.—Our publisher will write to you on the subject.

Amateur.—The method of cleaning and drying U-tubes is too simple to need description in our columns.

Communications have been received from Victor Cruse; J. Landauer (with enclosure); R. Beezley; Rev. Edwin Smith (with enclosure); H. Neale; W. Lea; Ernest Hart (with enclosure); Dr. Odling, F.R.S.; G. Kellogg; G. Wischin and Th. Wilm (with enclosure), W. Little (with enclosure); H. Sugg; C. Collins; A. Liversidge; H. McLeod (with enclosure); Dr. H. E. Roscoe, F.R.S. (with enclosure); H. Fisher; F. O. Ward; J. Samuelson; P. Spence: Messrs. Townsend and Adams, New York; Stephen Dowell; Capt. W. A. Ross (with enclosure); J. Blackburn (with enclosure); J. Apjohn; Foot & Co.; A. Payne; Prof. Wanklyn; Alex. S. Macrae; T. R. Fraser, M.D., Nova Scotia; Dr. Watts; S. Mellor; Niel Mathieson; T. Brown; Dr. W. Bird Herapath, F.R.S.; C. J. Woodward; J. Dobson (with enclosure).

Books Received.—"List of Chemicals and Drysalteries," from Messrs. E. Brooke and Co., Manchester, Glasgow, Bradford, and Huddersfield; "On Vanadium," by Henry E. Roscoe, B.A., F.R.S.; "On Faraday, as a Discoverer," by Professor Tyndall, LL.D., F.R.S.;" "Report of Medical Officer of Health for the Limehouse District;" "The Acadian Reporter;" "The Mining Gazette," Halifax, N.S.; "Le Moniteur Scientifique."

MEETINGS FOR THE WEEK.

MONDAY.—Medical, 8.
TUESDAY.—Royal Institution, 3. "On Historical Portraiture," Mr. G. Scharf.
WEDNESDAY.—Meteorogical, 8.
—— Society of Arts, 8.
—— London Institution, 6½.
THURSDAY.—Royal Institution 3. "On Historical Portraiture," Mr. G. Scharf.
—— Royal, 8½.
—— Zoological, 4.
—— Chemical, 8. "On the Artificial Production of Urea," Prof. Kolbe. "On the Manufacture of Glass," Mr. H. Chance.
—— Royal Society Club, 6.
FRIDAY.—Royal Institution, 8. "On Alloys and their Uses," Prof. Matthieson.
SATURDAY.—Royal Institution, 3. "On the Non-Metallic Elements," Professor Roscoe.

PATENTS.

Communicated by Mr. VAUGHAN, F.C.S., Patent Agent, 54, Chancery Lane, W.C.

GRANTS OF PROVISIONAL PROTECTION FOR SIX MONTHS.

3537. A. V. Newton, Chancery Lane, "An improved method of manufacturing cast steel and malleable iron."—A communication from E. L. Seymour, New York, U.S.A.—Petition recorded December 12, 1867.

3695. J. Jowett, Parkhead, Lanark, N.B., "Improvements in furnaces."—December 28, 1867.

3707. M. A. F. Mennons, Southampton Buildings, Chancery Lane, "The manufacture of a vegetable substitute for animal hair from a product not hitherto used for that purpose."—A communication from W. Staufen, Rue Auber, Paris.

3714. H. Bessemer, Cannon Street, London, "Improvements in the treatment of crude or cast iron, and in the manufacture of malleable iron and steel."—December 31, 1867.

28. J. T. Emmerson and J. Murgatroyd, Heaton Norris, Lancashire, "Improvements in the manufacture of iron, and in the application thereof to certain useful purposes."

33. W. H. Atkinson, Aldersgate Street, London, "Improvements in the preparation and use of compositions for cleansing and sweetening casks and other vessels, such composition being also applicable to other purposes."—January 4, 1868.

39. E. R. Southby, Lanark, N.B., "Improvements in separating paraffin from its solutions, and in apparatus therefor."—January 6, 1868.

Spiller's Boiler Fluid, for the Prevention and Removal of Calcareous Incrustation from Steam Boilers, has been extensively employed for nearly ten years past in the Royal Arsenal at Woolwich, the Government Powder Mill, at Waltham Abbey, besides being used successfully by Railway Companies and in many private establishments.

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"The book deserves a more extended review than our limited space affords. This, however, we think it right to add, our conviction, viz., —that the alkaline permanganates, as purifiers, disinfectants, and deodorisers, are superior to any others we are yet acquainted with, and that they are perfectly fitted to accomplish, under the direction of man, in his limited sphere of action, what ozone effects in nature."—*Brit. and For. Medico-Chir. Review.*

London: Robert Hardwicke, 192, Piccadilly.

THE CHEMICAL NEWS.

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ON

VANADIUM, ONE OF THE TRIVALENT GROUP OF ELEMENTS.*

By HENRY E. ROSCOE, B.A., F.R.S.

The metal vanadium (so-called from Vanadis, a cognomen of the Scandinavian goddess Freia) was discovered in 1830 by Sefström in the celebrated Swedish bar-iron made from the Taberg ore. From this source, even when using many pounds of the iron, Sefström obtained only minute quantities of the new substance, but he found it in somewhat larger amount in the slag or cinder produced in the reduction of the iron ore. Sefström ascertained some of the most peculiar characters of the substance, proved it to be a new element, and prepared some of its compounds in the pure state. The reactions by which vanadium can be separated and distinguished from all the other elements are: (1) The formation of a soluble sodium vanadate when the vanadium compounds are fused with sodium carbonate; (2) the formation of an insoluble ammonium vanadate when sal-ammoniac is added to the solution of a soluble vanadate; (3) the production of a splendid blue solution when this ammonium salt, dissolved in hydrochloric acid, is warmed with reducing agents such as oxalic acid.

Sefström not having leisure to prosecute the full examination of the properties of the new metal, handed over his preparations to Berzelius; and it is to the investigations of the great Swede (1831) that we owe almost all our acquaintance with the chemistry of vanadium.

Since Berzelius's time vanadium has been discovered in many minerals, of which a lead ore containing lead vanadate, and called by the mineralogists vanadinite, is the most important. It has also been found in many iron ores, in clay, bricks, and even in caustic soda. Still the quantity of the substance found in all these various sources has been extremely small; so much so, that the vanadium compounds murt be reckoned amongst the chemical rarities, and we find them quoted in the price lists of dealers in chemicals at 1*s.* 6*d.* per grain, or £35 per ounce! It is clear that our knowledge of the chemical properties of a substance so rare must necessarily be but incomplete, as the difficulties of obtaining exact or satisfactory results with small quantities of material are evident; and, in fact, the statements of the only persons who have worked upon the subject recently (Schafarik Czudnowicz), instead of giving us any more reliable information respecting the character of vanadium, have only served to throw doubt upon some of the conclusions of Berzelius, and thus to render our knowledge even less complete than it appeared to be.

Hence it was with much satisfaction that, in February, 1865, the speaker came into possession of a plentiful source of vanadium in a by-product obtained in the preparation of cobalt from the copper-bearing beds of the lower Keuper-sandstone of the Trias at Alderley Edge, in Cheshire. The manager of the works was puzzled to know why a blue solution, supposed by him to contain copper, did not deposit the red metal upon a strip of zinc; the speaker recognised this reaction as due to the presence of vanadium, and secured the whole of the by-product, which he found to contain about 2 per cent of the rare metal. The exact position of the vanadium mineral in the sandstone beds cannot now be stated, as the mine (at Mottram St. Andrews) from which the cobalt ore was obtained, is now closed, and cannot be entered. The general characters of the deposit are, however, well known, and exhibit points of great interest; they have been well described by Mr. Hull as follows:—

"The 'edge' or escarpment of Alderley rises from the eastern side of the plain of Cheshire gradually towards the east, but with a steep and abrupt ridge towards the north. This northern bank is richly wooded, and has a very beautiful aspect when viewed from a distance, as it contrasts strongly with the almost level plain which sweeps away to the northward and westward from its base. The ridge has here been upheaved along the line of a large fault, bearing east and west, throwing down at its base the Red Marl; and on the other side bringing up the soft sandstone of the Bunter, capped by a mural cliff of Lower Keuper conglomerate, which often breaks out in conspicuous masses through the foliage. The beds rise from the plain towards the east at an angle of about from 5° to 10°, and the escarpment is continued southward for some distance facing the east."

Succession of Beds in Descending Order.—(*Hull.*)

Red marl		Red & grey laminated marls.
Waterstones ..	Lower Keuper sandstone, 500 feet.	Brownish flaggy sandstones and marls.
Freestone		White and brown freestone.
Copper-bearing sandstone ..		Soft white, yellow, and variegated sandstone.
Conglomerate ..		Hard quartzose conglomerate, underlain by bands of Marl, forming the base of the Keuper sandstone.
Upper red and mottled sandstone	Bunter.	Soft fine-grained yellow and red sandstone, being the uppermost member of the Bunter sandstone.

The beds in the above series which claim the greatest share of our attention are those at the base of the Keuper series, for in these occur the copper and other minerals. The copper, as both blue and green carbonate, occurs disseminated throughout the sand, the ore coating the outside of the grains of sand and the pebbles of quartz. In addition to copper, bands containing lead both as carbonate and sulphide (galena) occur, also bands and veins of cobalt ochre, oxide of manganese, and iron ochre in workable quantity. The copper is extracted from the ore by solution in hydrochloric acid and precipitation as metal by scrap iron. The ordinary copper liquor, as well as the oxide of iron precipitated by lime from the solution of the chloride, does not contain any trace of vanadium, nor was the speaker able to detect any of this metal in the ore as at present worked.

Following, in the main, the process of preparation adopted by Sefström, the speaker obtained from the above-mentioned lime precipitate several pounds of pure ammonium vanadate, from which all the other compounds of vanadium can be prepared.

What now were the conclusions to which Berzelius arrived from his experiments concerning the constitution of the vanadium compounds? He corroborated Sefström's statement, that the most characteristic feature of the substance is the existence of an acid-forming oxide, termed vanadic acid, produced whenever any of the oxides are heated in the air. Berzelius also discovered two other oxides of vanadium, of which he ascertained the composition; and likewise a volatile chloride. To the highest oxide he gave the formula VO_3, to the second

* Read before the Royal Institution of Great Britain, Friday, February 14th, 1868.

VO_2, and to the lowest (or suboxide) VO; whilst the chloride was represented by VCl_3. The atomic weight of the metal he ascertained to be V = 68·5. Berzelius came to this conclusion from the following experimentally ascertained facts: (1) That on passing hydrogen over heated vanadic acid a constant loss of weight occurred, and the suboxide was formed; (2) that when dry chlorine is passed over the suboxide thus prepared, the volatile chloride was formed, and a residue of vanadic acid remained, which was exactly equal in weight to one-third of the acid originally taken for reduction. Hence assuming that the lowest oxide contains one atom of oxygen (an assumption borne out by the analysis of the chloride), the acid must contain three atoms of oxygen,* and the following formulæ represent the composition of these compounds according to Berzelius:—

$$VO \quad VO_2 \quad VO_3 \quad VCl_3 \quad (V = 68{\cdot}5).$$

The interest attaching to the conclusions which Berzelius fairly drew from his experiments was much heightened by an observation made by Rammelsberg in 1856, as to the exact crystalline form of the mineral vanadinite, a double salt of lead vanadate and lead chloride.

So long ago as 1780 Werner had observed the identity of crystalline form of two minerals, viz., apatite, a phosphato-fluoride of calcium, and pyromorphite, a phosphato-chloride of lead; to which may be added, mimetesite, an arsenato-chloride of lead. These minerals all have an analogous composition, being represented by the formulæ:—

Apatite $3\,(Ca_3P_2O_8) + CaFl_2$
Pyromorphite $3\,(Pb_3P_2O_8) + PbCl_2$
†Mimetesite $3(Pb_3As_2O_8) + PbCl_2$.

They are truly isomorphous, crystallising in hexagonal prisms, terminated with hexagonal pyramids, having the same angles and the same length of axes. Rammelsberg added to this list the mineral vanadinite, which he ascertained by measurement to be strictly isomorphous with the foregoing, and to be as follows. The angle P on P was in

1. Vanadinite	142°30′
2. Apatite	142°20′
3. Pyromorphite	142°15′
4. Mimetesite	142°7′

and the relation of the length of the axis:—

1.	1	: 0·727	3.	1	: 0·736
2.	1	: 0·732	4.	1	: 0·739.

So far, indeed, has the identity of crystalline form been traced, that crystals have been found which at one end consisted of vanadinite, and at the other of pyromorphite (Heddle). Now judging from the crystallographic analogies alone, we should conclude that the formula of vanadinite is

$$3(Pb_3V_2O_3) + PbCl_2.\ddagger$$

the oxide of vanadium contained in the mineral having a formula V_2O_5, agreeing with the corresponding oxides of phosphorus and arsenic, P_2O_5 and As_2O_5. In making this assumption, we are, however, at once confronted with the unyielding chemical facts of Berzelius, according to which the oxide in question must be represented by the formula VO_3, and contains three, and not five, atoms of oxygen.

It is, then, evident that we have here either to do with an exception to the law of Isomorphism, or else Berzelius's views are erroneous. Until this latter has been proved to be the case, chemists have, however, only been justified in assuming the former alternative to be the correct explanation.

The speaker stated that in order to endeavour to clear up this question, he had most carefully repeated Berzelius's experiments, and that he had confirmed them in every particular; but having pursued the subject further than Berzelius, he had at last come to conclusions concerning the constitution of the vanadium compounds totally different from those drawn by the Swedish chemist, and had succeeded in obtaining the key to the enigma presented by the above anomalous crystallographic relations.

The speaker has proved that the substance supposed by Berzelius to be vanadium, V = 68·5, is not the metal, but an oxide, and that the true atomic weight of the metal is 68·5 − 16 = 52·5 (or rather, according to the speaker's exact determinations of the atomic weight, 67·3 − 16 = 51·3).* The highest oxide, the vanadic acid, VO_3, of Berzelius, hence becomes a pentoxide, V_2O_5, corresponding to P_2O_5 and As_2O_5, and the isomorphism of vanadinite with the pyromorphite group of minerals is fully explained. The suboxide of Berzelius is a trioxide, V_2O_3, whilst the terchloride (VCl_3) of Berzelius is an oxychloride, having the formula $VOCl_3$, and corresponding to oxychloride of phosphorus, $POCl_3$. The oxide supposed by Berzelius to be the metal contains 51·3 parts by weight of vanadium to 16 parts by weight of oxygen, and the vanadic oxide of Berzelius also exists, containing 51·3 parts of the metal to 32 parts of oxygen; to these oxides the empirical formulæ V_2O_2 and V_2O_4 may be given. Thus we have the following as representing the true composition of these vanadium compounds:—

	Dioxide.	Trioxide.	Tetroxide.	Pentoxide.	Oxytrichloride.
V = 51·3	V_2O_2	V_2O_3	V_2O_4	V_2O_5	$VOCl_3$

Each of the four oxides can be obtained in the anhydrous state; the dioxide is prepared as a grey metallic powder, by passing the vapour of the oxytrichloride mixed with hydrogen over red-hot carbon. The trioxide is obtained by the reduction of vanadic acid in a current of hydrogen, and the tetroxide is formed by the slow oxidadation of the trioxide.

The lowest or dioxide of vanadium (V_2O_2) is obtained in solution by the reducing action of nascent hydrogen evolved from zinc, cadmium, or sodium amalgam upon the sulphuric acid solution of vanadic acid, which, passing through all stages of blue and green colour, ultimately assumes a permanent lavender tint. This solution of V_2O_2 in sulphuric acid acts as a most powerful reducing agent, bleaching indigo solution and other vegetable colouring matters as rapidly as chlorine; it also absorbs oxygen with avidity from the air, forming a deep brown solution. The other oxides of vanadium may be obtained in solution by the action of various reducing agents on the sulphuric solution of vanadic acid. Thus, by the action of nascent hydrogen evolved from magnesium a permanent *green* tint is obtained, and the vanadium is contained in solution as the trioxide, V_2O_3; whilst if moderate reducing agents, such as sulphurous acid, sulphuretted hydrogen, or oxalic acid are employed, the colour of the liquid does not pass beyond the *blue* stage, and the

---

* Berzelius concludes that the acid does not contain two atoms of metal, inasmuch as no alum could be formed with potassium sulphate corresponding to those formed by well-known sesquioxides.

† This group of minerals may be considered as calcium triphospho-fluorhydrine, &c., thus:—

$$3\,(PO''')\,Ca_5\,F\}O_9 \qquad 3PO'''\,Ca_5\,Cl\}O_9$$

Apatite. Pyromorphite. (*Wurtz.*)

‡ Or lead trivanadochlorhydrine $3VO'''\,Pb_5\,Cl\}O_9$

* In his paper on Vanadium, read before the Royal Society (Dec. 19. 1867), the author ventured to predict that the difference between the number he obtained (67·3) and that found by Berzelius (68·5) was probably owing to the fact that the vanadium compounds employed by Berzelius contained traces of phosphorus, which render the perfect reduction of the vanadic acid in hydrogen impossible. Most fortunately this supposition has been singularly verified, inasmuch as Dr. Frankland has kindly placed in the speaker's hands a small specimen of vanadiate of ammonia found in Faraday's collection, and labelled, "Sent to me by Berzelius. 1831." On examination, this sample was found to contain considerable quantities of phosphorus, thus confirming the speaker's previously expressed opinion.

vanadium is contained in solution as tetroxide, V_2O_4.* The different colours of solutions containing these oxides was exhibited by means of the magnesium light.

The fact that the lemon-coloured chloride (the ter-chloride of Berzelius) contains oxygen was clearly demonstrated during the discourse by passing the vapour from a few grammes of the substance, together with perfectly pure hydrogen gas, over red-hot carbon. A portion of the oxygen of the oxychloride unites with the carbon to form carbonic acid, and the presence of this gas was shown by the precipitation of barium carbonate in clear baryta water contained in two test-tubes placed one before the other. At the commencement of the experiment, the carbonic acid was entirely absorbed by the small quantity of baryta water contained in the first test-tube; but after some time the hydrochloric acid gas simultaneously produced by the decomposition of the chloride saturated this liquid, expelling the carbonic acid gas, which being carried forward into the second test-tube, threw down a bulky precipitate of barium carbonate, thus showing that the turbidity cannot possibly be due to the presence of any vanadium compound. It was found quite unnecessary to place a tube containing heated copper oxide after the red-hot carbon, for the purpose of oxidising any carbonic oxide gas which might be formed, inasmuch as carbonic acid was always left in sufficient quantity to give a considerable precipitate. No method has been found for separating the whole of the oxygen from the oxychloride, and hence it has been impossible to make the above experiment quantitatively. Solid oxychlorides are obtained by the action of hydrogen upon the oxytrichloride, one of which resembles mosaic gold, possessing a bright metallic bronze-like lustre, and having been taken for the metal by Schafarik.

The atomic weight of vanadium was determined (1) by reducing the pentoxide to trioxide in a current of hydrogen. (2) By the analysis of the oxytrichloride. The atomic weight obtained as the mean of a large number of well-agreeing experiments is 51·3.

The metal itself has not yet been obtained, but a compound of vanadium and nitrogen has been prepared, shown by direct analysis to contain 14 parts by weight of nitrogen to 51·3 parts by weight of vanadium, corresponding to the formula VN. The existence of this compound is proof positive of the true atomic weight of the metal, and the nitride serves as the point of departure from which to seek for the metal and the true chlorides of vanadium, one of which, VCl_3, has already been prepared by the action of chlorine upon the nitride. It is a dark brown liquid, which decomposes when thrown into water, forming a green solution containing V_2O_3. The speaker demonstrated the fact that the oxychloride, $VOCl_3$, when thrown into water decomposes with formation of a *yellow* solution of vanadium pentoxide, V_2O_5, whilst the trichloride, VCl_3, on being similarly treated yields a *green* solution containing the metal in solution as trioxide, V_2O_3. He then compared these reactions with the decomposition of the corresponding phosphorus compounds, $POCl_3$ and PCl_3, forming P_2O_5 and P_2O_3, and rendered these reactions visible by obtaining a precipitate of yellow silver phosphate in the first case, and of black metallic silver in the second.

The characters of the vanadates themselves bear out the analogy of the highest oxide with the corresponding oxides of phosphorus and arsenic. In the first place, all the naturally occurring vanadates are tribasic; secondly, the true character of vanadic acid is shown to be tribasic, by the fact that, when the pentoxide is fused with sodium carbonate, three atoms of CO_2 are liberated, and the normal or orthovanadate, $Na_3V_2O_8$ (corresponding to $Na_3P_2O_8$), is formed; thirdly, the so-called mono-vanadates are monobasic salts, corresponding to the monobasic phosphates, and may be termed meta-vanadates, thus, Na V O_3 and Ba 2 V O_3, whilst the so-called bi-vanadates are anhydro-salts.

All the reactions by which Berzelius explained the facts he discovered, can equally well be represented according to the new atomic weight and constitution; thus:—

Berzelius's Formulæ.

V = 68·5 O = 8

1) $VO_3 + H_2 = VO + H_2O_2$

2) $3\,(VO) + Cl_6 = VO_3 + 2\,(VCl_3)$

New Formulæ.

V = 51·3 O = 16

$V_2O_5 + 2H_2 = V_2O_3 + 2\,(H_2O)$

$3\,(V_2O_3) + 6\,Cl_2 = V_2O_5 + 4\,(VOCl_3)$.

The speaker stated that the foregoing facts clearly pointed out that vanadium, hitherto standing in no definite relation to other elements, must be regarded as a member of the well-known trivalent or triad class of elementary substances, comprising nitrogen, phosphorus, boron, arsenic, antimony, and bismuth.

It is true that we are still but imperfectly acquainted with many of the characters of vanadium, but the more its nature is studied, the more points of family resemblance will be discovered, and the more close will the ties be found, which bind it to the great triad family.

The following tabular statement of the compounds of the most important members of this group clearly shows their common relations:—

Trivalent Group of Elements.

	Nitrogen. N=14	Phosphorus. P=31	Vanadium. V=51·3	Arsenic. As=75	Antimony. Sb=122
Trihydrides ..	NH_3	PH_3	—	AsH_3	SbH_3
Trichlorides ..	NCl_3 (?)	PCl_3	VCl_3	$AsCl_3$	$SbCl_3$
Pentachlorides	—	PCl_5	—	—	$SbCl_5$
Oxychlorides	—	$POCl_3$	$VOCl_3$	—	—
Monoxides ..	N_2O	—	—	—	—
Dioxides.. ..	N_2O_2	—	V_2O_2	—	—
Trioxides ..	N_2O_3	P_2O_3	V_2O_3	As_2O_3	Sb_2O_3
Tetroxides ..	N_2O_4	—	V_2O_4	—	Sb_2O_4
Pentoxides ..	N_2O_5	P_2O_5	V_2O_5	As_2O_5	Sb_2O_5

In conclusion, the speaker remarked that vanadium was the fourth substance, supposed by its discoverer to be a metal, which had in recent years been shown to be a compound body.

Titanium.	Uranium.	Niobium.
Wollaston, 1823.	Klaproth, 1789.	Hatchett, 1801.
Wöhler, 1849.	Peligot, 1849.	Rose, 1842-64.
		Marignac, 1865.

Vanadium.
Sefström and Berzelius, 1831.

Trimethylamine in Wine.—White Austrian wine, according to E. Ludwig, contains trimethylamine, which the author isolated in the following way:—The wine was freed from alcohol by distillation, then again distilled with a solution of sodic hydrate, the alkaline distillate neutralised with sulphuric acid, and evaporated. The residual salts, containing much ammonic sulphate, were extracted with absolute alcohol, the alcoholic solution evaporated, and the residue again distilled with sodic hydrate. From this distillate, the platino-chloride was prepared, and identified as that of trimethylamine.—(*Akad. z. Wien.*, 56, 1857.

* In his communication to the Royal Society (Bakerian Lecture, Proc. Royal Soc. xvi. 220), the author gave the empirical formula VO and VO2 to the 1st and 3rd oxides of vanadium, as the molecular weights of these oxides have not been determined, and it is uncertain whether they obey the law of even atomicities, or, like the only corresponding compounds, the nitrogen oxides, are exceptions to this law. On consideration, the author has, however, thought it best to adopt the doubled formula as urged by Sir Benjamin Brodie on the occasion above referred to.

CONTRIBUTIONS TO OUR KNOWLEDGE OF THALLIUM.*

By Prof. Dr. J. W. GUNNING.

One of my former pupils, M. Serrurier, now managing director of the Amsterdam soda manufactory, had the kindness to make me a present of the flue dust obtained at the works where the pyrites used for making sulphuric acid is derived from the neighbourhood of Suhrort. I found this flue dust to yield about 1 per cent of chloride of thallium; the bulk of the dust is made up of arsenious and arsenic acids, some iron and lead, but hardly any sulphuric acid. It is usual in order to obtain thallium from this dust to boil it (the dust) with dilute sulphuric acid, to strain, and to precipitate the thallium by means of hydrochloric acid; the chloride of thallium so obtained is washed, and afterwards dissolved in strong sulphuric acid, yielding the well crystallising sulphate of thallium. Another plan is to digest the flue dust with a solution of carbonate of soda, and to precipitate the thallium by means of hydrosulphuret of ammonium. It has struck me while engaged with this matter that neither of these methods answer the purpose well; the sulphate and carbonate of thallium are not very readily soluble, and unless therefore one is prepared to lose a portion of thallium, there is no end of boiling the flue dust with solvents. One must, moreover, bear in mind that the flue dust contains a portion of the thallium as peroxide, insoluble in soda, and indifferently soluble only in dilute sulphuric acid. The presence of Tl_2O_3 in flue dust is proved in this way: after long treatment with soda solution there is a brownish muddy mass left, which, when acted upon by sulphurous acid dissolved in water, becomes partly discoloured and yields a large proportion of sulphate of thallium.

I have applied phosphoric acid to extract thallium from the flue dust, and I find it answer admirably well. The phosphates of thallium, and especially so the acid phosphate, are among the most soluble of the salts of thallium. Since phosphoric acid itself is rather too expensive to be thus applied, I have substituted therefore a mixture of bone ash and sulphuric acid, which answered the purpose splendidly; it is only required to digest and heat the mixture of flue dust and bone ash with sulphuric acid, and some water, a sufficiently long time, to render a twice repeated digestion quite efficient to remove from the flue-dust I had obtained, all the thallium it contained, amounting to about 1 per cent of the whole mass; of course the filtrate was treated with hydrochloric acid. The fluid from which the chloride of thallium had been separated by filtration, contained, however, yet a considerable amount of thallium dissolved, partly so in consequence of the non-thorough insolubility of chloride of thallium, partly also as thallic salts not precipitable by hydrochloric acid; in order to obtain this portion sulphite of soda is added to the acid liquid, whereby the thallic salts are reduced to thallous salts, next the acid is pretty nearly neutralised with carbonate of soda, and the thallium compounds afterwards precipitated by iodide of potassium as insoluble yellow iodide of thallium.

The best and easiest plan to convert crude chloride of thallium into pure salts of thallium is this, that, after washing with water acidulated with some hydrochloric acid (since pure water dissolves a larger proportion of the chloride) the chloride is peroxidised. This I perform in the following manner: Place in a porcelain basin a moderately concentrated solution of carbonate of soda, pass a powerful current of chlorine gas through it, and stir meanwhile in the chloride of thallium by small quantities at the time; the decomposition is instantaneous, and the chloride of thallium soon quite converted into black peroxide. If the quantity of chloride of thallium which has to be thus acted upon is somewhat large, it is requisite to take a fresh quantity of soda solution, since it is clear that the solution must not become acid. One may rest assured that there is no loss of thallium when after the operation is finished the liquid is yet alkaline, and contains free chlorine (hypochlorites). The peroxide of thallium should be first washed by decantation, afterwards on a filter, it is next diffused through a small quantity of water, through which a stream of sulphurous acid gas is passed, whereby it (the peroxide of thallium), becomes dissolved as sulphate of thallium; this latter salt is most readily obtained in the crystalline state by allowing its solution spontaneously to evaporate placed under a desiccator. This mode of operating is far preferable to that of decomposing chloride of thallium by strong sulphuric acid, whereby beside the inconvenience of acid vapours, also is experienced some difficulty in consequence of the slowness and somewhat difficultly attainable completeness of the process. I have found that crude chloride of thallium always contains arsenic, even when the precaution is taken to precipitate that salt from a dilute and perfectly clear solution; I think this is due to the fact that the precipitated chloride of thallium, carries down along with it arseniate of thallium mechanically. Since the flue dust contains arsenic acid as well as arsenious acid, the former of these is the cause why crude chloride of thallium, dissolved in sulphuric acid, always yet contains arsenic.

It is a known fact that when arsenious acid is heated in the presence of chlorides and sulphuric acid, there is formed $AsCl_3$, which, however, does *not* take place with arsenic acid. The sulphuric acid solution, while submitted to Marsh's test, exhibits quite plainly the well-known reaction for arsenic; but, on the other hand, on being treated with H_2S, yields a beautiful, brownish-red, very flocculent precipitate, which might be taken to indicate antimony, although I thoroughly verified the absence of that metal in every way. It has been asserted by some chemists that there exists a reddish, or brown-reddish coloured higher sulphuret of thallium insoluble in dilute acids, but a rather instable compound. The substance just alluded to is no doubt that sulphuret, of the existence of which one may easily satisfy one's self by treating dry sulphate of thallium with *aqua regia*, or by boiling the sulphate of thallium with sulphuric acid and peroxide of lead, or of manganese; the solution of the thus partly-oxidised sulphate yields, on being submitted to a current of H_2S, in the first instance, a brownish-red precipitate, which, however, gets soon after dissolved, while sulphur is deposited; H_2S reduces also the thallic salts to thallous salts. I have not succeeded in obtaining this sulphuret under conditions of greater stability.

Böttger, *Ann. der Chem. u. Pharm.*, cxxviii., p. 249, speaks of this sulphuret, which, according to his researches, should be a higher sulphuret of thallium mixed with sulphide of arsenic and free sulphur; he also mentions that the said compound may be obtained in pure state by treating an acid thallic salt with a rather small quantity of hyposulphite of soda. I consider this last assertion to be highly improbable, since even sulphurous acid so readily reduces thallic salts to thallous salts. On repeating the experiment with a solution of the chloride or sulphate of thallium, and addition of some hyposulphite of soda, I have seen nothing but a separation of sulphur. I cannot, however, consent to agree to the opinion that the sulphuret in question should just be a higher sulphuret, for the following reason:—I found that the solution of the crude chloride of thallium in sulphuric acid, after having been treated with a current of sulphurous acid in excess, and afterwards with H_2S, just again yields the same reddish-brown precipitate. The following reactions justify the opinion that the brownish-red precipitate is a peculiar sulphide of thallium; strong bases readily convert it into black sulphide of thallium TlS_2, while sulphuret of arsenic becomes dissolved in the alkaline lye, and is re-precipitated therefrom on addition of an acid, as yellow As_2S_3, without simultaneous

* Translated by A. Adriani, M.D., Ph.D., &c.

precipitation of sulphur, and also without disengagement of H_2S, which latter occurrence could not have failed to take place if one had had to deal in this instance with a higher degree of sulphuration than As_2S_3, or Tl_2S; its behaviour on being heated, whereby a sublimate is obtained partly of As_2S_3, partly of As_2O_3, while at the same time black sulphuret of thallium remains as a molten mass; it is easy to obtain at will the very self-same precipitate from every thallous solution by simply adding to it, first, some arsenious acid, then to pass through a current of H_2S, while it does not in the least matter whether the fluid is acid or has been rendered alkaline by ammonia; the said precipitate also occurs when an ammoniacal solution of As_2S_3 is precipitated with an ammoniacal solution of a thallous salt; from these results I draw the inference that I had simply to deal in this case with a mixture of As_2S_3 and Tl_2S. Analysis has proved this to be quite correct; the substance does not contain anything else than arsenic, thallium, and sulphur, while the latter is present in the proportion required to form Tl_2S and As_2S_3. My assistant, Mr. Adriaanz, performed the analysis in different ways: 1st. The substance weighed in a small flask was treated with a solution of pure caustic potassa, quite free from any sulphuric acid [sulphate], chlorine gas was passed through, and after a while the matter was entirely dissolved; from this solution pure carbonate of potassa free from sulphate, precipitated Tl_2O_3, this, after having been well washed upon a filter, was dissolved in sulphurous acid, next evaporated to dryness, again re-dissolved and precipitated with iodide of potassium as insoluble iodide of thallium, then dried and weighed. In the filtrate from Tl_2O_3 the sulphuric acid was determined in the usual way, and the arsenic As_2O_5 as ammonio-magnesian arseniate. This mode of proceeding always gave the arsenic too low. 2nd. The same method was applied, with this difference: that in order to prevent loss of arsenic, iodine was used instead of chlorine to effect the oxidation; the excess of iodine was eliminated by evaporation along with alcohol; by this process, however, it was found that a portion of the sulphide of thallium was not properly oxidised, in consequence whereof the result for S was found too low. 3rd. The substance was oxidised with pure nitric acid, or pure *aqua regia;* the sulphur which partly escaped oxidation, was weighed in that state, after having eliminated the excess of acid by evaporation; the thallic salt was, after addition of some potassa, precipitated by means of iodide of potassium; the iodine which hereby separated was eliminated by evaporation along with alcohol, the iodide of thallium separated by filtration, and As and S estimated as just described. While executing these operations, my assistant experienced great difficulty in obtaining a complete reduction of the thallic salt, in consequence whereof the iodide of thallium did not present its usually bright, splendid yellow colour, but was always somewhat blackish; it was, moreover, very difficult to remove from the filter, whereupon the sulphur had been collected, some thallic salt tenaciously adhering to both sulphur and filter. As a sequel of these difficulties, the estimation of the different compounds did not take place from one and the same weighed quantity, but care was taken to estimate all compounds in samples obtained by one and the same preparation. After it had been duly ascertained that on being heated above 100°C., the substance did not lose any more water, the samples submitted to analysis were dried at 100°C. Dried at that temperature the substance is found to be a readily mobile very light powder which, on being rubbed, becomes electrified. The following are the results obtained on analysis:—Tl = 204.

	No. 1.	No. 2.	No. 3.	No. 4.	No. 5.	No. 6.
Sulphur ..	18·82	19·56	21·27	26·75	18·14	24·66
Arsenic ..	21·40	21·56	20·08	—	—	26·00
Thallium ..	60·57	57·07	58·65	41·52	67·33	46·27
	100·79	98·19	100·00			96·93

Nos. 1—3 have been prepared by passing a current of H_2S through a solution of arsenious acid and sulphate of thallium in excess, acidified with sulphuric acid. The wash water ought to be mixed with an aqueous solution of H_2S, since otherwise the filtrate becomes somewhat coloured.

No. 4, prepared by mixing an ammoniacal solution of As_2S_3 with excess of an ammoniacal solution of sulphate of thallium, and afterwards addition of sulphuric acid until acid reaction ensues.

No. 5, prepared by adding to an excess of an ammoniacal solution of As_2S_3 an ammoniacal solution of sulphate of thallium.

No. 6, prepared as No. 5, but in reversed order, *i.e.*, thallium in excess. I am quite aware that these analyses exhibit some imperfections due especially to the difficulty quite correctly to estimate As. but more so yet Tl; this, however, is demonstrated that from acid solutions containing thallium in excess, is obtained a combination made up of equivalent proportions of sulphide of arsenic and sulphide of thallium. The calculated result of this compound As_2S_3, Tl_2S, is the following:—

Sulphur	18·7
Arsenic	21·9
Thallium	59·4

Ammoniacal solutions do not yield a compound of constant composition; even while being prepared the washings run off coloured, while the precipitate itself is evidently decomposed by ammonia. When a current of H_2S is passed through a solution containing equivalent proportions of As_2O_3 and Tl_2SO_4, only a small proportion of thallium is thrown down along with an excess of As_2S_3. If the quantity of As_2O_3 is increased above that of Tl_2SO_4, yet thallium remains in the solution, even a large excess of arsenic is incapable of assisting in throwing down the whole of the thallium; this, notwithstanding all these different precipitates, exhibits the same bright reddish-brown colour. In consequence of this somewhat singular behaviour, I, for a moment, suspected that some peculiar variety of the thallium salt might perhaps exist. I soon found, however, that if a solution of a salt of thallium is treated with As_2O_3 and H_2S, and this operation is repeated with the same solution of thallium, a second time a loss of metal is experienced, and that by repeating the same operation with the same sample over and over again, at last all the thallium is eliminated. I do not think it is possible to assign to this red substance a place among the well defined compounds. I do not know of another instance of similar composition and origin. It appears to me that the most probable explanation may be the following: To assume that beside the black sulphuret of thallium there exists another sulphuret of the same composition, but of different colour (akin to what is well known to be the case for mercury and antimony), and that this modification of the sulphuret of thallium is perhaps crystalline, and hence more apt to resist the action of acids, and that, furthermore, this compound formed as has been explained, can unite with sulphuret of arsenic, and form a molecular compound therewith. But even this explanation leaves unanswered the question which can be asked, How is it that the sulphuret of arsenic, the presence of which, and aptitude to combine with this red sulphide of thallium, is the cause of the origin of the latter, does not act in proportion of its quantity present in a given solution, but only transforms a comparatively small proportion of the thallium present into that compound?

The chemical history of this red substance teaches us that thallium cannot be separated from arsenic by means of H_2S, a point of some importance in quantitative analysis, as well as while operating on the flue dust from sulphuric acid works. It also deserves notice that the orange-yellow colour often exhibited by sulphur obtained from pyrites, and which is usually accounted for by ascribing that colour to the presence of selenium, may be

equally well due to such sulphur containing thallium. I beg leave to state in connection herewith, that Mr. Wm. Crookes, the discoverer of thallium, found in crude sulphur obtained from Spanish pyrites 0·29 per cent of thallium. Mr. Crookes, however, did not record what colour this sulphur exhibited.*

A NEW ASPIRATOR.

By J. LANDAUER.

Though a number of aspirators of different construction are proposed and used in chemical analysis, the following may be added to the number as being distinguished by great simplicity. This new aspirator is based on the principle of the siphon. A capacious flask is hermetically closed by a cork provided with two holes. One of the latter receives the siphon, and the other a glass tube for connecting the apparatus, through which the passing of a current of air is desired. After having made the connections and filled the flask with water, the latter is made to run out of the flask by sucking the outer leg of the siphon, the end of which must, of course, be lower than the level of the water. The current of air is thus effected.

The efflux of water is regulated by joining more or less width to the tubes between apparatus and aspirator. Two aspirators are connected with the apparatus, and used alternately, in order to enable a refilling of the flasks without interruption of the process. For this purpose an intermediate apparatus, consisting of a glass tube about two inches long, and one inch wide, is required, which is connected on one side with the apparatus intended for receiving the current of air, and on the other side with the two aspirators. The latter connection may be effected by india-rubber tubes, each provided with Mohr's pinch-cock. Such arrangement will enable the alternate working of the two aspirators. It is of course understood that all the connections must be effected hermetically. One of the advantages of this aspirator is that it allows an easy determination of the quantity of air passed through by employing graduated vessels.

PROCEEDINGS OF SOCIETIES.

PHARMACEUTICAL SOCIETY,

Wednesday, March 4th.

H. Sugden Evans, Esq., Vice-President, in the Chair.

The minutes of the preceding meeting were read and confirmed.

Specimens of crab oil, cayenne pepper in olive oil, and other interesting drugs from British Guiana and New South Wales, were presented to the museum by P. L. Simmonds, Esq.

The Chairman directed attention to a spurious jalap, from New York, 16 bales of which had been offered at a public sale, but was not purchased. He thought it was the rose-scented jalap, and previous to the meeting he had shown it to Professor Bentley, who thought it bore a great resemblance to the specimen of rose-scented jalap in the museum.

Mr. D. Hanbury, F.R.S., who read a paper a short time since "*On the Cultivation of Medicinal Plants*," mentioned that he very recently dug up a root of jalap from his father's garden, at Clapham, which was planted last June twelvemonth. It had remained in the open ground during the winters of 1866 and 1867. One tuber had produced six large tubers and twenty-four small ones. He thought it grew better in the open air than under glass. Unfortunately the flowering was too late for the seeds to ripen. This circumstance goes far to prove that jalap might be cultivated in Europe with ordinary attention.

In reply to the Chairman and Mr. Morson, Mr. Hanbury said that he was not prepared to speak of the properties of the jalap, not having tested it sufficiently.

Mr. Umney had examined the specimen of jalap from New York, on the table, and thought it differed from the rose-scented jalap in the museum: it possessed a peaty odour which the other did not.

Professor Attfield then read a paper "*On the Analysis of the Water of a Remarkable Medicinal Spring in Jamaica*," which he had received for analysis in May, 1867. It had been sent from Jamaica with the statement that thousands of the negroes had for weeks flocked to the spring, thinking it was a cure for all diseases. It was clear, inodorous, and strongly alkaline to the taste, its specific gravity being 1026·6. An imperial gallon contained 2493½ grns. of solid matter, which is about the average amount of saline compounds in sea water, but the author thought that spring water containing so much mineral matter had never been known. The constituents in one gallon were:—

Chloride of calcium	1510·00 grs.
Chloride of sodium	981·00 ,,
Chloride of ammonium	2·43 ,,
Water..	69368·57 ,,

So that a gallon contained about 3½ oz. of chloride of calcium, 2 oz. of salt, and 2½ grs. of chloride of ammonium. Dr. Attfield had tested it for sulphates, nitrates, carbonates, potassium and magnesium salts, bromides, iodides, fluorides, sulphides, phosphates, nitrites, silicates, borates, and a number of other salts, but found none, and animal and vegetable matter were also absent. The proportion of chloride of calcium he believed to be unprecedented. There was another spring in Jamaica of a thermal character, which contains 105 grs. of chloride of calcium to the gallon; the saline and chalybeate water of Harrowgate contains between 120 and 130 grs. of chloride of calcium; and the water of the Dead Sea, 25 per cent of which was stated to be solid matter, also contained a little chloride of calcium. The gases dissolved in the water were small in amount—one gallon contained 3·33 cubic inches of nitrogen, 1·55 of oxygen, and ·50 of carbonic acid. In sending the result of the analysis to Jamaica, Dr. Attfield asked the proprietors of the estate to send him some information on the history of the spring and topography of the district, thinking it would be of geological as well as chemical and physiological interest. From the report he had received it would appear that the water had been used for medicinal purposes upwards of forty years. The negroes believed it a cure for every disorder, but it was chiefly used for scrofulous affections, glandular swellings, &c. The author quoted from Pereira's statement of the therapeutic action of chloride of calcium, which shows that it is most useful in those diseases for which the negroes have recourse to the medicinal water. The spring is 68 ft. above the sea level, and 76 chains from it; temperature, 82°F.; and it makes its appearance in the diluvial gravel that nearly fills a small brook known as the Saint's Ann's Great River. Dr. Attfield presumed it was of volcanic origin.

The Chairman said they were very much obliged to Dr. Attfield for his paper, which contained a number of points of great interest.

Mr. C. H. Wood suggested that it might be used to water the road, as an artificial water was manufactured for that purpose.

Mr. C. H. Wood read a paper "*On the Syrup of Hypophosphite of Iron*," in which he referred to the disadvantages of the processes given in the *Pharmaceutical Journal* (vol. vii. p. 440) and *Parrish's American Pharmacy*.

* The colour varies between orange, red, and dark grey, according to the quantity of thallium present.—W. C.

He had made a good syrup, containing 2 grs. of hypophosphite of iron in 1 dram, by dissolving 480 grs. of granulated sulphate of iron in 1 ounce of dilute phosphoric acid and 1½ ounce of distilled water, then reducing 326 grs. of pure hypophosphite of lime to fine powder, adding to it the solution of sulphate of iron, and triturating before transferring to calico. The liquid was then pressed out, filtered, and mixed with seven times its volume of simple syrup.

The Chairman thanked Mr. Wood for his practical paper, and

Mr. Umney referred to a process for the preparation of the syrup which contained 1 gr. of hypophosphite of iron to the dram, but he thought Mr. Wood's process was an improvement.

Dr. Attfield asked Mr. Wood if he had experienced any danger in preparing the hypophosphite of lime. He had heard of an explosion occurring, even when it was dried at a low temperature, 130 to 140°.

Mr. Wood had not attempted to make any quantity, on account of phosphoretted hydrogen being evolved, which would prove an annoyance to the neighbourhood.

Professor Redwood had never heard of an explosion taking place in the preparation of hypophosphite of lime, but in preparing hypophosphite of soda it was necessary to dry at a very low temperature, to prevent an explosion.

Professor Attfield described a laboratory experiment relating to "*Magnetic Hydrate of Iron*." He had added an alkali to a solution of ferrous and ferric sulphate, in molecular proportions, and obtained the usual black hydrate of iron having the well-known property of being attracted by a magnet, even when the latter was simply immersed in the mixture. He then precipitated appropriate quantities of ferric hydrate and ferrous hydrate in separate vessels; neither of the precipitates was attracted by a magnet. The contents of the vessels were then well mixed, when a hydrate resulted, which at first was not at all magnetic, feebly attracted after ten minutes, its attractability slowly increasing until, after twenty-four hours, it appeared to be more strongly attracted than the black hydrate made in the usual way.

This experiment showed, first, that in making magnetic oxide of iron for use in medicine, fuel need not be wasted in obtaining ferric sulphate entirely free from the nitric acid used in its preparation, for the ferric solution could be poured into the alkali before the ferrous, any nitric acid thus becoming neutralised and prevented from oxidising the ferrous salt; second, it afforded confirmation, were any needed, of the view that magnetic hydrate was a compound and not a mere mixture of ferrous and ferric hydrates; third, it was a good illustration of the influence of time in chemical change.

Dr. Redwood thought that by boiling the solution, one would obtain a more certain and definite preparation.

The Chairman, after thanking Dr. Attfield for his communication, announced that the next meeting would be held on the 1st of April, when a paper would be read by Mr. C. H. Wood, and several laboratory experiments would be described.

The meeting was adjourned at an early hour.

Derivatives of Vinyl-Compounds.—Glinsky has studied the reaction between vinylic bromide and mercuric oxide, or hypochlorous acid (suggested by an analogous investigation by Linnermann, *vide* Chemical News, No. 428, 83), and obtained aldehyde, and a white amorphous body, which was found to be a compound of aldehyde and mercurous bromide of the formula

$$\left.\begin{matrix} C_2H_3 \\ C_2HO_2 \end{matrix}\right\} + 2Hg_2Br.$$

—(*Zeitschr. Chem.*, N.F. iii., 675.)

FOREIGN SCIENCE.

Paris, March 17, 1868.

Colouring Principles in Madder.—Important Process in Dyeing.—Preparation of Uric Acid.—Extraction of Oils by means of Bisulphide of Carbon.—The Sesqui-fluoferrates.—Academy of Sciences: Production of Chlorine and Oxygen.—Products of the Slow Oxidation of Phosphorus.—Diffusion and Endosmose.—Mode of Action of Common Salt as a Manurial Agent.—Antiputrescent Properties of Sulphuric Ether.

Some time ago madder formed the subject of a communication to the Société d'Encouragement, by M. Schiitzenberger. This communication has led the way to results of great interest, whether taken in a scientific aspect, or a pecuniary one. The existence of five pigments in this colouring matter, he had remarked, may be recognised—they are alizarine, purpurine, orange madder, pseudo-purpurine, and xantho-purpurine. These bodies are distinguished by their composition, their physical and chemical properties, and especially by their comportment in dyeing operations. Different samples of madder give, then, different results in dyeing, according to the predominance of one or other of the principles, and, as is well known, different tints according to the mordants used. Pseudo-purpurine is distinguished by its fugitiveness under the influence of soap; and the want of stability in the tinctures made from the madder of Alsace, is due to the presence of this principle in large proportion. M. Martin, observing these points in M. Schutzenberger's investigation, and knowing alizarine to be the only one of the five colouring matters yielding an unalterable dyeing material, essayed the transformation of the other principles into alizarine. The result has been that M. Martin has discovered a process by which purpurine, pseudo-purpurine, xantho-purpurine (yellow), and the orange-colouring matter can be transformed into alizarine: the process he has patented. This transformation is easily effected by the combined action of dehydrating and reducing agents. The colouring matters, either separately or mixed, are dissolved in concentrated sulphuric acid, and when dissolved, zinc is added. Elevation of temperature and a finely divided condition of the metal, facilitate the reaction. As soon as the transformation is considered complete, the mass is diluted with water, which gives rise to an abundant precipitate of alizarine; washing with water will render the matter ready for dyeing purposes. It will be interesting to compare the composition of the five principles occurring in madder, as given by M. Schützenberger:—

Alizarine	$C_{40}H_{12}O_{12}$
Purpurine	$C_{40}H_{12}O_{14}$
Orange madder	$C_{40}H_{16}O_{18}$
Pseudo-purpurine	$C_{40}H_{12}O_{18}$
Xantho-purpurine	$C_{40}H_{12}O_{12}$

The process of M. Martin would seem to be one of great importance.

Dr. Löwe has indicated the following method of preparing large quantities of uric acid from Peruvian guano. The guano is pulverised and dried at 100°C.; 1 part by weight is added in small quantities at a time to 1 part of sulphuric acid contained in a capsule, heated by a water bath, and stirred with a glass rod. The mixture is allowed to remain on the water bath as long as hydrochloric acid continues to be evolved. When the odour of hydrochloric acid is only slight, and the mixture seems homogeneous, 12 or 15 volumes of distilled water are added; this dilution causes a yellow precipitate, for the subsidence of which time is allowed. Afterwards the supernatant fluid is decanted off, the precipitate is washed with fresh quantities of water, thrown upon a filter, and the greater part of the sulphuric acid washed out. Small portions of this precipitate are now boiled in a weak solution of an alkali, the solution filtered and acidified with dilute hydrochloric acid to precipitate the uric acid, which appears as a

yellow cloud. When cold, the precipitate is thrown on to a filter, washed, and dried. The yellow colour may be removed by heating with sulphuric acid at the temperature of a water bath, and repeating the process described ; it is necessary to avoid adding more water in precipitating than is absolutely necessary, since the product is always yellow when excess of water has been used.

The extraction of oils by means of bisulphide of carbon is now carried on at Moabit, near Berlin, upon a very great scale. In the manufactory of M. Heyl, 2,570 kilog. of oil, of sufficiently good quality to be employed in lubricating machinery, are manufactured daily. Colza and linseed are the materials chiefly operated upon : the residues serve very well to feed cattle with. The seeds are first crushed and dried by heating. For the daily fabrication of 2,570 kilog. of oil, only six men are required. Analysis has shown the residues to contain only 2 per cent of oil and 7 per cent of water, while the residues of the ordinary pressure process contain 9 per cent of oil and 15 per cent of water. In the extraction of the oil, 7,000 kilog. of bisulphide of carbon are used daily, and the amount lost is 28 kilog.

M. Nicklès has published a paper on the sesquifluoferrates. The sesquifluoride of iron forms combinations with fluoride of sodium and ammonium ; they are obtained in two ways, either by direct union of the sesqui fluoride of iron with the alkaline fluoride, or by decomposing the latter with sesquichloride of iron. The alkaloids also unite with this fluoride : combinations have been prepared with quinine and brucine. When the solutions of the ammonic and potassic fluo-salts are boiled, they are decomposed, depositing yellow flakes charged with iron. Ammonia separates sesquioxide of iron from these solutions : ferrocyanide of potassium causes a blue colouration, unless the solution contain an excess of alkaline fluoride. The colouration which is usually manifested is of a fine violet tint, quite different from the blue precipitate of prussian blue ; it is capable of utilisation, M. Nicklès thinks, in the preparation of colours.

Among the memoirs brought before the Academy at the *séance* of the 24th of February, were the following :—A memoir on the production of chlorine and oxygen, by M. Mallett ; on ozone and phosphoric acid, the result of the slow oxidation of phosphorus, from M. Blondlot ; a memoir relating to diffusion, endosmose, molecular movement, &c., from M. Dubrunfaut ; on the mode of action of common salt employed as a manure, by M. Jean ; a note on the anti-putrescent properties of sulphuric ether, from M. Martin ; analyses of some waters from the thermal springs of Ischia, near Naples, by MM. Mène and Rocca-Tagliato. The section of rural economy has presented the following list of candidates for the place vacant in it from the death of M. Rayer :—(1) M. Reiset ; (2) MM. Bouley, Dubrunfaut, and Hervé Mangon ; (3) M. Richard.

M. Mallett's memoir was an explanation of a process to which he called the attention of the Academy last year. He remarked that the fixation of the atmospheric oxygen upon protochloride of copper, permitted either of making the latter yield the oxygen, or yield chlorine upon addition of hydrochloric acid. The absorption of oxygen by protochloride of copper is spontaneous ; the air being ordinarily moist, it will be complete in a few hours, if fresh surfaces be renewed. But elevation of temperature, and this is a main point, induces a much more rapid absorption : at temperatures between 100° and 200°, as well as at higher temperatures in the presence of water, this absorption may be considered as almost instantaneous. By this process 100 kilog. of chloride of copper (cuprous chloride), usually mixed with inert matter for convenience, will yield 3 to 3½ cubic metres of oxygen, or 6 to 7 cubic metres of chlorine, and as four or five operations may be made in four-and-twenty hours, this quantity, 100 kilog., would yield 15 to 18 cubic metres of oxygen, or 200 to 300 kilog. of chloride of lime, during the same time : the price of the chloride of copper does not exceed 1 franc the kilogramme.

When phosphorus undergoes slow combustion in air, it is generally considered, M. Blondlot says, that ozone and phosphorous acid are produced ; these two bodies being incompatible, he thought the matter worthy of investigation. For this purpose, he took a flask of several litres capacity, closed with a cork carrying two tubes. One descending to the bottom of the vessel, communicated at the upper extremity, by means of a caoutchouc tube, with a reservoir of water furnished with a tap ; the other was simply a curved tube for the delivery of the gas. In the ascending portion of this tube he placed a thin cylinder of phosphorus about 15 centimetres in length. This arrangement made, a fine jet of water was made to issue into the flask, when the air was expelled, bubble by bubble, between the phosphorus and the sides of the tube. The resulting gas, collected in the usual way, was washed at several intervals with water, until white vapours had completely disappeared. Two important facts have been demonstrated by the experiment. The first is, that if the ambient air should not attain vigourously 12°, when it leaves the apparatus, it will have acquired distinctly the characteristic odour of ozone, but it will not affect iodide of potassium and starch paper ; while if the air of the apparatus is at 12° to 13°, these same papers, suspended in the receiving flasks, become as distinctly blue as if the temperature had been much higher. The second is that, whatever temperature one operates at, the white vapours which escape from the apparatus are composed exclusively of phosphoric acid, without admixture of phosphorous acid. There is no difference in the product after the greater part of the oxygen has been withdrawn from the air, this condition being attained by collecting the escaping gases in a flask over water, and then making the phosphorus undergo slow combustion in these collected gases. To be enabled to prove that phosphoric acid is the only product of combustion, it suffices to pass the escaping gases into distilled water. The solution, very distinctly acid, being exactly neutralised with potash, causes in nitrate of silver a yellow precipitate ; it does not decolourise permanganate of potash, and introduced into a Marsh's apparatus, produces no green flame. This being so, it would seem curious why, in the class experiment, by the aid of which one obtains what was formerly called phosphatic acid (PO_3), there is produced phosphorous acid mixed with phosphoric acid. The explanation is simple. M. Blondlot has found that in a flask filled with a solution of phosphoric acid, to which a few pieces of phosphorus have been added, and corked, a portion of the phosphoric acid is speedily converted into phosphorous acid according to the equation $3PO_5 + 2P = 5PO_3$. From this it follows that when small sticks of phosphorus are exposed in narrow tubes to the action of moist air, the phosphorous acid produced is the result of a deoxidation of the higher oxide first produced. In conclusion, M. Blondlot states that, whatever be the rapidity of the combustion, when phosphorus undergoes combustion in air, phosphoric acid is the only product.

M. Dubrunfaut placed before the Academy a number of theorems relating to diffusion and endosmose. Among others, these :—Diffusion is always a molecular property, whether manifested with or without diaphragms (the first condition being considered endosmose, the second, diffusion). It is always accompanied by the double current, observed by Priestley and Dutrochet, and it is the result of an attractive force which is developed by the juxtaposition of molecules of matter of different kinds, or of molecules of matter in a different physical state. The effects of diffusion and endosmose are referable to one force—diffusion, which, though exerted at insensible distances, canno be said to result from contact, since these distances are really great, and depend upon the finite dimensions of the molecules of the matter operated upon. The force of diffusion is always exerted in one direction, which is normal at the surface of contact of the

fluids. It varies in intensity with different fluids, and with fluids of different densities, wherefore, also, with the temperature and pressure. Mechanical work in commensurable degree is performed. In fact, fluids which mix with great perfection offer with the displacement of their centres of gravity useful data wherewith to calculate the work performed in producing the mixtures.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Carbonic Oxysulphide.—C. Tran. This gaseous compound is formed; 1. By direct union of carbonic oxide with sulphur, at a low red heat; 2. By the action of dilute acids upon sulphocyanhydric acid:

$$CSNH + H_2O = NH_3 + CSO.$$

By making use of the latter reaction large quantities of the new compound may easily be prepared. Powdered potassic sulphocyanide is added to a mixture, by volume, of 5 sulphuric acid and 4 of water, (in such proportions that the mass remains liquid), and the evolution of gas which sets in at once kept constant by alternately heating and cooling. The gas is purified from traces of cyanhydric acid (and formic acid), carbonic disulphide, and aqueous vapour, by letting it pass successively over mercuric oxide, non-vulcanised india-rubber, and calcic chloride. It may be collected over mercury when dry, without suffering change; water dissolves an equal volume, and gradually decomposes it. Its temperature of ignition is very low; it burns with blue flame; the products of combustion being carbonic anhydride, and sulphurous acid. Alkalic hydrates absorb it with formation of carbonate and sulphide. Its sp. gr. was found = 2·10.—(*Ann. Chem. Pharm.* 5 suppl. 236.)

Isomeric Compounds derived from Benzoic Acid.—H. Hübner and F. Mecker. The bromnitrobenzoic acids obtained from brombenzoic acid by the action of very strong nitric acid, at a moderate temperature, are best separated by extracting the mixture of acids repeatedly with insufficient quantities of boiling water, until the residue has become insoluble and infusible under water. (Those acids, derived from the insoluble portion, fusing at 248°C., are named α; those from the soluble, fusing at 140° β compounds.) β bromamidobenzoic acid,

$$C_7H_4Br(NH_2)O_2,$$

obtained by reduction of the corresponding nitro-compound with tin and chlorhydric acid in the proportions shown in the following equation:—

$$C_7H_4Br(NO_2)O_2 + 6Sn + 6HCl = 6SnCl + 2H_2O + C_7H_4Br(NH_2)O_2$$

crystallises in small needles, and fuses at 171 to 172°. β bromamidobenzoic acid, $C_7H_3Br(NH_2)O(OH)$, crystallises in long needles, which are sparingly soluble, and fuse at 202 to 204°. α and β amidobenzoic acid, $C_7H_5(NH_2)O_2$ and $C_7H_4(NH_2)O(OH)$, which are obtained from their respective bromnitro-compounds by employing the following proportions:—

$$C_7H_4Br(NO_2)O_2 + 8Sn + 7ClH = C_7H_5(NH_2)O_2 + 7SnCl + SnBr + 2H_2O$$

have been found identical so far as experiments at present went, but isomeric with ordinary amidobenzoic acid.—(*Zeitschr. Chem.*, N. F. iii. 564.)

Chlorhydrate of Cyanhydric Acid.—Arm. Gautier. Dry cyanhydric acid is saturated with chlorhydric acid gas at −10° C., and then heated in a closed vessel to 35°—40°. On again cooling the liquid solidifies to a white crystalline mass which is cyanhydric chlorhydrate $CNH + ClH$. This compound decomposes very readily, is soluble in water, alcohol, and glacial acetic acid, insoluble in ether. Sulphuric acid expels chlorhydric acid with formation of sulphate. When heated with glacial acetic acid to 150° or 160° complete decomposition takes place, amongst the products of which formylamide and acetamide were found. When decomposing in presence of alcohol the chlorhydrate of a new base is formed together with ethylic chloride and formic ethide.

$$2N\left\{\begin{matrix}CH\\H\\Cl\end{matrix}\right. + 2\left.\begin{matrix}C_2H_5\\H\end{matrix}\right\} O = CH_5N_2Cl + C_2H_5Cl + \left.\begin{matrix}CHO\\C_2H_5\end{matrix}\right\} O$$

The base cannot be isolated with potassic hydrate, on account of its splitting up into ammonia and formic acid the moment it is set free. The chloroplatinate has the formula $2(CH_5N_2Cl)PtCl_4$. This chlorhydrate is isomeric or perhaps identical with the compound obtained by the addition of chlorhydric acid to ammonic cyanide, and its constitution is represented by the formula—

$$N\left\{\begin{matrix}(CNH_4)\\H\\Cl\end{matrix}\right. \quad \text{or} \quad N\left\{\begin{matrix}CH\\NH_4\\Cl\end{matrix}\right.$$

preference being given to the latter.—*Comptes R.*. lxv. 410 and 472).

Dichlorsulphobenzid.—R. Otto has not been able to confirm Gerike's statements as to the formation of dichlorsulphobenzid, by the action of chlorine or phosphoric chloride upon sulphobenzid, the products of this reaction being, according to the author, chlorbenzol and sulphobenzolic chloride. He, however, succeeded by acting upon monochlorbenzol with sulphuric anhydride at a low temperature. Dichlorsulphobenzid is formed according to the following equation:—

$$3\left.\begin{matrix}C_6H_5\\Cl\end{matrix}\right\} + 2SO_3 = \left.\begin{matrix}C_6H_4ClSO_2\\C_6H_4Cl\end{matrix}\right\} + \left.\begin{matrix}C_6H_4ClSO_2\\H\end{matrix}\right\} O + H_2O$$

This compound crystallises in white needles, fusing at 140—141° C., insoluble in water, readily soluble in hot alcohol or ether. It is not decomposed by an alcoholic solution of potassic hydrate. Sodium-amalgam reduces it principally to benzol and sulphobenzolic acid.—(*Zeitschr. Chem.* N.F. iii., 609.)

CORRESPONDENCE.

ELECTRICAL RESISTANCES.

To the Editor of the Chemical News.

Sir,—The following table of diameters and resistances of pure copper wires may not, perhaps, be unacceptable to electrical students and others.

The capacity of pure copper is taken at 100, and the unit of resistance used is the ohmad adopted by the Electrical Committee appointed by the British Association, one unit being equal to 1,760 yards, or 1609·31 metres of copper wire ·2302 inch diameter.

The value of the table consists mainly in its being a standard by which the student is enabled to ascertain the conducting power of any wire he may have under test, and also of its giving relative lengths of pound and kilogrammes, and proportionate diameters in decimals of inch and millimetres.

It is, perhaps, unnecessary to say that there is no better apparatus for obtaining tests than the electric balance manufactured by Mr. Becker, of Messrs. Elliott Brothers:—

Diameter of inch $\left(d = \sqrt{\frac{C}{l}}\right)$	Diameter of m.m. $(= d \times 25{\cdot}4)$	Number of yards per lb. $\left(l = \frac{C}{d^2}\right)$	Number of metres in 1 kilo. $(= l \times 2{\cdot}016)$	Resistance in ohms. of pure copper (unit of length 1760 yds. or 1609·31 mtrs.)
·2302	5·847	2·095	4·223	1·00
·226	5·740	2·175	4·384	1·038
·198	5·029	2·834	5·713	1·352
·183	4·648	3·317	6·680	1·583
·175	4·445	3·628	7·314	1·731
·160	4·064	4·350	8·75	2·068
·136	3·454	6·007	12·11	2·867
·128	3·251	6·781	13·671	3·237
·107	2·717	9·705	19·555	4·623
·10	2·54	11·11	22·398	5·300
·092	2·336	13·125	26·46	6·266
·08	2·032	17·36	35·00	8·288
·07	1·778	22·67	45·71	10·82
·065	1·651	26·29	53·00	12·25
·0625	1·587	28·472	57·40	13·59
·06	1·521	30·864	62·223	14·73
·058	1·473	33·03	66·588	15·76
·056	1·422	35·432	71·431	16·91
·054	1·371	38·104	76·818	18·18
·052	1·32	41·091	82·839	19·61
·05	1·274	44·444	89·60	21·21
·048	1·219	48·225	97·222	23·02
·046	1·168	52·51	105·86	25·06
·044	1·117	57·39	115·70	27·39
·042	1·066	62·98	126·96	30·06
·04	1.016	69·444	140·00	33·14
·038	·965	77·16	155·50	36·72
·036	·914	85·766	172·91	40·92
·034	·864	95·29	292·70	45·48
·032	·813	108·5	218·74	51·79
·03	·762	123·46	248·90	58·93
·028	·711	141·72	285·71	67·65
·026	·660	164·36	331·35	78·46
·024	·609	192·9	380·26	92·08
·022	·558	229·56	462·80	109·58
·02	·508	277·78	560·01	132·59
·018	·457	342·94	691·36	163·69
·016	·406	434·03	875·00	207·17
·014	·355	569·51	1148·10	270·58
·012	·305	771·60	1555·50	368·30
·01	·254	1111·11	2239·80	530·35
·0095	·241	1231·10	2481·90	587·64
·009	·228	1371·7	2765·30	654·75
·0085	·216	1537·8	3100·20	734·05
·008	·203	1736·1	3500·00	828·67
·0075	.190	1975·3	3982·20	942·84
·007	·177	2267·6	4571·00	1082·4
·0065	·165	2629·9	5300·00	1225·3
·006	·152	3086·4	6222·30	1473·1
·0055	·139	3673·1	7404·90	1753·2
·005	·127	4444·4	8960·00	2121·4
·0045	·114	5487·0	11062·00	2619·0
·004	·106	6944·4	14000·00	3314·7
·0035	·088	9070·3	18285·00	4329·4
·003	·076	12346·0	24890·00	5892·7
·0025	·063	17777·0	35838·00	8485·6

The foregoing table is sufficiently comprehensive, embracing every size of wire likely to be used; but should the student have an intermediate size, he can obtain the diameter for himself as follows:—

d (diameter) $= \sqrt{\frac{C}{l}}$, l being the length in yards of lb., or $d = \sqrt{\frac{\cdot 111111}{28{\cdot}472}} = {\cdot}0625$, corresponding to one of the diameters in table. The value of C is 1-9th, or ·111111.

An easy method of obtaining the conducting power (P) of copper wire, assuming the experimentalist to have ascertained the accurate diameter by the above formula, is to test for resistance (r) by means of the balance above referred to, and the conducting power, $P = \frac{R}{r} \times 100$.

As a familiar example, one mile of ·0625 copper wire has, say, a resistance, r, = 16·5 ohms; the value of R in table = 13·59; hence $P = \frac{R}{r} \times 100 = \frac{13{\cdot}59}{16{\cdot}5} = 82{\cdot}36$ per cent of pure copper.—I am, &c.,

WALTER HALL.

Telegraph Works, Mansfield Street, Borough Road, S.E.

ARSENIOUS ACID.

To the Editor of the Chemical News.

SIR,—Seeing a notice in the CHEMICAL NEWS of last week, on the occurrence of prismatic arsenious acid, I beg to communicate a few observations made by myself in September last, on some peculiar crystals which I found in a flue from a calciner in which copper ores containing arsenic and sulphur are calcined.

The crystals were spear-shaped, not unlike marcasite in form; the lustre was pearly. They were deposited on small crystals (octohedral) of arsenious acid, and on taking them into the air I found that they began instantly to deliquesce.

They were readily soluble in a little hot water, with the exception of a trace of coal dust, mechanically mixed. On analysis, they were found to consist of—

Arsenious acid	75·10
Sulphuric acid	23·00
Sesquioxide of iron	1·25
Coal dust	trace

I imagine the iron to exist as a basic sulphate of sesquioxide.

Perhaps some of your readers will inform me whether such a compound as I have described has ever come under their notice. It is certainly quite new to me.—I am, &c.,

RICHARD PEARCE.

Morfa Works, Swansea, March 17, 1868.

ON THE REACTION OF NITRITES WITH IODIDE OF POTASSIUM.

To the Editor of the Chemical News.

SIR,—In the last number of the CHEMICAL NEWS, Mr. Holland proposes to make use of the well-known reaction between nitrites, iodide of potassium, and sulphuric acid, to determine the amount of nitrous acid present in a water. The reaction referred to is certainly at first sight a most inviting one, and some time ago I made a few experiments to ascertain if a quantitative process could be procured from it; the results I then obtained convinced me that no accurate determination of nitrous acid could be founded upon it. The difficulty is that the reaction is a repeating one: that a minute amount of nitrite is capable (theoretically) of decomposing any quantity of hydriodic acid. I was led to this conclusion by the following experiments:—

To a weak solution of nitrite of sodium was added ten times as much iodide of potassium as there was nitrite present, and sulphuric acid exactly sufficient to decompose the iodide of potassium. After the addition of some starch paste, the iodine liberated was determined by a standard solution of hyposulphite of sodium. This determination of the iodine evolved, it was conceived, would afford the necessary datum for the estimation of the nitrous acid present. After decolourising the solution with the hyposulphite, the blue colour reappeared almost

as strong as ever after a minute's standing. The solution was again decolourised, but the colour reappeared as before. I observed that the blue colour always commenced to show itself on the *surface* of the liquid, and thinking that the oxygen of the air was taking part in the reaction, I filled a test-tube to the brim with some of the decolourised liquid, and inserted a tightly fitting cork, thus excluding air from a portion of the solution. Thus circumstanced, the blue colour did not reappear, while in the remainder of the liquid in the open beaker it was soon as dark as ever.

What is then the nature of the reaction of nitrites with hydriodic acid? Fresenius tells us* that nitrites, acted on by an acid, split up into nitrates and nitric oxide.

$$3(NaNO_2) + H_2SO_4 = NaNO_3 + Na_2SO_4 + H_2O + 2(NO).$$

Nitric oxide is without effect on hydriodic acid, but on contact with air it is at once converted into hyponitric acid (NO_2), and this decomposes hydriodic acid, *nitric oxide being reproduced.*

$$NO_2 + 2(HI) = NO + H_2O + I_2.$$

If, therefore, the nitric oxide were not slowly lost by diffusion into the atmosphere, a minute quantity of it would be sufficient to liberate an infinite amount of iodine. Here, I believe, is the reason of the extreme delicacy of the reaction: the minutest trace of nitrous acid in a water is sufficient, *in time*, to liberate a very distinct amount of iodine.

Mr. Holland says the solution is to be "allowed to stand until the colour is fully developed." Supposing the operator to wait till the greatest depth of colour is arrived at, the tint will depend (if an excess of hydriodic acid has been present) on the rate of diffusion of the nitric oxide into the atmosphere; this will be regulated by the temperature of the solution, the surface it exposes to the air, and by its state of concentration, as the gas will escape more rapidly from a strong solution than from a weak.

It appears then, I think, that the reaction in question does not afford the means for a satisfactory determination of nitrous acid.—I am, &c. R. Warington.

LECTURE EXPERIMENTS.

MELTING METAL IN A HANDKERCHIEF.

We are all familiar with the experiment of wrapping a handkerchief tightly round the bowl of a spoon, and holding the part of the handkerchief thus stretched over a spirit lamp, as an illustration of the conducting power of the metal of the spoon for heat. A more sensational form of the same experiment is to be found in such books as "The Young Man's Book of Amusement," "Endless Amusement," &c., in which a bullet is to be melted in a handkerchief by wrapping it round the bullet, and then holding the enclosed bullet over a candle until melted. On trying this experiment I have failed owing to the difficulty of preventing creases. The following modification of the experiment, however, is easily managed, and is very telling:—Two or three pounds of fusible alloy are melted, and run into an evaporating dish; when cold, a handkerchief is stretched over the smooth convex form thus obtained, and the mass may then be melted over a Bunsen's burner in the course of a few minutes; on piercing the handkerchief the melted metal runs out, and may be received in a mould.

C. J. Woodward, B.Sc.

Midland Institute, Birmingham, March 11th, 1868.

* "Qualitative Analysis," sixth edition, p. 82.

MISCELLANEOUS.

Spectral Analysis and the Bessemer Process.—The application of the spectroscope for conducting the charges in the Bessemer apparatus has become a practical reality. Professor Liellegg, of Gratz, of whose experimental researches with the spectroscope in the Bessemer steel works of the Southern Railway Company of Austria we have only recently given an account, has succeeded in pointing out a sufficiency of marked changes in the spectrum for enabling the managers of these steel works to watch and conduct the charges with the assistance of the spectroscope in preference to the routine previously adhered to at those works, a routine which, as a matter of course, was a strict copy of that existing in the Bessemer steel works of this country. By the aid of the spectroscope the manufacture of Bessemer steel, in the Gratz steel works, has been considerably improved with regard to that exact uniformity of hardness which formerly was more difficult to ensure under all circumstances. The great certainty with which the exact moment of complete decarburisation can be fixed by spectral analysis has reacted upon the amount of care now bestowed upon keeping the percentage of carbon in the spiegeleisen to a uniform or at least to a correctly ascertained amount, and regulating the quantity of spiegel employed by exact calculation. The accidental irregularities and differences of hardness between the different charges have thereby been lessened to a very considerable extent, and an increased reliability has by these means been given to the Bessemer process. Some other Austrian Bessemer steel works, and the Government works of Neuberg amongst these, have sent engineers to Gratz in order that they may subject and introduce it into their own respective establishments, and an account of Professor Liellegg's discoveries has been published in the *Austrian Gazette for Mining and Metallurgy*. The spectrum pointed out by Professor Liellegg, belongs to the flame of carbonic oxide. It can be seen in the flame escaping from the mouth of the converter during the preliminary operation of heating this vessel with coke only; and in that case the lines referred to are very faint, and it requires some practice or knowledge of the precise spots in the spectrum where these bright lines should be looked for, to discover them. During the first period of the Bessemer process the spectrum is very faint. The yellow portion is almost invisible, and even the sodium line is missing; the blue and purple portions are extremely faint. The absence of the sodium line can be accounted for only by the consideration that there is no real flame formed by incandescent gases escaping from the converter at that early stage, but only a mass of sparks carried by the nitrogen from the blast, the oxygen of which remains in the converter, combining with silicium. As the flame gradually appears in the centre of the volley of sparks, the spectrum widens and shows yellow light, until suddenly, the sodium line in the yellow field becomes visible, first only for moments as a flashing bright streak, and after less than one minute as a constant and clearly defined line. The appearance of the sodium line marks the commencement of the decarburisation, although this line does not belong to the charge of iron at all, but rather to the accidental presence of sodium compounds in very minute quantities. It is therefore only indirectly connected with the combustion of carbon; *i. e.*, the appearance of the sodium line is a signal of the completion of the continuous spectrum, and this continuous spectrum belongs to the combustion of carbon. As soon as the sodium line has taken a steady and permanent appearance, the characteristic lines of the carbonic oxide may be looked for in the greenish-yellow, in the green, and in the purple field. In each of these three fields one bright line becomes clearly visible at that time. As the flame increases in size and brilliancy, the spectrum comes out more and more clearly. Bright lines increase in number in each of the

first-named three fields, and ultimately, at the height of the process, some bright lines show themselves in the red and, occasionally, also in the blue field. The green field of the spectrum, however, is the real point of observation in practice, as in this the lines are most clearly visible, and in it they appear first and disappear last. The spectrum, as a whole, is by no means steady or constant, but its fluctuations do not displace any of the bright lines; they only alter the back ground for the continuous spectrum upon which they appear. After the "boil," the maximum intensity is reached; and at that stage, and only with very hot charges, a bundle of bright lines appear in the bluish-purple portion of the spectrum. About four or five minutes before the end of the charge of three tons, the lines begin to disappear in rapid succession, and in the invented order of their appearance—first, the bluish-purple, then the blue lines, after these the red, &c. When the last green line disappears, the vessel is turned, and the charge completed by the addition of spiegeleisen. The yellow sodium line does not disappear to the end of the operation. Sometimes the vessel is turned when all lines in the green field with the exception of two have disappeared. This depends upon the special experience of the case, and it is clear that it is of less importance whether the one or the other mark be taken, if it is only regularly adhered to, and the charge of spiegeleisen regulated accordingly. The practical results are highly satisfactory, since they make the regularity of the "temper" of Bessemer steel practically independent of the skill and experience of the charge-manager, the changes of the spectrum being made more marked and unmistakable than those of the appearance of the flame itself. Hitherto, no experience with British hæmatite irons bas been gained, and the use of the spectroscope in this country must be preceded by some careful trials and observations in order to fix the character of the changes. It is highly probable that they will prove very similar, if not absolutely the same as those observed with Styrian charcoal iron, but mere probabilities are not sufficient in the case like this. If the Bessemer steel makers should gain no more by the use of the spectroscope than the possibility to show to the noisy disbelievers in the uniformity of Bessemer steel that a child may conduct the charge without the least chance of error, just the same as a boy can now work the whole mechanical apparatus of the converters, the gain would be very great. But there is a greater gain immediately to be realised by the use of the spectroscope. The steel-masters will become less dependent upon the skill and attention of their charge-managers or foremen, and the percentage of waste or unsuitable material produced by carelessness or mistakes will be lessened in the general run of practice. —*Engineering.*

NOTES AND QUERIES.

Treating Mineral Oils.—Can any of your correspondents give me a process for treating heavy mineral oils so as to free them from smell, and make them of a bright colour on a manufacturing scale.—A. O. R.

Sugar Refining.—Will you kindly inform me in your "Answers to Correspondents" what becomes of the acetic acid which is set free by the sulphurous acid in "Scoffern's Proccss" for the refining of sugar? As nearly all the acetates are soluble, and free acid in contact with the dissolved sugar would convert some of the cane sugar into grape sugar."—J. DAVIES, Bath.

Sulphite and Hyposulphite of Soda.—In reply to "G. W. R., Liverpool," I beg to say that there is a large demand for hypotulphite of soda by paper makers as so-called *antichlore;* about 200 sons per annum are yearly consumed in photographic operations alone, while a far larger amount is used by paper makers: hyposulphite of soda is also used by bleachers of calico fabrics. Sulphite of soda is of a more limited use, and somewhat superseded by the hyposulphite. —Dr. A. A.

Signs of Rain.—Of the many prophets that appear in the present day, certainly the weather-eyed ones predominate. Of their prophecies, I may dub myself sceptical, as nearly 99·999 per cent of the human world do; for the simple reason that their statements are always the reverse of facts. But there is a sign of rain that is known as true to every observing observer, without the study of the stars, or anything else. I allude to a red sunrise, which invariably denotes rain within a few hours. I wish to ask if any reader of the CHEMICAL NEWS can explain why the sun should rise apparently so red when the atmosphere is in, I suppose, a dense state previous to rain. I remember reading a very interesting paper or lecture in the CHEMICAL NEWS, giving the reason for red sunsets. I read the papers at the time with great relish; but I have been subsequently occupied with other matters, incompatible with the retention of theoretic knowledge, and I have not my volumes of the NEWS at hand to refresh my memory. However, I think my memory is correct enough to be able to assert that the facts, as represented in the papers referred to, do not give any reasons for this atmospheric phenomenon, or any basis for establishing a theory of its cause.—AN OBSERVER OF NATURE.

PATENTS.

Communicated by Mr. VAUGHAN, F.C.S., Patent Agent, 54, Chancery Lane, W.C.

GRANTS OF PROVISIONAL PROTECTION FOR SIX MONTHS.

48. C. D. Abel, Southampton Buildings, Chancery Lane, "An improved method or process for removing sulpher, phosphorus, and other impurities from iron, steel, and other metals."—A communication from J. F. Bennett, Pittsburgh, Penn, U.S.A.

59. G. Davies, Serle Street, Lincoln's Inn, Middlesex, "Improvements in the mode of combining wrought and cast iron or other metals for various useful purposes."—A communication from W. M. Arnold, New York, U.S.A.

64. P. Spence, Newton Heath, Manchester, "Improvements applicable to roasting or calcining copper and other ores containing sulphur and also regulus, and in apparatus connected therewith."—January 7, 1868.

78. W. E. Kenworthy, Crown Point Road, Leeds, "An improved method of purifying drains and sewers."—January 8, 1868.

85. C. J. B. King, Great Portland Street, Middlesex, "An improved process to be employed in the tanning of skins or hides."

86. C. H. Newman, Brentford End, Middlesex, "Improvements in the manufacture of unfermented and unintoxicating malt liquors."—January 9, 1868.

95. J. Fawcett, Kirton in Lindsay, Lincolnshire, "An improved manufacture of cattle food."

96. J. M. Rowan, Glasgow, N.B., "Improvements in the manufacture of artificial fuel."—A communication from J. R. V. S. Traunfels, Vienna.—January 10, 1868.

105. J. Somervell, Kendal, Westmoreland, "An improved method of obtaining and preserving alimentary substances in a highly-concentrated form."

112. T. Whitwell, Stockton-on-Tees, Durham, "Improvements in furnaces."—January 11, 1868.

131. G. Nimmo, Jersey, New Jersey, U.S.A., "An improved composition for furnace linings, fire bricks, pots, crucibles, and other articles.

138. J. Kidd, St. Paul's Wharf, London, "Improvements in obtaining artificial light, and in apparatus employed therein."

139. J. Head, Newport Mills, near Middlesborough, Yorkshire, "Improvements in furnaces for puddling, boiling, melting, or heating iron or steel."—January 15, 1868.

149. J. A. Jones, Middlesborough, Yorkshire, "Improvements in the manufacture of iron and steel."—January 16, 1868.

164. H. Aitken, Falkirk, Stirling, N.B., "Improvements in treating iron ores or iron stones for the purpose of effecting economy in the obtainment of iron and other products therefrom."—January 17, 1868.

MEETINGS FOR THE WEEK.

MONDAY.—Geographical, 8.
TUESDAY.—Royal Institution, 3. Dr. M. Foster, "On the Developement of Animals:"
WEDNESDAY.—Society of Arts, 8.
—— Geogloical, 8. 1. "On some New Forms of Palæozoic Corals," by Henry Woodward, Esq., F.Z.S., F.G.S. 2. "On the Coniston Group," by Professor Harkness, F.G.S., and H. A. Nicholson, Esq., F.G.S. 3. Death of Fishes on the Coast of the Bay of Fundy," by Dr. A. Leith Adams, F.G.S.
THURSDAY.—Royal Institution, 3. Dr. M. Foster, "On the Developement of Animals."
—— Royal, 8½.
—— Zoological, 8½.
—— Philosophical Club, 6.
FRIDAY.—Royal Institution, 8. Dr. Carpenter, "On the Unconscious Action of the Brain."
SATURDAY.—Royal Institution, 3. Professor Roscoe, "On the Non-Metallic Elements."

TO CORRESPONDENTS.

Drs. Wischin and Wilm's telegram from Berlin, asking us not to publish their letter "On the Reduction of Carbonic Acid to Oxalic Acid," arrived on Monday morning—three days too late.

*** Owing to pressure on our space, the further "Answers to Correspondents" will be given in our next.

THE CHEMICAL NEWS.

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ON THE

NATURE AND EXAMINATION OF THE ORGANIC MATTER IN POTABLE WATERS.*

By CHARLES R. C. TICHBORNE, F.C.S., Dublin.

The importance of my subject must be my excuse for bringing before you this rather heterogeneous mass of remarks and notes upon the subject of the Organic Matter in Water. During the cholera visitation of last year, I made some observations in connection with water-analyses, the cream of which experience is embodied in my paper.

It must be palpable to every one, that no individual point in the analyses of waters can give much information as regards their fitness or unfitness for drinking purposes. But it is also quite as evident that the danger (where there is any) lies buried in what the chemists term the organic matter; and that there are times when that organic matter is in a more than usual dangerous state. The point of difficulty in the examination of potable waters, is not so much its quantitative determination as its qualitative. You may have a water rich in organic matter, yet harmless; or a water containing but a minimum of organic matter, but organic matter of such a deleterious nature, that our ideas of the most virulent substance fall short of the horrible results that these invisible poisons can accomplish. Weight for weight, or bulk for bulk, how far behind in virulence are such poisons as hydrocyanic acid, strychnine, or the secretion of the dreaded cobra fangs.

I may say, our opinion upon the state of a water, or rather the state of the organic matter therein (presuming it contains any), is, so far as the chemists have gone, only to be arrived at by the putting together of many examinations. For all the following points bear upon the state of the organic matter, or its qualitative examination.

Total Estimation of the Organic Matter.—If this be present in abnormal quantities, that cannot be explained by its origin, such water should be at once condemned.

Ammonia.—This again depends upon the source from which the water was obtained. We possess a very good test for ammonia. Messrs. Wanklyn and Smith have pointed out some important facts connected with the examination of water; that, on distilling water that contains urea (a probable ingredient in sewage-water), it is converted into ammonia. Thus, an approximate estimation of the nitrogenous substance resembling urea may be made, after making an allowance for the ammonia occurring as such. The experiments of Messrs. Wanklyn and Smith in connection with the nitrogenous matter are well worthy our careful consideration, although we do not agree with them entirely.

A ready and quick mode of estimating the organic matter in solution has yet to be discovered—the estimation by a volumetric solution of permanganate of potassium being worthless.

The *microscopic examination* of any sediment, if there be any present, is of the utmost importance, as often an accidental and recognisable impurity will afford evidence of the inroad of sewage.

Taste, odour, hardness, chlorides, total saline ingredients, sulphuretted hydrogen, relative proportions of the gases dissolved in the water, are all points connected with the state of the organic matters, but call for no particular remarks upon my part.

Another very important point in connection with water intended for drinking purposes, is the colour. Every kind of water, whether from a spring, river, or reservoir, possesses a certain tinge, however faint that tinge may be. An instrument, which I will call the chromiometer, is particularly suited to this purpose. In a paper, which I wrote some years ago, "On the Urinary Pigment" (Chemical News, March, 1862), I described this instrument, and suggested its use for the determination of the relative amount of pigment voided. A similar instrument is now used by Dr. Letheby for the examination of water. I shall describe my own arrangement of this instrument. To observe the colour, a pencil of light passing through a considerable body of the water is viewed by means of a tube. The apparatus consists of this tube, a yard in length, well closed at one end by a stopper. The stopper is preferable to a permanently fixed end, as it enables the operator to clean the tube perfectly—a matter of some importance. On the bottom is fixed a piece of white porcelain—a sight, if I may use the term. The tube is graduated into convenient divisions for the relative examination of different samples. By filling the tube with the water to be examined, and looking through the water at the white disc at the bottom, a faint colouration is at once perceived. Thus an experienced eye will, after a chromatic examination, prognosticate the character of his microscopic examination. The green chlorophyll tinge is almost invariably produced by the presence of desmidiaceæ and other algæ, or similar bodies. A white opacity is frequently indicative of fungoid growths. The finely suspended basic persalts of iron frequently found in waters rich in organic matter, which pass through iron-pipes, etc., are instantly recognised by a peculiar ochrey colour. True chalybeate springs also deposit a similar precipitate when exposed to atmospheric oxygen. Another important application of this tube is the determination of the state of oxidation of the iron-salts in the water, if present. This is a matter of some importance, particularly in connection with the organic matter; and I shall perhaps be excused if I dwell a little upon this point.

Iron is almost always present in well-waters, and frequently so in river or reservoir water. If present in very minute quantities, the primitive state of oxidation can only be recognised with the aid of the chromiometer.

As the microscope magnifies the form, so does the chromiometer magnify the colour so as to be recognisable by the eye. We are enabled to extend the range of our ordinary chromatic tests, and to recognise minute quantities of the proto- and per-salts of iron. Any attempt to concentrate the water for examination would alter the state of oxidation, and the results would be fallacious. Thus, on adding to separate portions of the water in the tube a few drops of the ordinary reagents, the following results are obtained. Sulphocyanide of ammonium developes a pink tinge, indicative of the persalts of iron. A few drops of nitric acid are added to another specimen, and tested with sulphocyanide of ammonium. A darker shade is produced than in the first experiment. This is indicative of the presence both of per- and proto-salts of iron. Colouration, only produced after the addition of nitric acid and sulphocyanide, indicates that the iron is present as a proto-salt. When the iron bears a considerable proportion to the organic matter in the water, little or no organic matter, if we except ammonia, will be found in solution, providing the iron is in the state of a per-salt. If, however, the iron be present as a proto-salt, it will exert very little influence on the organic matter, and frequently such waters (except deep chalybeate springs) will be found to contain large quantities of soluble organic matter. Thus it is, that a water containing proto-salts of iron and organic matter will frequently, although quite clear on drawing from the well, become cloudy and deposit a mass of red flakes. They consist of a basic salt of iron, which retains the whole, or nearly the whole, of the organic matter. The trace left in solution is of an unfermentable nature.

* Read in the Physiological Section, before the Annual Meeting of the British Medical Association in Dublin, August, 1867.

The estimation of nitrites is one of importance. It is easily and readily performed in the following manner. I am induced to insert it here, as it differs from all the methods in use. It might at first sight appear strange that I should advocate the use of a volumetric solution of permanganate of potassium, when, in another part of my paper, I heartily condemn that reagent; but it will be also seen that there are two exceptions to the uncertainty of the action of permanganate of potassium; viz., oxalic acid and the nitrites. There are many operations in vogue for estimating nitrates; two of which the author proposed some years ago. ("On the Estimation of Nitrites in the presence of Nitrates," *Proceedings of the Pharmaceutical Conference*, Birmingham, 1865.) All of these methods require more or less care, combined with a considerable amount of manipulation. The following process is particularly applicable to potable waters as the nitrites therein are in a very diluted state. The method is based upon the reaction that nitrite of ammonium is on heating split up into nitrogen gas and water ($NO_2NH_4 = 2N + 2H_2O$). Eight ounces of the water to be examined are boiled, and, after cooling, are made up to the original bulk with pure water. The readily oxidisable matter in this measured portion is then estimated with volumetric solution of permanganate of potassium according to the directions given by Dr. Miller ("Observations on some Points in the Analysis of Potable Waters," by Professor Miller—*Journal of the Chemical Society*, vol. 3, new series, p. 119), *i.e.*, the volumetric solution is added a few degrees at a time, at short intervals, until a permanent pink tinge remains. We will suppose that the sample of water under examination used up 30° of the permanganate solution. The thirty degrees were consumed in the oxidation of all the nitrites, *plus* some of the more readily oxidisable products. To a separate eight ounces of the water is added a few drops of a solution of pure sulphate of ammonium, and the whole is evaporated to dryness at a temperature below 100° C. The residue is again dissolved in 8 oz. of pure water, and estimated under exactly similar circumstances with the volumetric solution of permanganate of potassium. I will suppose that only 10° are now decomposed. These 10° represent readily oxidisable matter—the difference in the two estimations—*i.e.*, the 20° represent the nitrous acid present, which has been entirely destroyed and dissipated as nitrogen and water.

It must be self-evident to chemists, that to look for a specific test for miasma in water is absurd; that there are certain subtle substances of intense power which are physically unrecognisable—substances that, so far as we have gone, no balance can weigh, no microscope can see. Some time ago, there appeared an elaborate paper from M. Stas. M. Stas has made a great name for himself; he is deservedly looked upon as one of our best chemists, and he has made his name by the exquisite care with which he conducts his experiments. He found out what most chemists have found out—how very difficult it is to get pure water. Simple distillation will not do it. He therefore proposed to digest the water upon manganate of potassium—to destroy the organic matter. But I have here a water which has been treated as he directs; and I have even gone further with these experiments, for one of these specimens is now standing upon manganic acid itself—the most powerful oxidiser we have. One is water which had originally been distilled from a few grains of musk, and the other from fusel oil. It will be seen from the odours that one of these substances has escaped the oxidation entirely, and that the other has merely absorbed oxygen to be converted into another substance even more disagreeable than that originally contained therein, namely, valerianic acid. The late researches upon limited oxidation by Messrs. Wanklyn and Chapman show that the results of the action of oxidisers upon organic matter in solution are complicated, and entirely differ in their character from a perfect combustion. Therefore M. Stas's assumption, that nothing is present in water distilled from manganate of potassium, must be fallacious. This naturally brings me to the consideration of the utility of permanganate of potassium, which has been fashionable until lately for determining the amount of organic matter in water. It has been the opinion of most practical chemists who have had much experience with water, that that test was worthless as a measure of organic matter. A chemical friend of mine remarked to me, "It is the colour that does it. If it were not for its beautiful colour and the prettiness of the reaction, no one would use it." And I really think there is something in the remark.

Dr. Frankland was, I believe, the first to enter his protest (in type) against the use of this reagent as a measure of organic matter "Lectures on Water" (Royal Institution, 1867); although, as far back as 1866, I, in the presence of Dr. Mapother, practically illustrated its worthlessness at a public meeting in this city, when the experiments were entered upon the minutes of the meeting. I merely mention this fact in case the experiments which I give below might be considered as a babbling echo of what has, perhaps, been so much better put to the public; but I insert them here from two reasons. The first of these is, that many of the substances upon which I have experimented, although they are not given by Dr. Frankland, are yet typical of the organic matter likely to be met with in potable waters; secondly, that some curious points are illustrated by my experiments, which would not be foreseen by the scientific man.

A large number of experiments were instituted under exactly similar circumstances, for the purpose of observing what class of organic substances were readily oxidised, and how far their state of combination might affect the change. To effect this, the process recommended by Dr. Miller was fixed upon as the most suitable for watching the relative action. A volumetric solution of permanganate of potassium was used, which corresponded to ·22 oxalic acid. The different solutions were made by dissolving in each case ·2 of a *gramme* of the organic matter in (568 c.c.) an imperial pint of pure water. A degree of the permanganate solution was added at intervals of a quarter of an hour as long as any decolouration took place, half an hour being the crucial test allowed. The temperature was in each case 60° Fahr., or as near as it was possible to conduct the experiments. The quantity operated upon was eight ounces. Sulphuric acid was used in every case for acidulating the solutions; soda for rendering them alkaline. From above one hundred experiments, the following are selected as typical.

No.	Substance.	In an acidulated solution.	Rendered faintly alkaline.
1	Oxalic acid	36° (reaction very marked and decided)	No reaction.
2	Nitrite of potassium	(Reaction well marked and decided)	No reaction.
3	Nitrate of ammonium	No reaction	No reaction.
4	Chloride of ammonium	No reaction	No reaction.
5	Ammonia	No reaction	No reaction.
6	Trimethylamine	35°	[action
7	Nicotine	2°	Very decided re-
8	Conium	4·5°	
9	Aniline	3·14°	
10	Urea	No reaction	No reaction.
11	Uric acid	26°	52°
12	Urinary pigment	4·5°	4·5°
13	Hydrocyanic acid	4·5°	54°
14	Strychnine	35° (not finished, as the action could not be followed).	More decided
15	Sugar	1°	1°
16	Albumen (partially decomposed)	38°	1°
17	Albumen (coagulated)	38°	
18	Oxalic and urea	18°	
19	Lactate of lime = *gramme* ·2 Lactic acid	No reaction	
20	Butyrate of lime = *gramme* ·2 Butyric acid	No reaction	2°

In most cases, the products are more easily oxidised accordingly as their chemical affinities are disengaged

or not. This is illustrated in Experiment 7 (nicotine), where its marked basic properties affect materially the oxidising influence. Urea, sugar, &c., which we should naturally expect to be so easily decomposed, have no action under these circumstances with permanganate of potassium. The action with hydrocyanic acid is curious; but it is probable that the number of degrees required in an alkaline solution is merely due to the conversion of a cyanide into a cyanate. Experiment 18 was tried to find if the decomposition or the decomposing influence of one substance might be transferred from one substance to the other; but, with a few exceptions, this was not found to be the case. The degrees required were simply what would be consumed by each ingredient. It must be borne in mind that temperature influences the activity of the permanganate of potassium; that many substances that are not acted upon in the above experiments are de-decomposed at a more elevated temperature. These experiments are, in my opinion, useful, from the fact that they probably enable us to watch changes similar to those that are carried on from atmospheric oxidation. Changes which are too slow and gradual to permit of a practical examination may, perhaps, be fathomed by such experiments as those detailed. They are, therefore, worthy of a further extension. It was found that waters containing different organic substances, when allowed to decompose, do not give rise to the same microscopic phases of life; but certain substances seem conducive to certain species. It would, however, extend this paper too much to enter into the microscopic examinations.

I am of opinion that peroxide of hydrogen is a substance which ultimately may be used with advantage for the purification of drinking water. It may be viewed as water supersaturated with oxygen, the extra equivalent of oxygen being held by the faintest phase of attachment coming within the term of chemical attraction; so that the least disturbing agency imaginable would decompose it into oxygen, ozone, and water. Peroxide of hydrogen oxidises organic matter, and stops fermentation: therefore, providing that we had a pure article, we could oxidise the organic matter, without introducing anything but pure water. But at present peroxide of hydrogen is very dear, and, as found in commerce, is much too impure to be used in drinking water. Another doubtful point is, that this curious substance sometimes seems to play the part of a reducing agent, as well as an oxidiser. If so, sulphuretted hydrogen might be generated by its action upon sulpho-compounds, such as albumen.

It has been lately stated that charcoal will not, after a little time, purify drinking water; and that, so far from taking from the water organic matter, it gives up again a certain amount. Thus: that water, on analysis before and after passing through charcoal which had been some time in use, was more contaminated with organic matter after having been passed through the said filtering medium. If it were not for the fact that the paper containing the above statement had received a considerable amount of commendation, and, *ergo*, publicity, I should have passed it by. But the author of that paper evidently misunderstood the position. The *modus operandi* by which charcoal acts on oxidisable organic substances is not so much by virtue of any attractive or selective power that it possesses; but, as a carrier of oxygen in a concentrated form, it is one of the most powerful oxidisers we possess. The oxygen condensed within the charcoal acting more energetically than the available oxygen, we can apply in the form of permanganate of potassium. The original experiments of Dr. Stenhouse in connection with the action of charcoal have received ample confirmation from the late investigation of Professor Calvert (*Journal of the Chemical Society*, June, 1867).

It is true that charcoal, after a certain time, becomes effete—its activity destroyed; but it is equally wonderful to observe the length of time a charcoal-filter with the following provisions will retain its vitality, if I may use the expression. 1. That the water passed through it does not contain a very large percentage of organic matter; 2. That it is drained from the water the better part of each day, so that the atmospheric oxygen (with as little of the dust as possible) may have access to it. Charcoal, under these circumstances, will be found to do its work well. You must give it its food quietly, so that it can digest it.

I can hardly believe the statement made, that charcoal will, after a certain period, transfer again organic matter to water. I have examined water before and after passing through a charcoal filter which had been in daily use in my own house over a month. There was not much difference, it is true; but the water which had passed through the filter had most decidedly the advantage. I can well imagine that, as the charcoal acts as a mechanical recipient to the insoluble organic matter, that substance may at last accumulate to such an extent as to enter itself into a state of fermentative change. The activity of the charcoal, being by this time exhausted, or at least only sufficient to supply a minimum of oxygen, would only assist such a decomposition. This state of the case would be simply the putrefaction of a mass of solid organic matter independent of the charcoal—not the rendering back from the charcoal of something it had absorbed from the water. I would suggest that a well-constructed water-filter should have an arrangement by which the insoluble organic matter should be separated before the water comes into contact with the filter. In such a filter, the organic matter could never accumulate.

In conclusion, I may point out that the most valuable method of examining water is that which I believe was first used by Dr. Frankland—that is to say, to examine the relative amount of oxygen and nitrogen found in water. In absorbing atmospheric air, the oxygen is dissolved with a little greater avidity, so that that gas is found in water in a larger relative proportion than in atmospheric air; viz., the relative proportion of oxygen to nitrogen is about 21 to 79; but as found in water it is about 32 to 68. Now, if the enclosed air is found to contain less oxygen, it shows that that element is being consumed by chemical changes going on within the water; or, in other words, such a water is not in its normal state, and therefore is unfit for general use.

The aqueducts of antiquity show how important the blessings of pure water were considered from time immemorial, and how necessary to the welfare of all communities. But I cannot see that we have improved upon our forefathers; for, whilst they spent enormous sums to produce stupendous supplies, we seem to me to spend our money in merely building immense reservoirs. Anything is, however, better than that "like a dog we should return to our own vomit."

> "Most blessed water, neither tongue can tell
> The blessedness thereof; no heart can think,
> Save only those to whom it has been given,
> To taste of that divinest gift of heaven."

An Alleged Preservative against the Cattle Plague.—Chloride of copper is now extensively used in Germany against the cattle plague, or rather as a preservative. The *modus operandi* is as follows:—Take green crystallised chloride of copper, 8 grm.; spirits of wine, 2 kilog., and dissolve. With this solution impregnate a pad of cotton, lay it on a plate, and set fire to it in the centre of the stable, turning the animals' heads towards the flame, so as to make them breathe the fumes. This operation is performed morning and evening, burning one pad for every three heads of cattle. At night, a spirit-lamp, filled with the solution, is lighted in the stable. To prevent accidents, the flame is surrounded with wire-gauze. The liquid is also administered internally, with the addition of 15 grm. of chloroform for the above quantity. A tea-spoonful of this is put into the animal's drink three times a day. As a further precaution the litters are watered with the same solution.

ON THE FORMATION OF

A SERIES OF DOUBLE SULPHOCYANIDES OF CERTAIN OF THE ALKALOIDS

WITH THE METALS

ZINC, TIN, MERCURY, AND MOLYBDENUM.

By WILLIAM SKEY,
Analyst to the Geological Survey of New Zealand.

WHILE engaged in testing some of the chemical properties of the alkaloids, in relation to those of the inorganic bases, a new set of reactions have just discovered themselves, which I will briefly note preparatory to a further communication thereon. These reactions consist in the formation of precipitates, when an acid solution of certain of the alkaloids is brought in contact with a solution of a salt of zinc, tin, mercury, or molybdenum, in presence of hydro-sulphocyanic acid.

To avoid the formation of single sulphocyanides, it is best to employ solutions of the metals and alkaloids of such strength that a sulphocyanide makes no precipitate in either solution separately.

The precipitate formed by nicotina, mercury, and sulphocyanogen, in presence of each other, by quina, sulphocyanogen, and zinc, or mercury, and by strychnia, zinc, and sulphocyanogen, was found to yield these several substances respectively; the remaining precipitates I have not yet had time to examine, but pending this and the quantitative analysis of some, I will for the present suppose all these precipitates to be compound sulphocyanides.

Generally these compound sulphocyanides are very insoluble in cold water, more soluble in hot water, and freely soluble in alcohol; they are but little affected by hydrochloric or sulphuric acids,—but are decomposed by alkalies,—while their physical properties are in some instances very characteristic.

The following are details respecting the more interesting of them:—

Sulphocyanide of Strychnia and Zinc separates as a gelatinous mass, but gradually assumes the form of long acicular crystals. Sulphocyanide of strychnia and mercury is crystalline (it is soluble in sulphocyanide of potassium).

Sulphocyanide of Quina and Zinc is solid and brittle at 70°, soft and plastic at 90°, and changing to a fluid, or semi-fluid substance at about 200°, which appears to crystallise on cooling.

Sulphocyanide of Nicotina and Zinc is crystalline, while the tin, mercury, and molybdenum salts are oils at common temperatures; they are nearly colourless, with the exception of that of molybdenum, which is of a rich purplish red colour.

The Sulphocyanide of Atropia and Tin is a semi-solid fat at 60°, while the zinc, mercury, and molybdena salts are oils; the last is of a dark red colour.

Sulphocyanide of Morphia and Zinc or Tin was obtained in amorphous forms; they fuse at a very slight elevation of temperature; the mercury compound is an oil.

Sulphocyanide of Narcotine and Mercury is crystalline, and easily fusible.

Sulphocyanide of Veratria and Zinc, Tin, Mercury, and Molybdenum resemble the sulphocyanide of veratria in the gelatinous appearance they assume.

Sulphocyanide of Conia and Mercury is a green crystalline substance.

The remaining natural alkaloids have not yet been tested, but sufficient is shown to make it probable that this property of forming double sulphocyanides with certain of the metals, is possessed by all.

With regard to the metals thus connected together by these reactions, I would observe, the only character common, and at the same time, to some extent, peculiar to all, is that of the ready fusibility and volatility of their chlorides. I may state that aniline does not give any precipitate when substituted for the alkaloids.

The feebly basic nitrogenous substances, gelatine and isinglass, behave like veratria.

For the detection, separation, and determination of certain of these alkaloids, it is possible these reactions may be advantageously employed.

ON THE USE OF METHYLATED SPIRIT IN PHARMACY.

THE following paper is a condensed translation of a report made to the Medical Council of the province of North Holland, by six of its members, among whom was Prof. Dr. J. Gunning. In accordance with the excellent laws which, since January, 1866, regulate in the Netherlands all matters relating to medicine in its more extended sense, there is in every province [county] of the kingdom a medical council composed of medical men, pharmaceutists, and lawyers. As regards the free use of methylated spirit by pharmaceutists, the committee just alluded to is of opinion that it would be manifestly unfair to pharmaceutical chemists who are in the habit of preparing, or manufacturing, for instance, such substances as quinine and other alkaloids, to require that such articles should, as far as such is required, be made by them with alcohol, whereas the wholesale maker would use, and quite justly so, methylated spirit. The committee, however, distinctly desire it to be understood that the use of methylated spirit cannot be allowed in the preparation of medicinal tinctures, for although it is true that the methyl-alcohol, as it is met with in ordinary wood-spirit, bears the greatest analogy to ethyl-alcohol, there occur beside in wood-spirit, acetate of methyl and aceton, both of which, in their solvent power, more resemble ether, and, consequently, influence and alter the real constituents of tinctures to be prepared with alcohol; the same, of course, applies to alcoholic extracts. The committee also disapproves of the manufacture of ether and chloroform for medicinal use from methylated spirit. Since the inspection of chemists' shops in the Netherlands, and the testing of the divers pharmaceutical preparations is a duty of the Medical Council's, it was necessary to find ready tests to ascertain whether or not methylated spirits have been unlawfully applied. The following are the results of some experiments instituted on purpose by the members of the above-named Committee. It is quite possible to recognise even in tinctures which contain strongly-scented substances, the wood-spirit, if methylated spirit was used in the preparation thereof; the smell is even detected three months after the tinctures have been made, but if a doubt arises, it is best to mix the tincture in question with double its bulk of boiling water. Tinctures containing free ammonia beside must be first rendered neutral. Another test is the following:—The alcoholic fluid in question is mixed with twice its bulk of strong ammonia.

Next there is added, while the fluid under examination is well stirred up, a few drops of a solution of 10 grs. of iodine and 20 grs. of iodide of potassium, in half a fluid ounce of distilled water. In case the fluid under examination does *not* contain methylated spirit, there will soon be observed a finely-divided precipitate of a black substance (iodine?), giving to the fluid a dark bluish appearance; if, on the other hand wood spirit or methylated spirit is present, the fluid remains clear, assumes a brownish yellow, but rapidly again vanishing hue; after the fluid has become quite colourless again, there will distinctly be perceived, on smelling it, an odour of saffron, while shortly after, also very frequently, crystals of iodoform are deposited. This test and reaction are discovered by Mr. J. Polak. Tincture of iodine made with methylated spirit may be detected, since on addition of liquid ammonia it becomes readily and without application of heat, discoloured, the saffronaceous odour will be smelt, and

crystals of iodoform deposited. The above-named test is not disturbed by the presence of essential oils, camphor, compound ethers, &c. It is best, however, that, as regards the application of this test to tinctures, the latter should be submitted to distillation, and the distillate tried by the reagent. From a series of interesting experiments instituted by the committee, in order to test in how far methylated spirits might change the constitution of alcoholic extracts made with methylated spirit instead of with pure alcohol, it appears that methylated spirit dissolves out from 2 to 7 per cent more from vegetables than alcohol does, while in the case of extracts of cicuta and belladonna the amount was from 13 to 14 per cent more if methylated spirit instead of pure alcohol was applied. The following are the results of experiments instituted with ether, æther muriaticus alcoholicus, æther aceticus, æther nitricus, alcoholicus, and chloroform made with pure alcohol and methylated spirit.

Ether from methylated spirit cannot be detected by the smell, but easily by the following test:—Pour carefully some strong sulphuric acid in a test tube, hold it then as slantingly as possible, and then pour in as carefully as possible some of the ether; if the latter is obtained from methylated spirit, it will be seen that at the place of contact of the two fluids, a dark brownish yellow colouration ensues, which, if the ether were obtained from pure alcohol, and submitted to a similar experiment, will be found absent, or at least hardly perceptible. Æther muriaticus, alcoholicus, and æther nitricus alcoholicus can at once be detected by the iodine test spoken of before if they have been prepared with methylated spirit.

Æther aceticus, prepared either with pure alcohol or with methylated spirit, is not recognised by the smell, but the iodine test detects the origin from methylated spirit at once, and it hence follows that acetic ether obtained from methylated spirit contains acetate of methyl and also aceton.

Chloroform, if prepared with methylated spirit, may be recognised by the smell, which is different from that of chloroform obtained from pure alcohol; beside this, the discolouration with sulphuric acid takes place with chloroform as with ether made from methylated spirit.

ON A

NEW GENERAL METHOD OF VOLUMETRIC ANALYSIS.

By WOLCOTT GIBBS, M.D.,
Rumford Professor in Harvard University.

IN a memoir on the quantitative determination of nitric acid, H. Rose* suggested that in particular cases the metal in the nitrate might be precipitated by means of sulphydric acid, and the nitric acid set free determined in the filtrate by volumetric methods. So far as this application of the volumetric analysis is concerned, Rose's method appears not to have been carried out in practice or even supported by actual experiment. It occurred to me that the method might be generalised so as to form the basis of a new application of the processes of acidimetry, and the following analyses will serve to show the degree of accuracy which may be attained. When the salt to be analysed contains a fixed acid which does not act upon sulphydric acid gas, a weighed portion is to be dissolved in water, the solution brought to a boiling heat, and a current of sulphydric acid gas passed through until the metal is completely precipitated. When quantities of about 5 grammes are employed the precipitation is usually complete in half an hour. The precipitate may then be allowed to settle, and a drop of the supernatant liquid taken out with a glass rod, and tested upon a white porcelain plate, with a drop of a saturated solution of sulphydric acid in water, or with any other reagent which may be specially adapted to the metal in the salt examined. The precipitation being complete the liquid is filtered upon a ribbed filter, the filtrate and the washings allowed to flow into a half litre or litre measure, and the washing with hot water continued until a drop of the filtrate no longer exhibits an acid reaction. The liquid is then allowed to cool, and the volume made up to exactly a half litre or litre by the addition of water. After thoroughly mixing the contents of the measure, fifty or one hundred cubic centimeters are to be taken out, a few drops of a solution of cochineal or logwood added, and the free acid determined by means of one-tenth normal ammonia in the usual manner. The first determination is to be used simply as a guide. Two or more successive portions of the acid liquid may then be taken out and determined successively, and the mean of several determinations obtained. With very little practice the results will be found to correspond to one-tenth c.c. when a burette with Erdmann's swimmer is employed. From the quantity of ammonia required to neutralise the acid, the quantity of acid, and in many cases also of base, in the salt may be readily calculated.

With crystallised sulphate of copper the following results were obtained.

Grns. sulphate.		Per cent acid.		Per cent base.		
1·8435	.. gave	31·89	..	31·70	..	(Sharples)
3·7183	.. ,,	32·14	..	31·93	..	,,
8·3955	.. ,,	32·10	..	31·89	..	(Tower)

The formula $CuSO_4 + 5aq$ requires (Cu = 63·50).

		I.	II.	III.
CuO,	.. 31·86	.. 31·70	.. 31·93	.. 31·89
SO_3,	.. 32·07	.. 31·89	.. 32·14	.. 32·10

The first analysis was made with a commercial sulphate, the other, with a pure salt prepared from electrotype copper. In crystallised sulphate of copper and potassium, ($CuSO_4 + K_2SO_4 + 6aq.$) 2·6601 gr. gave 18·23 per cent acid and 18·11 per cent of oxide of copper. The formula requires

		Found.
CuO,	.. .. .. 18·00	18·11
SO_3,	.. .. .. 18·12	18·23

In the memoir already referred to, Rose points out the necessity of diluting the solutions of metallic nitrates to such a degree that the nitric acid set free shall not act sensibly upon the sulphydric acid.

In the experiments made in my laboratory to test the method this precaution was not found to be sufficient. Thus with crystallised nitrate of lead, Mr. Sharples obtained the following results.

I. 2·147 grammes of salt were dissolved in 200 c.c. water, and the lead precipitated from the boiling solution by sulphydric acid gas. The filtrate was made up to 500 c.c., of which three portions containing each 75 c.c. were taken for titration, and each required 18·2 c.c. of ammonia. This gives 30·51 per cent of nitric acid, while the formula $Pb(NO_3)_2$ requires 32·61 per cent.

II. 2·4992 of the nitrate were dissolved in 500 c.c. of water and treated as above, only the lead was thrown down in the cold by sulphydric acid, and the excess of the latter expelled from the filtrate by boiling. The acid found corresponded to 32·12 per cent in place of 32·61. From this it is clear that even dilute nitric acid acts too powerfully upon sulphydric acid to permit a very accurate determination of the former under the circumstances of the experiment. Precipitation from a boiling solution is necessary because the filtrate is then at once free from sulphydric acid.

To obviate the difficulty arising in the case of nitric acid it occurred to me to add to the solution of the nitrate a portion of a neutral salt containing a fixed organic acid, an equivalent quantity of which would be set free by the combination of the free nitric acid with the base

* Pogg. Ann., B. cxvi., p. 125.

contained in the salt. This method was found to give perfectly satisfactory results, as the following analyses by Mr. S. P. Sharples will show.

4·409 grms. of nitrate of lead were dissolved in 200 c.c. of water, five or six grammes of pure Rochelle salt added, and the lead precipitated as above. The quantity of acid found corresponded to 32·58 per cent. The formula $Pb(NO_3)_2$ requires

		Found.
N_2O_5	32·63	32·58
PbO	67·37	67·27

0·9380 grm. of nitrate of bismuth were treated as above, Rochelle salt being added. The nitric acid found, corresponded to 33·40 per cent and the equivalent quantity of oxide of bismuth to 47·82 per cent. The formula $Bi(NO_3)_3+10$ aq. requires

		Found.
N_2O_5	33·47	33·40
Bi_2O_3	47·94	47·82

5·6553 gr. of chloride of mercury were treated as above, 6 or 8 grm. of Rochelle salt being added to the solution. The free acid corresponded to

	Calc.	Found.
Cl_2	26·20	26·10
Hg	73·80	73·90

When chlorine is separated in the form of chlorhydric acid the volatilisation of the acid in the process of boiling is completely avoided by the addition of the organic salt. The same remark applies to nitric acid, though it is probable that the principal cause of loss in this case is the action of the acid at a boiling heat upon the gas passed through the liquid. The precipitation of a metal by sulphydric acid is usually much slower when boiling solutions are employed.

The analyses given above show that, under favourable circumstances, the method employed is capable of giving satisfactory results. It is easy to see that it applies in the case of soluble definite compounds of all those metals which are easily and completely precipitated from boiling solutions by means of sulphydric acid gas. When the compound to be analysed contains an excess of free acid, not combined with the oxide of the metal to be determined, this must be first separated by evaporation to dryness in the usual manner. The presence of alkalies and alkaline earths is of course without influence on the result, but, on the other hand, even very small quantities of iron, alumina, and various other bases, make it almost impossible to determine the point of saturation with precision, these oxides in solution giving, with cochineal and logwood, specific reactions not easily distinguished from those produced by the alkalies in excess. For this reason the method does not apply when oxides of this class are present, and this case is precisely that which most frequently occurs in practice. The method will be applicable to all these cases if hereafter a colouring matter should be discovered sensitive only to acids or bases in excess, but not producing specific colouration with salts which are neutral in constitution. It is possible that cyanin, the remarkable properties of which have been described by Schönbein, or the rosocyanin of Schlumberger,* may fulfil the condition indicated, but I have had no opportunity of experimenting with either.

The method of precipitation above described may be used with advantage in preparing a pure normal acid for titration. Pure crystallised sulphate of copper is to be powdered and heated in a porcelain crucible placed within a Hessian crucible for about an hour, the temperature being gradually raised and not allowed to exceed a low red heat. The anhydrous sulphate is then, while still hot, to be transferred to a perfectly dry glass tube, which can be closed with a good cork covered with tin foil. After cooling, the tube is weighed, the contents poured into a flask, the salt dissolved in water, and the copper precipitated at a boiling heat as above. The filtrate and washings are then to be made up to a known volume. From the weight of the anhydrous sulphate employed, the quantity of sulphuric acid present in the solution is known. In experiments made in this laboratory by Mr. R. Chauvenet, this method has been found very accurate and expeditious.—*American Journal of Science and Arts.*

* Bull. de la Société Chimique, Mars, 1866, p. 194.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, March 19th.

DR. WARREN DE LA RUE, F.R.S., &c., President, in the Chair.

THE minutes of the previous meeting were read and confirmed, and the donations to the library were announced.

The following gentlemen signed the statute book, and were formally admitted as Fellows of the Society, viz., Professor Hermann Kolbe, of Leipsig; Dr. B. H. Paul, Messrs. Herbert M'Leod, J. R. Carulla, and Peter Griess.

Lieutenant Francis C. H. Clarke, Royal Artillery, Staff College, Farnborough, was proposed for election, and the names of the following gentlemen were read for the second time:—John Tyndall, LL.D., F.R.S., &c., Royal Institution of Great Britain; Frederick Guthrie, Ph.D., F.R.S.E., Lecturer on Chemistry at the Royal College of Mauritius; William Brantingham Giles, Old Swan Borax Works, Liverpool. The ballot was taken for the following gentlemen, all of whom were declared unanimously elected; Mr. R. Calvert Clapham, Walker Alkali Company's Works, Newcastle-upon-Tyne; Dr. Rustomjee Byramjee, Assistant-Surgeon in Her Majesty's Bombay Army; and Dr. Meuzel, who was elected an associate.

The names of officers and members of Council proposed for election at the anniversary meeting on Monday, 30th inst., were suspended in the meeting room.

Professor Kolbe was then invited by the President to favour the Society by giving an account of his experiments on the "*Conversion of Carbonate of Ammonia into Urea.*" The statement made by Professor Kolbe had reference to the production of artificial urea by heating in sealed tubes dry carbonate of ammonia to a degree of temperature a little lower than that at which the urea formed would be again destroyed. The speaker likewise referred to the electrolysis of acetic acid which furnished a new acid isomeric with glycolic acid, but of which the properties were as yet but imperfectly known.

Dr. FRANKLAND briefly interpreted Professor Kolbe's remarks, and alluded to the interest attaching to the investigation of the new isomer of glycolic acid, which might possibly throw light upon the question of the equality of the value of the four bonds of carbon in these bodies.

Mr. HENRY CHANCE, M.A., then delivered a most interesting lecture "*On the Manufacture of Glass,*" an account of which is unavoidably postponed until next week. The author briefly sketched the history of this manufacture, and quoted several analyses of various kinds of glass. The action of heat in causing devitrification, and of sunlight as affecting the colour, besides other considerations having reference to permanence, were discussed. Mr. Chance appended to his remarks upon glass a statement of his mode of treating the Rowley Rag basaltic rock of South Staffordshire. This material gives by fusion a black obsidian-like glass, which again devitrified furnishes a material suitable for building purposes, and capable of ornamental application. The formation of soluble silicate

of soda by Gossage's process was described, and some of the corroded flints exhibited. An excellent series of samples illustrative of the manufacture of glass, and of the two materials producible from Rowley Rag, were laid on the table for inspection.

A short discussion followed, in which Drs. Frankland, Williamson, Miller, Hugo Muller, and Messrs. Dallmayer, Church, and David Forbes, took part. The accommodation afforded by the meeting-room was as usual on this lecture evening taxed to the utmost. After votes of thanks had been passed both to Professor Kolbe and Mr. Chance, and Messrs. Duppa and Mills had been appointed auditors, the meeting was adjourned until the anniversary on Monday, 30th inst., when the President and Treasurer would present their reports.

At the next ordinary meeting, on Thursday, April 2nd, Professor Church will read a paper entitled, "*Chemical Researches on some New and Rare Cornish Minerals*;" and there will be other papers, by Messrs. Perkin and Duppa, "*On the Constitution of Glyoxylic Acid*," and by Dr. Odling, "*On Glyoxylic Amide*." On the second ordinary meeting in April, Professor Guthrie will read a paper "*On Graphic Formulæ*."

GLASGOW CHEMICAL SOCIETY.

On Monday evening last, a meeting, embracing a large representation of the chemists of Glasgow, was held in the Philosophical Society's hall, for the purpose of forming a local Chemical Society. In the absence, from illness, of Dr. Anderson, Professor of Chemistry in the University,

Wm. McAdam, Esq., of Hyde Park Pottery and Bottle Works, occupied the chair, and briefly stated the object of the meeting, and then called upon

Mr. E. C. C. Stanford, F.C.S., manager of the British Sea-weed Company, who explained at some length the character of the proposed Chemical Society. He stated that the want of such a Society had long been felt among the chemists of Glasgow, and that as the Philosophical Society embraced in its membership many chemists, it had been suggested that a chemical section should be formed in the Society in accordance with the Society's rules. To this proposal the council of the Philosophical Society had readily agreed, and in order that the meetings and other privileges of the members might be open to other persons pursuing chemical studies, or engaged in chemical manufactures, they had resolved that associates should be admitted on payment of an annual subscription of five shillings, and that they should thereby be entitled to consult the Society's valuable reference library. Mr. Stanford also mentioned that he had received replies from about eighty members of the Philosophical Society, expressing approval of the projected chemical section. It was then resolved that such an organisation of the chemists of Glasgow should be formed, and a batch of eighteen associates were formally proposed and admitted, on the motion of Mr. James Mactear, F.C.S., of St. Rollox Chemical Works.

The night of meeting having been resolved upon, and Monday, the 6th of April, being fixed for the first formal meeting of the Society,

Mr. John Mayer, F.C.S., Government Science Teacher, proposed, and Mr. St. John Vincent Day, C.E., seconded, a motion—"That a council of eighteen members be elected to conduct the business of the Society, and that the council consist of a president, two vice-presidents, a secretary, and a treasurer, and thirteen ordinary members." The motion was unanimously agreed to, and in accordance with it the election was then proceeded with. The following are the names of the office-bearers:—

President: Dr. Thomas Anderson, F.R.S.E., Professor of Chemistry. *Vice-Presidents*, Dr. Wallace, F.R.S.E., Analytical Chemist; Mr. E. C. C. Stanford, F.C.S., Manager British Seaweed Company. *Treasurer:* Mr. Alexander Whitelaw, F.C.S., Soap Manufacturer. *Secretary:* Mr. R. Tatlock, F.C.S., Analytical Chemist. *Some of the other Members of Council:* Messrs. Ex. Bailie Harvey, Govanhaugh Dyeworks; James Napier, F.C.S., Chemical Manufacturer; William McAdam, Hyde Park Pottery; James Mactear, F.C.S., Alkali Department, St. Rollox Chemical Works; St. John Vincent Day, C.E.; John Mayer, F.C.S., Government Science Teacher: John Jex Long, Lucifer Match Manufacturer; John E. Poynter, Chemical Manufacturer.

We have every reason to expect that this new Chemical Society will be of much service in extending a knowledge of theoretical and practical chemistry. It would indeed be strange were it to prove otherwise, considering the chemical antecedents of the City of Glasgow, and the manufacturing district of which it is the centre. The class-rooms and laboratories of Glasgow are indissolubly associated with the names of Drs. Joseph Black, Thomas and Dundas Thomson, Thomas Clark, Birkbeck, Ure, Anderson, Penny, Professor Graham, Lyon Playfair, James Young, Greville Williams, and many others; and the chemical manufacturers of the Glasgow district furnish names not less famous in the history of chemistry. As evidence, there are the late Walter Crum; J. B. Neilson, of the hot-blast; Charles Tennant, the first manufacturer of bleaching-powder; Mushet, of the black-band ironstone; Macintosh, of waterproofing fame, &c.

We shall keep our readers apprised of anything that is novel, interesting, and important in the communications that may be made from time to time to the members of the Society, whose birth we have now put on record.

FOREIGN SCIENCE.

Paris, March 24, 1868.

Metallurgy of Iron.—Stannate of Sodium.—Oxidation of Butyric Acid.—Preservation of Meat.—Common Salt as a Manurial Agent.—Academy of Sciences.—Election of M. Bouley.—M. Le Verrier's Assistants.—Physical Properties and Calorific Power of Petroleum.—Analysis of Vegetable Tissues.—Corresponding term to Benzoic Acid in the Naphthalic Series.—Diastase.

M. Gillot's experiments on the metallurgy of iron have placed in evidence a certain number of points:—1. The theory of the reduction of silica, and the combination of the silicium with iron in the blast furnace; (2) a maximum limit which does not attain 1000° C., for the decomposition of the carbonate of lime; (3) the condition necessary for the progress of the furnace operations, viz., that each charge should furnish alone the total amount of heat required in its treatment; (4) the maximum and minimum limits: *a*, the temperature of complete combustion of the carbon at the tuyere; *b*, the mean temperature of the escaping products which result from this; *c*, the temperature of the body of the gaseous column, accruing to a charge after the conversion of the carbonic acid into carbonic oxide; finally, the temperatures and the modifications of the charge, and of the gaseous column at all stages. (5) The general cause of the transformation of bodies, of which cause cementation, oxidation, and reduction are only particular effects. (6) The principles which regulate the employment of one or of several tuyeres; (7) the theory of the employment of warm air in the blast furnace; the fact of a greater consumption of fuel with warm air than with cold; (8) the respective consumptions of heat by the pig-iron, and by the slag in blast furnaces, and in reverberatory furnaces; (9) the insufficiency of analyses of an aliquot part of the gaseous column, to determine either the composition of this gaseous column, or the reactions which take place in the furnace. Finally, M. Gillot compares the losses sustained in working the old process and the new, thus:—The losses in the carbonisation o the wood, and in the smelting of

the ores, taken together, entail in the old process a minimum loss of 90 per cent of the combustible employed, and a consumption of 779 kilos. for every 100 kilos. of iron or steel manufactured. In the new processes, there are only those losses of combustible, common to all systems—loss by radiation, and by the absorption of sensible heat by the products.

MM. Moberg and Rammelsberg have shown that when a solution of stannate of sodium is evaporated, crystals containing 3 molecules of water are obtained. Since then, M. Haeffely, in re-dissolving these crystals in their mother liquor, has obtained crystals containing 8 molecules; others containing 10 molecules of water have more lately been obtained by M. Scheurer-Kestner, by submitting a weak solution of pure stannate of sodium to a low temperature. The presence of foreign salts, and especially of an excess of hydrate of sodium, prevents the crystallisation.

M. Berthelot has made some experiments upon the oxidation of butyric acid. He performed the oxidation in an alkaline solution, so as to prevent, as far as possible, the destruction of the bibasic acids. For this purpose he dissolved 10 parts of butyric acid in 1200 parts of water, in the presence of 60 parts of potassa, This liquor gradually decolourised permanganate either in the cold, or better at 100°. An experiment was made at 100°, the temperature being maintained during several days, until the butyric acid had destroyed a little more than its own weight of permanganate. The liquor then contained a considerable quantity of carbonate, oxalate, and a small quantity of succinate, not to mention acetate and propionate. To isolate the various acids, this process was adopted. First of all, the liquor was rendered acid with hydrochloric acid; it was then boiled a moment, a drop of ammonia added, and precipitated by chloride of calcium. The precipitate contained oxalate of lime, and a small quantity of an analogous salt more carbonated, probably malonate. The filtrate was evaporated on the water-bath, and the chloride of potassium eliminated by repeated crystallisations. The last residue was rendered strongly acid by hydrochloric acid, and agitated at several intervals with a considerable volume of purified ether. Evaporated to dryness, the ether left a crystalliine acid, possessing the properties of, and reacting like, succinic acid. This fact has been verified by the solubility of the lime-salt in water, and by the precipitate occasioned upon the addition of alcohol; the properties of the magnesian salt, the precipitation of the neutral perchloride of iron by the solution of succinate of magnesia, &c., have also lent confirmation. The amount of succinic acid formed is very inconsiderable.

M. Martin has made some experiments upon the preservation of meat by means of ether. He placed in six tin boxes uncooked beef, surrounded by little tufts of cotton wool soaked in sulphuric ether; the boxes were then soldered tight, and exposed to the rays of the sun. Every three months a box was opened. Each piece of meat weighed a kilogramme. At the end of three months there was no alteration either in weight or form. The meat thus preserved does not undergo the putrid fermentation; it is strongly impregnated with ether, and the odour remains after numerous washings with cold water. When cooked the meat possesses a peculiar savour, probably due, M. Martin says, to the formation of a new ether: the fibre is disintegrated. The process is not applicable to the preservation of food, but other animal matters might perhaps be advantageously treated by it.

M. Jean has proposed the following as the mode of action of common salt employed as a manurial agent (considering all the salts likely to be present in the soil). The carbonate of lime decomposes the ammoniacal salts, transforming them into carbonate of ammonia; this carbonate meets in the subterraneous atmosphere with carbonic acid gas produced by the decomposition of organic matters, and forms bicarbonate; then if this salt finds chloride of sodium in the soil, a double decomposition is established, giving rise to chloride of ammonium and carbonate of soda. The chloride of ammonium, in its turn, is decomposed by carbonate of lime, yielding chloride of calcium, which passes into the sub-soil, and carbonate of ammonia. The carbonate of soda thus produced determines the decomposition of nitrogenous organic matter, and facilitates nitrification. The transformation of chloride of sodium into carbonate of soda takes place so readily that MM. Truck and Schlœsing have applied this reaction in the manufacture of carbonate of soda.

At the meeting of the Academy of Sciences held on the 2nd of March, the election of a member in the place vacant in the section of rural economy, was proceeded with. M. Bouley was the successful candidate. The meeting was essentially stormy. M. Deville had at the previous meeting defended in very energetic terms M. Foucault against some imputations M. Le Verrier had cast upon his memory. M. Le Verrier denied at this meeting that he had any other purpose than to do the amplest justice to, and exalt the fame of, the illustrious deceased. Afterwards another question became involved. The president, M. Delaunay, considered it right that the name of the assistant who discovered the 96th little planet should be made known; he said: "I have the honour to inform the Academy that the young man to whom the discovery of the 96th little planet is due, is M. Coggia." M. Le Verrier replied that only one result could follow from M. Delaunay's remarks—disorder in the observatory of Marseilles. He said that the young gentlemen who discovered planets at Marseilles did not deserve to be cited; the work they did was simply manual, and required no knowledge of astronomy. They were very well satisfied, he also said, with their condition; they received for every planet discovered an increase in salary of 250 fr., and a gold medal. A general protest from the members followed this statement.

M. Deville brought before the Academy at the *seance* of the 9th March, a memoir on the physical properties and the calorific power of petroleum and mineral oils. A large number of samples were experimented with. The mineral oil was submitted to distillation in a copper alembic furnished with a long serpent tube, and also with a thermometer. By means of this apparatus the amount of distillate passing over at various temperatures was estimated. The danger possible by explosion was measured by the proportion distilling below 140°. The same experimental fact represents as well the loss which must be sustained to remove the explosive property of the oil. Another danger is encountered when the oils are enclosed in air-tight vessels—explosion by dilation. The amount of space necessary to be left above a mineral oil is calculated from the coefficient of dilation. The data M. Deville has obtained from each sample are drawn generally from the following determinations. Loss by heating to 100°, to 120°, and so on, by intervals of 20° up to 200°; this is expressed in percentages. Composition of the oil, *i.e.*, percentages of carbon, hydrogen, and oxygen, obtained by combustion. Density at zero, and at 50°, and coefficient of dilation. Composition and density of the oil obtained by distillation, and density of the residue. In some cases the specific heat has been determined, and the latent heat at the mean temperature. M. Deville's memoir contained tables giving an immense number of experimental results; it is, however, only a first memoir; more upon the subject will be brought before the Academy shortly. Perhaps it will not be without interest to tell your readers that M. Deville has undertaken this research by command of the Emperor, to report upon the most advantageous arrangements to adopt for the economic and safe employment of mineral oils, with especial reference to its use in transports. M. Elie de Beaumont drew M. Deville's attention to a blackish schist which occurs at Vassy, near Avallon, as a source of oily matter; this schistous substance extends over a very large area.

MM. E. Fremy and Terreil contributed, at the same meeting, a memoir upon a general method for the immediate analysis of vegetable tissues. In wood they recognise the existence of three principles. The first component cannot be easily mistaken; it is insoluble in sulphuric acid, containing two equivalents of water; it is further transformed by chlorine water into a yellow substance, and afterwards dissolved, nitric acts in the same way as chlorine; it is not soluble in potash solution, either dilute or concentrated. The substance is distinguished by the name of the ligneous cuticle; it has also been designated by M. Hartig by the name of *eustathe*, in consequence of its great stability. The second component of ligneous tissue has been studied by M. Payen, under the name of *incrusting* substance: qualitatively, they recognise its existence by its solubility in sulphuric acid, which becomes blackened, and afterwards by its insolubility in alkaline solutions and in chlorine water. The third component is cellulose. When pure, cellulose dissolves without giving rise to colouration in concentrated sulphuric acid, and water does not precipitate it from the solution; it is difficultly attackable by chlorine water, and by nitric acid. They detail in their memoir the actual process. 1 grm. of the sawdust dried at 130° is introduced into a flask, with about a litre of chlorine water, and allowed to remain here for thirty-six hours. The chlorine water dissolves the ligneous cuticle and certain parts of the encrusting matter; it leaves in an insoluble state, cellulose mixed with a part of the encrusting matter, which has been transformed into an acid completely soluble in potash. This residue left by the chlorine water is, therefore, treated with an alkaline solution, afterwards washed with acid, then with water, and finally dried at 130°. Cellulose is thus obtained in a state of absolute purity. Determinations gave 40 cent of this substance for oak wood, and 39 per cent for the wood of the ash. For the determination of the ligneous cuticle, they take again 1 grm. of sawdust, and submit it to the action of sulphuric acid, containing 4 equivalents of water, for thirty-six hours; the portion which it is required to estimate alone remains insoluble; in some cases they replace the acid by other containing only 2 equivalents of water. The residue is washed with water and with an alkaline solution until the washings are no longer coloured; it is then dried. They take the precaution of testing the purity of the ligneous cuticle weighed; when pure it dissolves without residue in chlorine water or in nitric acid. The incrusting matter is determined by difference. But this part of the wood is composed of several substances, and they separate them thus:—(*a*) matter soluble in boiling water; (*b*) bodies, probably of the nature of pectose, which dissolve in dilute solutions of alkali; (*c*) matter rendered soluble in alkaline solutions, by treatment with moist chlorine. For instance, in the ligneous tissue of oak wood, they estimated the encrusting matter by difference, at 40 per cent; then they found *a* 10 per cent, *b* 15 per cent, and *c* 15 per cent.

Dr. A. W. Hofmann contributed a memoir on the corresponding term to benzoic acid in the naphthalic series, M. Payen, a memoir entitled "extraction and properties of diastase"; there were also several other very interesting papers, accounts of which will probably be introduced in your correspondent's next letter.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Toluolsulphurous Acid.—R. Otto (second communication). The action of phosphoric perchloride upon tuluolsulphurous acid gives rise to the formation of sulphotoluolic chloride, fusing at 68—69° C., and an oily body not obtained in sufficient quantity for examination. Nascent hydrogen forms metabenzylic sulphydrate:

$$\left.\begin{matrix}C_7H_7SO\\H\end{matrix}\right\}O + 4H = 2H_2O + \left.\begin{matrix}C_7H_7\\H\end{matrix}\right\}S$$

identical with that of Märker (*Ann. Chem. Pharm.*, cxxxvi., 75). When heated with water to 120—130° toluolsulphuric acid and oxybenzoldisulphide, $C_{14}H_{14}S_2O_2$ is obtained. Potassic hydrate at 250—300° causes it to split into sulphite and toluol. Fuming nitric acid forms the compound $C_{21}H_{22}N_2S_3O_6$, which may be looked upon as diazotrisulphotoluolic hydride, and besides this, nitrosulphotoluolic acid.—(*Zeitschr. Chem.*, N.F. iii., 600).

Oxidation of Benzol.—L. Carius. Benzol is readily oxidised by a mixture of manganic dioxide and sulphuric acid (5 acid, 1 water). Amongst the products of the oxidation were found benzoic acid, and a new diabasic acid of the composition $C_6H_4O_3$—oxybenzenic acid, *i.e.*, benzenic acid + 1 atom of oxygen. Oxybenzenic acid is sparingly soluble in water, fuses at 175° C., and distils undecomposed at 250°. The author considers it extremely probable that this acid is identical with phtalic acid, their composition, as found by analysis, differing but little, and fusion-point (phtalic acid, according to C., fuses at 175°, not at 120°) and solubility being the same with both. The formation of benzoic acid in this case is of interest as being the first in which, by simple oxidation of a hydrocarbon, an acid richer in carbon is obtained; this is due evidently to the presence of formic acid, which is formed during a certain stage of the reaction:

$$CO,H,OH + C_6H_5H = CO, C_6H_5, OH + OH_2.$$

—(*Ibid*, N.F. iii., 629.)

Derivatives of Mesitylene, containing Sulphur.—A. Holtmeyer. Mesitylensulphuric chloride, $C_9H_{11}SO_2.Cl$, is formed by the action of phosphoric perchloride on sodic mesitylensulphate. It dissolves in ether and alcohol, is insoluble in water, fuses at 57° C. Sodium amalgam converts it into mesitylen sulphurous acid, $C_9H_{11}SO_2.H$, soluble in alcohol and hot water, sparingly so in cold water, fuses at 98—99°. Mesitylensulphydrate $C_9H_{11}SH$ is obtained by adding the chloride to a heated mixture of zinc and sulphuric acid; it is a liquid boiling at 228—229°, soluble in alcohol, ether, or benzol, insoluble in water. The sulphydrate may be converted into mesitylendisulphide by adding to its alcoholic solution aqueous sodic hydrate. The disulphide fuses at 125°, its solubility in alcohol, &c., is the same as that of the sulphhydrate.—(*Ibid*, N.F. iii., 686.)

Reactions of Anisic Aldehyde.—Saytzeff and Samosadsky. If anisic aldehyde in alcoholic solution is digested with sodium amalgam two crystalline bodies are obtained. The one fuses at 172° C. is little soluble in cold, readily in hot alcohol or ether. Its composition is $C_{16}H_{18}O_4$, being intermediate between anisic aldehyde and alcohol; it seems to be the doubled aldehyde combined with 2H—analogous to the compound obtained by Hermann from benzoic acid with sodium amalgam. The second compound fuses at 125°, but otherwise resembles the first closely, and is supposed to be the doubled aldehyde—analogous to benzoin.—*Ibid*, N.F., iii., 678.)

Harmless "Pharaoh's Serpents."—A new method of making the curious chemical toys called "Pharaoh's Serpents" has been suggested by Vorbringer. The black liquor which results as a useless product when coal oil is purified with sulphuric acid, is to be treated with fuming nitric acid. The dark-coloured resinous matter which swims on the surface is then collected, washed and dried, when it forms a yellowish-brown mass having about the consistency of sulphur which has been melted and poured into water. When this mass is ignited it undergoes such a wonderful increase in bulk that a cylinder one inch long will give a snake about four feet in length. The briefness of the popularity enjoyed by the "original" serpents was due to the unhealthy vapours given off in the process of burning.—*Scientific American.*

CORRESPONDENCE.

CONTRACTION ON SOLIDIFICATION.

To the Editor of the Chemical News.

SIR,—During the discussion on "devitrification," at the close of Mr. Chance's interesting lecture at the Chemical Society, Mr. Forbes mentioned that Mr. Chance, at his suggestion, compared the measurements of the wooden frames used for making the moulds for casting certain blocks of basalt, with the blocks so prepared when cold, and it was ascertained that the frames and blocks corresponded in size, from which it was inferred that molten basalt suffers neither contraction nor expansion on solidification.

A short time since, I made a few observations on the cooling, after fusion, of some six or seven crystalline substances, and I noticed that, with the exception of bismuth, they *contracted* on solidification. The contraction, however, did not manifest itself, generally, by making the solidified masses smaller than the interior of the vessels in which the substances cooled; but by the formation of cavities, sometimes of considerable size.

The experiments referred to, were made with comparatively very small quantities of the various substances, the fusing points of all being far lower than that of basalt; still, as there was a considerable difference in their fusing points as well as in the amounts employed, I think it extremely probable were the blocks in question carefully examined, cavities would also be found in them, or on their surfaces, which in conjunction with the facts already elicited, would, I believe, settle the interesting question whether or not contraction takes place during the solidification and devitrification of basalt.—I am, &c.,

ALFRED TRIBE.

149, Gt. Portland Street. March 23, 1868.

ESTIMATION OF PHOSPHATES.

To the Editor of the Chemical News.

SIR,—As this is the season when so many persons are interested in the manufacture and composition of artificial manures, perhaps the following popular explanation of Mr. Burnard's process will serve as an answer to several of your correspondents and readers, and will be of use to any intelligent chemist and druggist, as an extremely simple mode of estimating their phosphatic value.

I take for granted that phosphate of lime consists of 3 equivalents of lime (3CaO) and 1 equivalent of phosphoric acid (PO_5), and is converted into biphosphate by the addition of 2 equivalents of sulphuric acid ($2SO_3$). The following diagram will exhibit this interesting reaction:—

Neutral phosphate of lime.	1 eq. phosph. acid	$PO_5=72$	Biphosphate.	
	3 eq. lime	$CaO=28$		
		$2CaO=56$	Sulphate of lime.	
	2 eq. sulph. acid	$2SO_3=80$		

Here we see 2 eq. of sulphuric acid combine with 2 eq. of lime to form sulphate of lime, while the remaining equivalent of lime combines with the phosphoric acid to form biphosphate of lime.

Mr. Burnard, in his ingenious process, recommends 100 gr. of the manure to be tested to be added to 2 pints of water; and this large quantity of water is employed that sufficient of the sulphate of lime may be taken up by the water, to afford lime by its decomposition with soda for combination with the biphosphate, which is also in solution. The following is the exact method I have adopted in working out Mr. Burnard's process:—I have a measure holding 1000 gr. of water; it is, in fact, a piece of glass tube, about half an inch in diameter, stopped at one end, and divided into 100 equal parts; consequently each division represents 10 gr. of water. I then dissolve 10 gr. of neutral carbonate of soda in rain-water, and make it up to 1000 gr. This constitutes my test solution of soda; every ten divisions of the measure represents exactly 1 gr. of soda. I then take another glass tube, about 1 ft. long and $\frac{1}{8}$th of an inch diameter, stopped at one end with a plug of gutta percha, which is perforated with a pin; the object of this tube is to add the test solution of soda, drop by drop, at the termination of the operation, when great nicety is requisite; this may be effected by applying the finger at the other end of the tube.

I now proceed to test, agreeably to the recommendation of Mr. Burnard, one half of the solution, or one pint, with my small tube, graduated into four divisions, so that each division represents $\frac{1}{10}$th of a grain of soda. It may require soda solution equal to about ten divisions of the small drop tube (or 1 gr. of soda) to neutralise the free sulphuric acid that may be present in the solution operated upon; but this is readily ascertained by adding the test solution gradually, and so long as the liquor remains quite clear when well agitated by stirring; directly the liquor assumes a slight milky appearance, the free sulphuric acid is neutralised, and the conversion of the biphosphate in solution into neutral phosphate is beginning, the milkiness being occasioned by the precipitation of phosphate of lime through the decomposition by the soda of the sulphate of lime in solution.

At this time a piece of blue litmus paper, fastened to a piece of cork, must be put into the liquor, when it instantly becomes red. Having noted the exact quantity of soda solution employed to neutralise the free acid, more of it may now be added gradually, until the liquor begins to show an alkaline reaction, constantly stirring the whole time; this will be readily seen by the litmus paper assuming a bluish colour. We will now suppose that 1 gr. of soda has been employed in neutralising any free acid that may be present, and 12 gr. have been employed in decomposing the sulphate of lime for the precipitation of all the biphosphate as neutral phosphate; it now remains to estimate the quantity of biphosphate in the 100 gr. of manure.

Now, 1 equivalent of carbonate of soda (53) combines with 1 equivalent of anhydrous sulphuric acid (40) to form sulphate of soda. Consequently, we have this proportion—as 53 is to 40, so is the 12 gr. of soda to the quantity of anhydrous sulphuric acid employed in the conversion of the neutral phosphate of lime into biphosphate, which is 9·05; but as we have only tested half the liquor, the whole will show 18·1 gr. as the quantity of anhydrous sulphuric acid employed in the 100 gr. of manure.

Again, our diagram shows that 80 parts of anhydrous sulphuric acid are necessary to form 100 parts of biphosphate; consequently, we have this further proportion to show the percentage of biphosphate contained in the 100 gr.

As 80 : 100 :: 18·1 : to the percentage of biphosphate formed by the 18·1 gr. of anhydrous sulphuric acid, which shows the manure to contain about 22½ per cent of soluble or biphosphate of lime.

As this is not intended for your professional readers, I hope it may not be considered two tedious for the pages of your valuable publication.—I am, &c.

W. LITTLE.

Heckington Hall, Lincolnshire.

MISCELLANEOUS.

Absorption of Arsenic, Tungstic, and Arsenious Acids from Solution by Charcoal—In a former number of the CHEMICAL NEWS, I stated that charcoal absorbed nitric acid from sulphuric acid: I have since found that it also absorbs the substances here specified under the following circumstances. If a few drops of a solution of a salt of arsenic, or arsenious acid, is put into a few ounces of dilute sulphuric acid and the mixed solution agitated at intervals with recently ignited charcoal for an hour or two, the clear liquid obtained by filtration does not manifest any reaction of arsenic when tested by Marsh's process. Lignite has not the same effect as charcoal, though absorbent of weak acids and bases generally, as I have before shown. Tungstic acid also from acid solutions is removed by charcoal applied in like manner, and is given up to a solution of caustic alkali.—W. SKEY.

Use of the Lime-Light in Barracks.—On Monday evening a series of experiments with the lime-light were conducted in the Queen's Barracks, Perth, with the view of testing the practicability of its introduction instead of gas, the Horse Guards having resolved to discontinue the use of gas in the Perth barracks, owing to its recent rise to 6*s*. 8*d*. per 1,000 feet. The experiments were made in the open air, in one of the lobbies extending the whole length of one of the wings of the barracks, and in one of the ordinary barrack-rooms. An apparatus, about 20 feet high, was erected in the square, having at the top an appliance for showing off the light, and a reflector above. When the flame was applied and the light regulated, the entire square was lighted up almost as clear as noonday, it being quite easy to read the smallest print at a distance of 100 yards. In the lobby a light of small size was used, covered with a glass globe, and the flame burned so brightly that a pin might have been seen on the floor at the extreme end of the lobby, a distance of fully 30 yards. The company then adjourned to one of the ordinary barrack-rooms, where a very small apparatus was fitted up; and, on the light being applied, the room was lighted up much more brilliantly than it would have been by gas. In fact, if there was any fault, it was that the light was too brilliant for the size of the room. The experiments were witnessed by a scientific gentleman from London sent by the War Office, and by the colonel commanding the Royal Engineers in Scotland, both of whom expressed themselves highly satisfied. We understand that contracts have been entered into for using it in the camp at Aldershot, and Government intend shortly to introduce it into all the barracks in the country.—*Edinburgh Courant.—Journal of Gas-Lighting.*

NOTES AND QUERIES.

Safflower.—Would any of your correspondents be good enough to enlighten me how to extract the colouring matter from safflower for dyeing pink?—G. JOHNSON.

Distillation of Grease.—Is there any Treatise published on the distillation of greases and animal and vegetable oils?—DAVID SHAW & Co.

Oxychloride of Magnesia.—If Mr. J. E. Hamilton is still looking for a supply of this article, he may hear of a cheap source by applying to the Publisher.

Production of Carbonic Acid.—In reply to "G.," in the CHEMICAL NEWS of the 13th inst., I beg to inform him that he may obtain carbonic acid very readily from limestone, marble, or chalk, at a low red heat, by passing a current of steam over them at that temperature.—W. H. POTTER.

Preservation of Meat.—Several sheep, killed in England, and preserved by Professor Gamgee's process, have been exhibited in New York last week in an excellent state of preservation. Would you please to give, through your CHEMICAL NEWS, or otherwise, a description of Prof. Gamgee's process, and the mode in which it is carried into effect?—A. G. HUNTER, Fair Haven, Connecticut, U.S.

Laboratory Stove.—Preserving Insects.—Can any of your correspondents tell me—1st, What do they consider the best laboratory stove? My present fire-place, besides being useless for heating a crucible, raises such a daily dust as seriously to interfere with analysis (chiefly agricultural analyses). 2nd, I have some eastern insects preserved in spirits in small glass bottles, but the spirit has softened the sealing-wax, and evaporated, leaving the insects dry and very brittle. What am I to do?—C. D. H.

Explanation, to Mr. S. A. S.—In the CHEMICAL NEWS, of March 6, you very severely take me to task, and criticise, 1st, the short hint I gave in "Notes and Queries" (CHEMICAL NEWS, No. 429); and 2nd, the equally very brief note of mine "On the Approximative Determination of Sulphur in Pyrites" (CHEMICAL NEWS, No. 428). Permit me to observe that your presumption of my ignorance is, in both cases, not proved by facts. I consider "Notes and Queries" only to be intended for short and brief hints; moreover, one might offend querists by supposing them not to be sufficiently well up in analysis to enable them to think and judge for themselves. Certainly, I think it would be quite out of place to expect "Notes and Queries" to contain full directions in every respect. As regards analysis of superphosphates, I am as well aware as the most prominent analyst in such matters, that, without full and thoroughly complete analysis, what I wrote about the estimation of free sulphuric acid would be leading to gross errors. As regards your objection to the approximative valuation of sulphur in pyrites, permit me to say that I thought it unnecessary to add, that I always and invariably, after having for some time ignited the previously-weighed pyrites, allow it to cool, then moisten it with pure nitric acid, expose again to heat with due precautions, and repeat this process at least from three to four times, when nothing but peroxide of iron will remain. My only reason for not mentioning this is, that I considered any one might think of it.—Dr. A. ADRIANI.

MEETINGS FOR THE WEEK.

MONDAY.—Medical, 8.
—— Chemical, 8. Anniversary Meeting.
TUESDAY.—Royal Institution, 3. Dr. M. Foster, "On the Development of Animals."
WEDNESDAY.—Society of Arts, 8.
—— Pharmaceutical, 8.
THURSDAY.—Royal Institution, 3. Dr. M. Foster, "On the Development of Animals."
—— Royal, 8½.
—— Chemical, 8. 1. "Chemical Researches on some New and Rare Cornish Minerals," by Prof. Church. 2. "On the Constitution of Glyoxylic Acid," by Messrs. Perkin and Duppa. 3. "On Glyoxylic Amide," by Dr. Odling.
—— Royal Society Club, 6.
FRIDAY.—Royal Institution, 8. Prof. Frankland, "On the Proposed Water Supply for the Metropolis."
—— Geologists' Association, 8.
SATURDAY.—Royal Institution, 3. Professor Roscoe, "On the Non-Metallic Elements."

TO CORRESPONDENTS.

Enquirer.—Liebig's process for silvering glass is well known, and has been more than once described in these pages. Some improvements have been lately introduced, which we will give in a few weeks.

North.—We are not aware that such a work exists. Consult the articles in "Watts's Dictionary," and in the text books.

Gaskell, Deacon and Co.—We will communicate with our Paris correspondent, and endeavour to ascertain further particulars.

Drysalter.—See answer to "*North.*"

A. G. Hunter, U.S.—Permanganate of soda will do, but we think sulphate of magnesia should also be employed.

Communications have been received from Gaskell, Deacon and Co.; W. H. Walenn; Dr. Adriani (with enclosures); M. Scott; A. G. Hunter; F. C. Correy (with enclosure); Norman MacLeod (with enclosures); F. Sutton; G. Johnson; W. H. Potter; C. Hunter (with enclosure); Drs. Wischin and Wilm (with enclosure); W. M. Watts (with enclosure); T. R. Fraser, M.D.; W. Chapman (with enclosure); Mr. Hargreaves (with enclosure); Dr. Sheridan Muspratt; A. Sarl; T. Reader; W. Smyth (with enclosure); J. Carter Bell (with enclosure); G. Foord, Victoria; J. Burton; J. Mayer (with enclosure); Dr. Letheby (with enclosure); F. H. Höbler; R. Lyle; David Shaw and Co.; W. Little (with enclosure); Baxter, Rose, Norton and Co.; José Piodela; A. Tribe (with enclosure); A. Coppins; J. Browning; T. A. Readwin (with enclosures); E. Stanford; T. Dorn (with enclosure); J. Van Voorst; Nicholson and Maule; J. C. Wilson; C. M. King; A. S. Herschel; Dr. W. B. Herapath, F.R.S. (with enclosure); Dr. Apjohn; C. M. King; W. B. Giles; Captain W. A. Ross, R. A.

Books received.—"A Sketch of a Philosophy, Part II.—Matter and Molecular Morphology:" London, Williams & Norgate; "Zeitschrift fur Chemie;" "Le Maraviglie Della Scienza;" "Recollections of the Paris Exhibition of 1867," by Eugene Rimmel. London: Chapman & Hall.

In Liquidation.—Re The London Chemical COMPANY (Limited). First portion of the Valuable Stock and Effects upon the Works, being the loose things in and about the premises. Messrs. STANLEY, ROBINSON, and PALMER are instructed by the Official Liquidator to SELL BY AUCTION, at the Works, Crockleford (3 miles from Colchester), on Thursday and Friday, April 2nd and 3rd, 1868, at Twelve for One o'clock precisely, the first portion of the Valuable Stock and Effects, consisting of Nitre Salt, Salt Cake, Glauber Salt, Molasses, Sulphur, Charcoal, Fire Clay, Ivory Black, 20 tons of Soda, 10,000 gallons of Vitriol and Tartaric Acid in liquors, 2,000 tons of Sulphate of Lime, quantity of useful Timber, Rod, Bar, and Old Iron, Cornish Boiler, 20 tons of Copper and Lead, Carboys, Mill Stones, Brown Paper, Iron-bound Tanks lined with Lead, Blacksmiths', Carpenters', Coopers', Plumbers', and Gas Fitters' Tools, Leather and India-rubber Driving Bands, Office Furniture, and numerous other Effects. May be viewed the day preceding and Mornings of Sale, and Catalogues had of Messrs. Hillyer and Fenwick, Solicitors, 12, Fenchurch Street; of Messrs. Frederick B. Smart and Snell, Accountants, 38, Gresham Street, E.C.; at the place of Sale; and of the Auctioneers, where Samples of the Stock may be seen, 11, Ironmonger Lane, Cheapside, E.C.

Important Sale by Public Auction.—On the 8th of May, a.c., will be offered for PUBLIC SALE, at BUCKAU, near Magdeburg, in the Province of Saxony, in PRUSSIA, the CHEMICAL WORKS, established for twenty-five years, and now at work. It is situated immediately contiguous to the Railway Station, on the border of a small navigable river, which, within a few hundred feet, joins the river Elbe. It is only a few miles distant from the Stassfurt Rocksalt Mines and Coal and Limestone Fields. Being situated in the midst of the Beet-root districts, its position is particularly advantageous.

The establishment produces chiefly Sulphuric Acid, Soda, and Chloride of Lime, for the manufacture of which there are :—

4 Lead Chambers, of an aggregate contents of 220,000 cubic feet.
1 Platinum Apparatus, capable of producing 60 cwt. of Concentrated Acid per day.
2 Sulphate Furnaces.
4 Soda Decomposing Furnaces.
8 Chloride of Lime Chambers.

Besides those there are—

2 Muriatic Acid Condensing Towers.
4 Apparatus for Refining Sulphur, and the complete Plant for the Production of Saltpetre, Aquafortis, Copperas, and Manures.

By means of the above-mentioned 4 Lead Chambers, about 4,000 tons of Acid can be manufactured per year.

The Soda Plant produced last year about 1,500 tons of Calcined and Caustic, and 750 tons of Crystals of Soda.

The Muriatic Acid finds a profitable sale among the numerous sugar refineries surrounding this establishment.

Labour is cheap, amounting to about 15 to 17½ silbergroschen for 12 hours' work.

6 Steam Boilers and 2 Steam Engines are employed.

Any further information can be had of Mr. Wilke, Solicitor, at Magdeburg.

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For Sale, a Complete Set of the Chemical Gazette from its commencement in 1842 till its incorporation with the Chemical News in 1859. Price £2, in Sheets. Complete sets being very rare, this is an excellent opportunity for subscribers to the Chemical News to secure the series in good condition. Apply to "Chemicus." Chemical News Office, Boy Court, Ludgate Hill, E.C.

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THE CHEMICAL NEWS.

Vol. XVII. No. 435.

ON THE SPECTRUM OF THE BESSEMER FLAME.

In our number for March 21st, we quoted an article from *Engineering*, giving the results of Professor Lielegg's observations on the spectrum of the Bessemer-flame. As these results were published in *Engineering* as entirely new, and no mention was made of any prior observations, it is only right that attention should be called to the fact that as long ago as 1862 the same results had been obtained by Professor Roscoe, and were published in the form of a short preliminary notice in the *Proceedings* of the Manchester Literary and Philosophical Society, for February 24th, 1863. As the note is extremely short, we transcribe it in full.

"Professor Roscoe stated that he had been for some little time, and is still, engaged in an interesting examination of the spectrum produced by the flame evolved in the manufacture of cast steel by the Bessemer process, on the works of Messrs. John Brown and Co., of Sheffield. The spectrum of this highly luminous and peculiar flame exhibits during a certain phase of its existence a complicated but most characteristic series of bright lines and dark absorption-bands. Amongst the former the sodium, lithium, and potassium lines are most conspicuous; but these are accompanied by a number of other, and as yet undetermined, bright lines; whilst among the absorption-bands, those formed by sodium vapour and carbonic oxide can be readily distinguished. Professor Roscoe expressed his belief that this first practical application of the spectrum analysis will prove of the highest importance in the manufacture of cast steel by the Bessemer process, and he hoped on a future occasion to be in a position to bring the subject before the Society in a more extended form than he was at present able to do."

In a lecture delivered before the Royal Institution (May 6, 1864) a year later than the communication quoted above, Dr. Roscoe described the Bessemer-spectrum more fully, and pointed out the existence of lines produced by carbon, iron, sodium, lithium, potassium, hydrogen, and nitrogen.

An important practical result of the observations on which these communications were based, was the discovery that the exact point of decarbonisation could be determined by means of the spectroscope with much greater exactitude than from the appearance of the flame itself, the change in which indicating the completion of the process is minute, and requires a lengthened experience to detect with certainty. This method of determining the point at which it is necessary to stop the blast was indeed at that time (1863) in constant use at Messrs. Brown's works, at Sheffield, and has since been introduced with equal success by Mr. Ramsbottom (at the suggestion of Dr. Roscoe) at the London and North Western Railway Company's steel works, at Crewe.

Dr. W. M. Watts now draws our attention to a paper "On the Spectrum of the Bessemer-flame," which he published in the *Philosophical Magazine* for December last. In it he states that he was at that time acting as assistant to Professor Roscoe, and in that capacity conducted a lengthened examination of the Bessemer spectrum at the works at Crewe. The results of that investigation were not published at the time, on account of their incompleteness; and he has since then continued in Glasgow the same research, which has now extended itself into an inquiry into the nature of the various spectra produced by the carbon compounds. These experiments are still incomplete; but under the circumstances of the publication of Professor Lielegg's papers, he has put together a few of the more important results obtained in the examination of the Bessemer-spectrum.

The changes which take place in the spectrum from the commencement of the "blow" to its termination are extremely interesting. When the blast is first turned on, nothing is seen but a continuous spectrum. In three or four minutes the sodium-line appears flashing through the spectrum, and then becoming continuously visible; and gradually an immense number of lines become visible; some as fine bright lines, others as intensely dark bands; and these increase in intensity until the conclusion of the operation. The cessation of the removal of carbon from the iron is strikingly evidenced by the disappearance of nearly all the dark lines and most of the bright ones.

The spectrum is remarkable from the total absence of lines in the more refrangible portion; it extends scarcely beyond the solar line *b*.

The occurrence of *absorption*-lines in the Bessemer-spectrum is in itself extremely probable; and that this is the case appears almost proved by the great intensity of some of the dark lines of the spectrum. It was with this view that the investigation was commenced, with the expectation that the spectrum would prove to be a compound one, in which the lines of iron, carbon, or carbonic oxide, &c., would be found, some as bright lines, others reversed as dark absorption-bands. To a certain extent this anticipation has been verified; but the great mass of the lines, including the brightest in the whole spectrum, have not as yet been identified.

In dealing with a complicated spectrum like that of the Bessemer-flame, it is indispensable that the spectrum should be actually compared with each separate spectrum of the elements sought. This was the plan actually pursued; the spectroscope was so arranged that the spectrum of the Bessemer-flame was seen in the upper half of the field of view, and the spectrum with which it was to be compared was seen immediately below. In no other way can any satisfactory conclusion be obtained as to the coincidence or non-coincidence of the lines with those of known spectra.

The spectrum of the Bessemer-flame was thus compared with the following spectra:—

(1) Spectrum of electric discharge in a carbonic-oxide vacuum.
(2) Spectrum of strong spark between silver poles in air.
(3) " " iron "
(4) " " iron poles in hydrogen.
(5) Solar spectrum.
(6) Carbon spectrum—oxyhydrogen blowpipe supplied with olefiant gas and oxygen.

The coincidences observed were, however, but very few and totally failed to explain the nature of the Bessemer-spectrum. The lines of the well-known carbon-spectrum do not occur at all, either as bright lines or as absorption-bands; nor was any coincidence observed between the lines of the Bessemer-spectrum and those of the carbonic-oxide vacuum tube.

The lines of lithium, sodium, and potassium are always seen, and are unmistakeable.

The three fine bright lines 73·7, 76·8, and 82 are due to *iron*. The red band of hydrogen is seen as a black band, more prominent in wet weather.

After the charge of iron has been blown, it is run into the ladle, and a certain quantity of the highly-carbonised *spiegeleisen* is run into it. The effect of the addition of the spiegeleisen is the production of a flame which is larger and stronger when the blow has been carried rather far. This flame occasionally gives the same spectrum as the ordinary Bessemer-flame; but more commonly a quite different spectrum is seen, which reminds one at first of the ordinary carbon-spectrum, but differs from it very remarkably.

In the carbon-spectrum, each group of lines has its strongest member on the left (*i.e.* less refrangible), and fades gradually away towards the right hand: in the spectrum of the spiegel-flame the reverse is the case; each group has its brightest line most refrangible, and fades away into darkness on the least-refracted side. A comparison of the drawing of the spectrum of the spiegel-flame with that of the Bessemer-flame will show that they really contain the same lines; but the general appearance of the spectrum is completely changed by alteration of the relative brightness of the lines. This was shown by direct comparison of the actual spectra.

Dr. Watts concludes his paper in the *Philosophical Magazine* by saying—"There can be no doubt that the principal lines of the Bessemer-spectrum are due to carbon in some form or other. My own belief is that they are due to incandescent carbon-vapour. The experiments in which I am at present engaged have already shown the existence of *two* totally different spectra, each capable of considerable modification (consisting in the addition of new lines) corresponding to alterations in the temperature or mode of producing the spectrum, and each due to incandescent carbon. It is possible that the Bessemer-spectrum may prove to be a third spectrum of carbon, produced under different circumstances from those under which the ordinary carbon-spectrum is obtained; and the intensity of the dark bands is more probably due to contrast with the extreme brilliancy of the bright lines, than to their actual formation by absorption."

COAGULATION AND PRECIPITATION OF CLAY BY NEUTRAL SALTS GENERALLY.

By WILLIAM SKEY,
Analyst to the Geological Survey of New Zealand.

A great many substances are recommended as substitutes for filtration in the clarifying of water turbid from the presence of clayey matter, but so far as I can learn, they all depend for their individual effects upon some chemical interchange between themselves and a portion of the clayey matter in suspension, or upon the formation of a new compound out of the elements of the agent employed; in either case the clay, or the residue of the clay, being carried down mechanically, entangled in the newly formed substance. The object of this communication therefore is to bring under notice that several neutral salts, having their component parts so strongly combined among themselves as to render their decomposition by clay apparently impossible, are individually capable of producing the same effect upon clay in suspension; thus a strong solution of chloride of sodium, chloride of ammonium, chloride of calcium, chloride of magnesium, or chloride of barium, or sulphate of soda, applied to a small quantity of clayey water, causes an immediate aggregation of the particles, and their complete precipitation shortly afterwards.

When the solutions are applied to a rather large proportion of clay water, the precipitation is not complete for several hours. The volume of clayey water clarified by one grain of certain of the above-named salts in 24 hours is approximately as follows—1 grain of common salt clarifies 5 ounces; 1 grain of chloride of barium or calcium, clarifies 10 ounces; 1 grain of sulphate of soda clarifies 5 ounces of clayey water; in addition to these it was found, 1 grain of lime clarifies 15 ounces, and 1 grain of sulphuric acid clarifies 50 ounces of clay water in the same time. Magnesia also when intermittently agitated with clay water has the same effects. Upon washing these clay precipitates repeatedly with pure water, the clay re-acquires its tendency of permanent diffusion. The quantity of clayey matters present appears of secondary importance, complete precipitation having nearer relation to the degree of dilution allowed to the salt employed. From these considerations, taken along with the fact of the existence of powerful affinities between the component parts of most of the substances here specified, it seems certain that these results of coagulation and precipitation are not due to their decomposition; the alternative is therefore, that they must thus act solely from their affinities for water; and if this is so, then from inference it would appear that the well-known quality of clay to remain in permanent suspension in rain or spring water in spite of its relative superior gravity is entirely due to the effect of a true chemical affinity existing between them; possessing in common with soluble bodies an insatiable quantitative affinity for water. Clay differs therefrom in the low intensity of this affinity (if I may use the term), a circumstance of course predicable from its insolubility; clay thus deports itself like certain other substances, which though soluble in pure water, are precipitated therefrom by others possessed of a superior affinity for the solvent,—for instance, ferrocyanide of iron by salts generally, silica in ammonia by chloride of ammonium, nitrate of baryta by nitric acid.

In conclusion I would desire to suggest that the transparency of the sea, into which is continually pouring such enormous quantities of turbid water, may be entirely due to the presence of so much saline matter.

ON THE PRECIPITATION OF COPPER BY HYPOPHOSPHOROUS ACID.

By WOLCOTT GIBBS, M.D.,
Rumford Professor in Harvard University.

IN a memoir on the hypophosphites, A. Wurtz* has shown that when solutions of copper are heated to 70° C. with hypophosphorous acid, a hydruret of copper is precipitated, which on boiling is reduced to metallic copper with evolution of hydrogen. On repeating this experiment, I found that the precipitation of copper is complete, and as the alkaline hypophosphites are now to be had in commerce, it appeared probable that the process might be applied to quantitative estimation. Experiments to determine this point have been made by Mr. R. Chauvenet with the following results:—

The copper should be in solution as sulphate, the liquid containing a little free acid. The precipitation from the nitrate is always incomplete. When chlorhydric acid or chlorides are present the method fails entirely, the copper being reduced to subchloride and remaining in solution. The solution must not be too dilute; the precipitation is complete if the saturated solution of sulphate be diluted with not more than ten times its bulk of water, before the addition of the hypophosphites. In order to avoid the sudden evolution of hydrogen gas, and also to obtain the precipitate in a spongy coherent form, it is best not to allow the liquid to boil. The solution of hypophosphite having been added in the cold, and in excess, the temperature is to be gradually raised until, after standing for some minutes between 80° C. and 90° C., the hydruret of copper has entirely separated in coherent masses. It is easy to determine when the precipitation is complete, by taking out a drop of the clear liquid with a rod, and testing upon a porcelain plate with a drop of sulphydric acid solution. No filter need be used if the precipitation be effected in an assay flask; the copper is easily washed by decantation, and may then be transferred to a porcelain crucible by the well-known method of inversion, dried and gently ignited in a current of hydrogen. The following analyses will serve to illustrate the accuracy of this

* Ann. de Chimie et de Physique, 3rd series, vol. vi., p. 199.

method. In all of them hypophosphite of magnesium was employed as the precipitant.

	Gr. pure sulphate of copper.		Gr. of copper.		Per cent.	
1. ..	1·1650	gave	0·2965	=	25·45	(Chauvenet.)
2. ..	1·5590	,,	0·3970	=	25·45	,,
3. ..	1·4255	,,	0·3625	=	25·43	,,
4. ..	1·3050	,,	0·3327	=	25·42	(R.B.Carman)
5. ..	0·8208	,,	0·2087	=	25·42	(E. F. Gale)

In (4) and (5) a large excess of sulphate of nickel was present.

The formula $CuSO_4$ + 5aq. gives 25·42 per cent of copper. In the third analysis, sulphates of iron, manganese, nickel, and zinc, in very large excess, were added to the solution of copper.

I. In a very pure subsulphide of copper from Arizona, Mr. Chauvenet found, in four analyses, 74·24, 74·37, 74·36, and 74·41 per cent copper.

II. In an alloy of copper and nickel

	Gr.		Gr. of copper.	Per cent.	
6.	0·4245 ..	gave ..	0.3605 =	84·92 ..	(Chauvenet)
7.	0.3615 ..	,, ..	0·3070 =	84·92 ..	,,
8.	0·1380 ..	,, ..	0·1170 =	84·85 ..	,,
9.	0·1980 ..	,, ..	0·1680 =	84·84 ..	,,

III. In brass wire

	Gr.		Gr. of copper.	Per cent.	
10.	1·6300 ..	gave ..	1·0705 =	65·67 ..	(Chauvenet)
11.	1·8655 ..	,, ..	1·2240 =	65·61 ..	,,
12.	1·6770 ..	,, ..	1·1010 =	65·65 ..	,,

In the last seven analyses the alloy was dissolved in sulphuric acid, nitric acid being added from time to time to assist in solution. The solution was then evaporated until the last traces of nitric acid were expelled. The presence of iron in the form of sulphate does not in any way interfere with the complete precipitation of copper by hypophosphite of magnesium. When sesquichloride of iron is present, however, the copper is always reduced to subchloride, and is not precipitated as metal or hydruret. A solution of a hypophosphite reduces sesquichloride of iron to protochloride: the reduction is particularly rapid and complete when a salt of copper is also present and the liquid contains free chlorhydric acid. I have endeavoured to base upon this reduction a method for determining iron volumetrically, but all the experiments failed, in consequence of the difficulty of determining the exact point at which the reduction of the iron is complete. Sulphocyanide of potassium, proposed for this purpose by Winkler,† in his process, with subchloride of copper as a reducing agent, was not found to give sharp indications. When copper and iron are present together, as chlorides, the addition of hypophosphite of magnesium simply reduces the copper to subchloride, as above stated. If in this case we add an alkaline chloride to keep the subchloride of copper dissolved, the copper may be easily precipitated as subsulphide by sulphydric acid gas. When arsenic or antimony are present with copper, these must first be separated before precipitating the copper as hydruret, as careful experiments by Mr. C. Lilly have shown that both arsenic and antimony are precipitated with the copper. Mr. Lilly obtained the following analytical results when arsenious acid was present.

Gr. of sulphate of copper.		Gr. metallic copper.		Per cent copper.
1·2690	gave ..	0·3279	=	25·83
1·5127	,, ..	0·3905	=	25·77
0·9638	,, ..	0·2509	=	26·03

The formula gives 25·42 of metallic copper. In presence of antimonious acid—0·7100 gr. sulphate of copper gave 0·2454 gr. of copper = 34·56 per cent. After addition of Sb_2O_3 and Rochelle salt, 0·9875 gr. sulphate of copper gave 0·2426 gr. copper = 24·56 per cent. Repeated analyses by Mr. Lilly also showed that copper could not be determined accurately in Schweinfurt green by hypophosphite of magnesium, and that the presence of Rochelle salt did not completely prevent the precipitation of arsenic with the copper when arsenious or arsenic acid were mixed with sulphate of copper.

In assaying copper ores, it is usually desirable to bring the metal at once into the form of sulphate. Numerous experinents made in this laboratory fully justify me in recommending the following method:—The finely pulverised ore (sulphides of copper and iron) is to be mixed in a porcelain crucible with three or four times its weight of a mixture of 1 molecule of bisulphate and 1 of nitrate of potassium. The mixture is then to be slowly heated to low redness, which is best accomplished in a muffle. The metallic sulphides are completely oxidised without the least frothing of the heated mixture. Enough strong sulphuric acid to convert all the sulphate of potassium into bisulphate is then added, and the crucible is to be again carefully heated, until the contents run to a clear fused mass. On cooling the mass usually separates readily from the crucible, which is not attacked, and on solution the iron and copper are found completely converted into sulphates. This process has been tried successfully with a great variety of ores. The whole operation requires about an hour. In the case of ores containing much bisulphide of iron, it is best to heat the powdered ore first as long as sulphur is given off, and afterwards to add the oxidising mixture and heat as above. The sulphides of lead, zinc, and antimony are completely oxidised by the same process.—*Amer. Journ. of Science and Arts.*

† Zeitschrift für Analytische Chemie, Bd. iv., p. 423.

COAL GAS

AS A POSSIBLE SOURCE OF CONTAMINATING SUBSTANCES TO BE TESTED FOR AMMONIA.

In a paper on analysis of water, published at the latter end of last year in the *Scheikundige Bijdragen uit het Laboratorium van het Athenæum Illustre*, at Amsterdam, Dr. Gunning calls attention to the fact, that coal-gas however well purified is by no means free from ammonia; he felt induced to institute some experiments on this subject, the result of which is, that last summer the gas used in the Laboratory at Amsterdam contained 0·00075 gramme of ammonia, or ammoniacal substances in 1 litre of illuminating gas, amounts in bulk to a little over one cubic foot thereof in one thousand cubic feet of gas. Attention is called to the fact, that where wet gas meters are in use, the water of which is never replenished unless by some accident, this water must become pretty fairly saturated with ammonia; on emptying the water from one of the meters at the laboratory at Amsterdam its bulk was found to amount to 219 litres, *i.e.*, 48·66 gallons. 10 cubic centimeters of this fluid yielded 192 mlgrm. of ammonia, or bases of a similar nature. The whole quantity of water contained, therefore no less than 4·2 kilogs. of these bases; the meter had been in use for only two years. Since coal-gas, moreover, always contains sulphur compounds, there is formed sulphate of ammonia, which, on becoming converted by the intense heat into bisulphate of ammonia, attacks the glass cylinders, or chimneys, placed on the Argand gas burners. Dr. Gunning has found from expressly instituted experiments that no combustion of the ammonia takes place, not even in Bunsen burners, and mentions that a platinum basin filled with perfectly pure water, and placed for even less than an hour over a Bunsen burner, had got contaminated with a perceptible quantity of ammonia in the form of sulphate. There are two gas companies supplying Amsterdam, and there is a strong competition, but also a good surveillance to secure the gas to be as pure as possible. The experiment above alluded to was made with gas taken directly from the street main.

BLOW-PIPE COAL ASSAY.*

By BENJAMIN SMITH LYMAN.

Many young assayers are perhaps hardly aware how well adapted the blow-pipe apparatus is to the assaying of coal. Not only does the portableness of the apparatus make it very convenient for use away from home, wherever the scales can be set up; but its use at home is quite as satisfactory on the score of exactness as the assay with the muffle or retort, or large platinum crucible and large scales.

Besides the ordinary pieces of the blow-pipe apparatus, as made at Freiberg, all that needs to be made expressly for the coal assay is a small covered platinum crucible of the same size and shape as the clay crucibles of that apparatus; and there must be a little ring for the crucible to stand on, of German silver, about three-eighths of an inch across, and half that in height. Such a crucible cover and ring weigh about two grammes and a half more than the ordinary metallic cup that rests on the pan of the scales; the crucible and ring without the cover weigh less than two grammes more than the cup. If it be desired to determine the amount of hygroscopic moisture in the coal, a small drying bath must be made too; but W. R. Johnson's coal assays have shown that the hygroscopic water in ordinarily well dried coals (not brown coals) is of little importance.

The size of the crucible allows the cokeing of 200 to 600 or more milligrammes of coal, according to the dryness of the coal and the extent of its swelling up when heated; and as the blow-pipe scales (of Lingke's make) weigh within a tenth of a milligramme, it is easy to weigh within much less than a tenth of one per cent of the amount of coal assayed, much nearer, in fact, than the exactness of the coke assay in other respects. In this point, indeed, the blow-pipe assay is quite as good as the assay with the larger scales, especially the muffle assay, where the coal must be brushed into a clay receptacle after weighing, and the coke or ashes brushed off from it before weighing; while here the crucible is weighed each time without removal of its contents, and without danger, therefore, of losing anything or adding any dust. It may be objected that the smallness of the amount of coal that can be assayed with the blowpipe makes it a less trustworthy indicator of the general composition of the coal than a larger assay; but the size of the lumps or powder assayed may be made finer accordingly, so that when mixed up, an equally just sample of the whole mass would be got for the small assay as for the large.

Any one who has a little experience, both in the use of the blow-pipe and in the ordinary muffle assay of coal, would scarcely need any further teaching for the coal assay with the blow-pipe. For others, it is worth while to say that the coal may be assayed either in a fine powder or in little lumps, and either with a slowly increasing or with a quickly increasing heat. A quick heat will give less coke by several per cent, but will often make a dry coal cake together that would not cake with a slow heat. The cover of the crucible should be left open a little crack, for the easy escape of the gas, but covered enough to prevent any flying off of solid material. The heat should increase to redness, and as soon as the escaping gas stops burning the heat should be stopped. As some coals part with their gas more quickly than others, of course no definite time can be fixed for heating all coals; but the burning of the gas is a good enough sign. Care should be taken not to let the coke take up moisture from the air before weighing, as it will quickly do if it has a chance. Of course, owing to the different effect of quick or slow heat, a certain uniformity of result, even with perfectly uniform samples of coal, can only be got, without error, by practice and by mechanical skill, by reproducing with nicety the same conditions in successive assays.

* American Journal of Mining.

After the coke has been weighed, it can be heated again with very free access of air, say, with the crucible tilted to one side, with the cover off, until everything is thoroughly burnt to ashes; and these should be re-heated until no change for the less is made in the weight. With free burning soft (semi-bituminous) coals this burning to ashes is very slow, so that it is very fatiguing or even impossible to carry it out with the blow-pipe; but in that case the crucible may be heated over a Bunsen gas-burner or an alcohol lamp, and left to glow for hour after hour. For the matter of that the coking is far more conveniently done in the same way than by blowing with the mouth.

Here is a pair of blow-pipe assays, made five years ago, of some West Virginia asphaltum, that seemed itself to be much more uniform in composition than coal from different benches in one bed is apt to be:—

	Volatile Matter.	Coke.	Ashes.
No. 1	47·29 per cent	52·71 per cent	1·65 per cent
No. 2	46·93 ,,	53·07 ,,	1·81 ,,
Mean	47·11 ,,	52·89 ,,	1·73 ,,

SWEET PRINCIPLE OF FROZEN POTATOES

By Dr. A. OTT.

At the meeting of the Polytechnic Branch of the American Institute, January 16th, Dr. Adolph Ott, well known as the author of a useful and interesting work on soap and candles, detailed the results of his recent investigations and experiments with potatoes, his object being to determine the sweet principle of frozen potatoes.

With a view to determine this question, he exposed 1 lb. of this vegetable to a very low temperature, and when thoroughly frozen, he first reduced a ¼ of a lb. of it to a pulp, by rasping, and expressed the sap, to 100 centimetres of which (33·8 fluid ounces) he added 10 cubic centimetres of basic acetate of lead; and this, filtered, and transferred to the glass tube of Mitscherlich's polarimeter, and placed between Nichols' prisms, gave no rotation of the plane of polarisation, thus establishing the fact that no sugar is present in raw, frozen potatoes. To decide whether sugar is formed in the process of cooking, he steamed 250 grm. (nearly 9 oz.) of the vegetable for one hour, and mashed them with 200 cubic centimetres of tepid water. The solution thus obtained was divided into two portions, from one of which was precipitated the gummy and protein matters with sub-acetate of lead, also discolouring the reddish brown liquid with a few drops of sulphuric acid. This, placed as before in the tube of the polarimeter, gave a change of colour, indicating the presence of uncrystallisable sugar. No calculation, however, was made from this, as the rotation of the grape sugar to the left is partly compensated by the dextrine and other substances; which are right-handed in respect to polarised light, and which are generally the product of heat upon albuminous starchy matter. He therefore had recourse to Fehling's test, by using the infusion of the boiled potato which had been set aside, by which the following elements were determined:—

1st. The percentage of sugar in the infusion.

2nd. The amount of water in the boiled potato.

Thus the percentage of sugar in the latter was calculated, and found to be 1·45 per cent. What, then, is the cause of the sweet principle? He would answer by saying that in freezing and thawing, the sap of the potato bursts the cells, and thus destroys vitality; at the same time, decomposition is setting in, which, though retarded by the cold, is not entirely arrested; the more so as, at the season most likely to freeze, and especially during a snow-storm, there abounds that powerful oxidising agent, ozone.

No doubt the outer portions of the starch grains are first attacked by it, and may thus be transformed into *diastase*, a body which, as we know, possesses the same power as dilute acids of converting a comparatively large quantity of starch, first into dextrine, and then into sugar, at a temperature of 140° to 170°, as in the process of cooking. Wheat contains enough diastase, as does every seed when sprouting, to convert all its starch into dextrine and sugar; not so with the potato, which, however, contains enough to form so much sugar as is necessary to give it that peculiar sweet taste.

The outer layers of the starch grains contain the albuminous bodies.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Anniversary Meeting, Monday, March 30th.

DR. WARREN DE LA RUE, F.R.S., &c., President, in the Chair.

AT this meeting there was a good attendance of members, and the officers of the Society were nearly all present.

The business of the evening commenced with the reading of the President's report, which gave a very satisfactory account of the past year's proceedings, but the obituary notices were, unfortunately, more numerous than usual. The list of members, in comparison with that of last year, stands thus :—

	1867.	1868.
Number of Fellows ..	499	510
Foreign Members ..	40	39
Associates	0	2

The number of papers read during the session amounted to forty-eight; and four lectures were delivered.

Five members have voluntarily retired during the year, viz.:—Dr. F. V. Paxton, and Messrs. Anselim Odling, C. N. Ellis, Edward Rea, and W. V. Russell. Eight names were struck off the list of members, by reason of arrears of subscription.

The losses by death included several distinguished Fellows, and one of the founders of the Society. They were Professor Michael Faraday, Dr. C. G. B. Daubeny, Dr. Thomas Clark, Dr. Wm. Herapath, Mr. Robert Warington, F.R.S., Messrs. J. Tennant, Walter Crum, W. H. Gossage, Alfred Noble, and Wm. Winsor, besides an eminent foreign member, Professor Jules Pelouze.

The PRESIDENT indicated some of the leading researches published during the year in the several departments of the science, and referred to the progress made towards establishing the new Chemical Theory. The investigations of Graham, Hofmann, Kolbe, Abel, Fittig, Frankland and Duppa, Perkin, and Pettenkoffer and Voigt were specially mentioned. The discussions upon water analysis had elicited facts which would ultimately prove useful in establishing a new method; and the review of geological phenomena, from a chemist's wide sphere of observation, could not fail to be productive of great results.

The TREASURER presented the balance-sheet for the year, which had been audited by Mr. Stephen Darby. The amount received from subscriptions was £540, and some of the disbursements were the following :—

Printing the Journal	£223
Proceedings of the Royal Society ..	50
Books and Magazines	43

The assets at the present time are a balance at the bankers of £637 1*s.* 11*d.*, and £2347 18*s.* 10*d.* invested in Government consols. The outstanding subscriptions due to the Society are stated at £186 16*s.* 0*d.*

The election of officers was then proceeded with, Dr. Hugo Müller and Dr. T. Stevenson being appointed scrutators. The result was declared to be in accordance with the printed list, or that proposed by the retiring Council. The President was re-elected, and the names of the remaining officers are appended :—

Vice-Presidents, who have filled the office of President : Sir B. C. Brodie, F.R.S.; Thomas Graham, F.R.S.; A. W. Hofmann, LL.D., F.R.S.; W. A. Miller, M.D., F.R.S.; Lyon Playfair, Ph.D., C.B., F.R.S.; A. W. Williamson, Ph.D., F.R.S.; Colonel Philip Yorke, F.R.S. *Vice-Presidents :* E. Frankland, Ph.D., F.R.S.; J. H. Gilbert, Ph.D., F.R.S.; J. H. Gladstone, Ph.D., F.R.S.; John Stenhouse, LL.D., F.R.S. *Secretaries :* William Odling, M.B., F.R.S.; A. Vernon Harcourt, M.A. *Foreign Secretary :* F. A. Abel, F.R.S. *Treasurer :* Theophilus Redwood, Ph.D. *Other Members of Council :* E. Atkinson, Ph.D.; F. Crace Calvert, F.R.S.; J. Lothian Bell; Dugald Campbell; W. Crookes, F.R.S.; David Forbes, F.R.S.; G. C. Foster; A. Matthiessen, Ph.D., F.R.S.; E. J. Mills, D.Sc.; H. M. Noad, Ph.D., F.R.S.; W. H. Perkin, F.R.S.; J. Williams.

Mr. E. T. CHAPMAN moved a vote of thanks to the President for his services during the past year, which was seconded by Mr. TENNANT, who took occasion to advise the printing in a separate form, and issue of duplicate copies to each member, of the annual report and address just now delivered by Dr. De la Rue. Such a course had been adopted with advantage in other learned Societies, and tended both to diffuse information respecting the aims of their body, and to do honour to the memory of the great chemists departed.

The vote of thanks was carried by acclamation.

Mr. Tennant's suggestion was afterwards made a substantive proposition, and warmly supported by Mr. Brayley; it was then put to the meeting, and carried unanimously.

Dr. DE LA RUE returned thanks, and in allusion to his wandering for a time from the paths of chemistry into the green fields of astronomy, humorously illustrated the feeling of amazement which overcame him on returning to a chemical career, by comparing his experience to the dream of Rip Van Winkle.

A vote of thanks to the retiring members of Council and a special acknowledgement of Mr. Watts's services, were moved and carried with acclamation. The meeting was then adjourned until the 2nd of April, the papers to be then read having already been announced.

FOREIGN SCIENCE.

PARIS, MARCH 31, 1868.

Method of estimating the proximate constituents of meteoric iron.—On the corresponding term to benzoic acid in the naphthalic series.—Determination of the equivalent of aluminium.

REFERENCE has already been made to the researches of M. Meunier, on meteoric iron, in one of my former letters; the same investigator has, more recently, proposed a general method for the proximate analysis of meteoric irons. According to his experiments, iron of meteoric origin is composed of mixtures of iron and nickel, carbide of iron, sulphide of iron, phosphide of iron, and graphite. Certain species of minerals are never found intermingled, wherefore it is often unnecessary to consider the separation of some of

the usual constituents. Frequently the carbide of iron disappears, or does not exist in estimable amount; such, for example, is the case in the meteoric iron discovered in 1784 at Xiquipilco, in the Valley of Toluca, Mexico. M. Meunier examined the various components given above separately. Nickeliferous iron obtained from various specimens of meteoric iron, differed considerably in composition, but the properties were sufficiently analogous to allow of its being considered as a single substance in the processes of separation. The substance is soluble in most acids, yielding a nickel salt and an iron salt; sometimes, although rarely, the solution is accompanied by the deposit of a little carbon: cold fuming nitric acid is without solvent action on it. Solutions of potash and soda, even at ebullition, are without action. Fused caustic alkalies do not dissolve, to any sensible extent, the nickel-iron mixtures. Sometimes nickeliferous iron does not precipitate salts of copper, such as nitrate and sulphate, in the cold, but upon heating to ebullition, the precipitation is always effected. Chlorine attacks nickeliferous iron pretty rapidly, especially in the presence of water; bromine and iodine exert a similar but less powerful action. Carbide of iron partakes, for the most part, of the properties of nickeliferous iron. The sulphide of iron, termed *troilite*, dissolves in hydrochloric acid with disengagement of sulphuretted hydrogen; fuming nitric acid has no action upon it; concentrated solution of sulphate of copper is not decomposed, even upon boiling, by troilite; alkaline solutions are almost without action in the cold, but they exert action, though sluggishly, upon boiling; the fused alkalies dissolve it instantaneously. The name of phosphide of iron M. Meunier gives provisionally to the compound better known as *schreibersite*, and which exists in most metallic meteorites. The composition of the mineral has not yet been defined; at the same time, the existence of phosphorus, iron, and nickel, is recognised. Magnesium possibly enters into the composition of schreibersite also. The mineral is not acted upon by boiling hydrochloric acid; alkaline solutions only attack it when aided by heat; fused potash and soda dissolve it instantly; *graphite* resists the majority of reagents.

By means of the various reactions indicated above, M. Meunier was enabled to separate the iron of Toluca into its four proximate constituents. The analytical process is conducted as follows:—(1.) Estimation of the nickeliferous iron—1 gramme of iron-filings, obtained by means of a very hard file, is projected into some grammes of pure potash in tranquil fusion, contained in a silver crucible. Care must be taken to throw the metal quite into the middle of the fused mass, and not on to the sides of the crucible, where it would undergo oxidation. Schreibersite and troilite are decomposed, and nickeliferous iron and graphite alone remain in the solid state. After cooling the fused mass is placed in strong alcohol, and allowed to remain there for about forty-eight hours. At the end of this time the whole of the potash is dissolved, and the lixivium, at the bottom of which the mixture of graphite and iron is found, is filled with brown flocks of oxide of iron. Decantation easily removes the light oxide; the residue is well washed with alcohol, and then dried. This residue is then treated with slightly warm fuming nitric acid: all the graphite disappears. It is only necessary to wash the nickeliferous iron, and dry. M. Meunier obtained about 96 per cent of nickeliferous iron.

(2.) Estimation of the graphite.—3 grammes of iron-filings are projected, as before, into fused potash, and a mixture of graphite and nickeliferous iron obtained. This mixture is treated with hydrochloric acid, which dissolves the iron and leaves the graphite. The graphite might contain a little carbon resulting from the decomposition of carbide of iron, but carbon due to this source can always be estimated. Another method of separating the iron and graphite would be lixiviation. It is, however, indispensable that the physical separation be controlled, to some extent, by a chemical process. The Toluca iron gave 1·176 per cent graphite.

(3.) Estimation of the troilite.—To estimate this, 3 grammes of filings are boiled for about a quarter of an hour with a solution of oxide of copper. All the nickeliferous iron is dissolved, and by decanting and washing, a residue is obtained, composed of troilite, schreibersite, graphite, and metallic copper. This mixture is treated with fuming nitric acid; the copper and graphite are removed, and troilite and schreibersite are thus obtained in a state of purity. No reaction has been discovered permitting the isolation of the troilite; it is therefore necessary to have recourse to lixiviation, and to separate the schreibersite and troilite by their different specific gravities. The specific gravity of troilite is never more than 4·7, while that of schreibersite is 7·01 to 7·22. Toluca iron gave by the process described 1·482 per cent of troilite.

Estimation of the schreibersite.—The preceding operation evidently gives a first determation of the schreibersite, in the lixiviation. The number thus found can be controlled by a chemical process. Having obtained the mixture of schreibersite and troilite, treatment with hydrochloric acid will dissolve all the sulphide, leaving consequently the phosphide in a state of purity. Toluca iron gave 1·232 per cent of schreibersite. The numbers found for each of the proximate constituents added together will be found to give 100·191.

The following is an abstract of Dr. Hofmann's communication to the Academy on the corresponding term to benzoic acid in the naphthalic series. When a mixture of four parts of naphthaline of commerce, and five parts of crystallised oxalic acid is submitted to distillation in an iron pot, the cover of which is furnished with a tube (the kind of vessel used in the manufacture of cyanide of potassium), water and naphthaline come over at the commencement; soon also, an oily substance which is not slow to solidify makes its appearance. This oily substance is composed of naphthylformamide, naphthyloxamide, oxalate of naphthylamine, naphthylamine, and water. A current of steam removes from the oily product notable quantities of an opaque brown oil, having a greater density than that of water. The analysis of this body leads M. Hofmann to call it cyanide of naphthyl, and proves to him that the succession of transformations undergone by naphthylamine when acted upon by oxalic acid is quite analogous to that already shown in the case of aniline and toluidine. The purification of the crude mixture presents no difficulty. Solution of the oil in ether excludes the water; the etherial liquid is evaporated, and the residue distilled. It is only at 218° or 220° that the thermometer becomes stationary; the portion distilling at this temperature soon solidifies. This portion is shown by many properties to be naphthaline, mixed with a small quantity of a substance possessing a peculiar aromatic odour, and boiling at a higher temperature. The point of ebullition soon rises to 290° and 300°; the remainder of the liquid distils in the form of a clear yellow liquid, which becomes a white crystalline mass when left a considerable time in a cold place, or when immersed in a freezing mixture. Once solidified it does not become fluid again at the ordinary temperature. Crystallisation from alcohol will render the body pure. The alcoholic solution mixed with water deposits the oil again. The crystals melt at 33·5°; and the substance boils at 290°. This new crystalline compound corresponds in the naphthalic series to the benzonitrile of the benzoic series. The formula is $C_{11}H_7N$. For this body to be placed among the nitriles, it ought under the action of powerful metallic hydrates, to fix a molecule of water becoming an amide, which by absorbing another molecule of water should give rise to a salt of ammonia: experiments upon these points have confirmed its position. If the nitrile is dissolved in alcoholic soda, only traces of ammonia are disengaged, but upon adding water one recognises the formation of a new compound.

The crystals deposited are little soluble in alcohol, and only melted with difficulty. Purified by repeated crystallisations from boiling alcohol, the compound presents itself in the form of white needles. Analysis has given the formula $C_{11}H_9NO$. The product is thus derived from the nitrile by the assimilation of a molecule of water, and is the amide. In making this last substance, by the action of soda, ammonia is evolved. It is only necessary to add hydrochloric acid to the alkaline solution to precipitate a crystalline acid resembling in its properties, vividly, benzoic acid. This precipitate can be obtained quite as readily from the crude nitrile, by treating the latter with an alcoholic solution of soda until ammonia ceases to be evolved, evaporating the alcohol, and decomposing the alkaline liquid by hydrochloric acid. The acid is purified by crystallising from alcohol, or better from boiling-water. The pure acid crystallises in white needles, which melt at 160°. At a higher temperature the acid sublimes; its boiling point is 300°. The acid has scarcely any odour or taste; gently heated, it exhales an odour analogous to that of naphthaline: the vapours excite coughing. Solutions of the substance possess a slightly acid reaction; they decompose alkaline carbonates with facility. M. Hofmann proposes the names menapthoxylic acid and naphthaline-carboxylic—the amide and nitrile would then be menaphthoxyl-amide and menaphthenylnitrile. Several salts of the acid have been made; the composition of the silver salt is represented by the formula $C_{11}H_7AgO_2$. The copper salt is a green precipitate, the lead salt white. When the acid is distilled with caustic baryta, naphthaline and carbonic acid are obtained—$C_{11}H_8O_2 = C_{10}H_8 + CO_2$. If 4 parts of fused menaphthoxylic acid are ground with 5 parts of perchloride of phosphorus, the two bodies act upon each other at once. The mixture is liquefied at the ordinary temperature; heated moderately, hydrochloric acid and oxychloride of phosphorus are produced. The boiling point rapidly rises to 300°; the fraction distilling between 296° and 298° is pure menaphthoxylic chloride. Its composition is $C_{11}H_7OCl$; it comports itself like most chlorides of the aromatic acids. Exposed to the air, it absorbs moisture, and is gradually transformed into menaphthoxylic acid; addition of water causes the reaction to take place instantaneously. Treated with ammonia, the chloride furnishes menaphthoxylamide, possessing all the properties of the body obtained by the action of an alcoholic solution of potash on the nitrile. When the chloride is placed in contact with an alcoholic solution of aniline, a white crystalline mass results. This compound is $C_{17}H_{13}NO$. The solution of aniline, replaced by naphthylamine, yields a compound of the formula $C_{21}H_{15}NO$. In treating the chloride with absolute alcohol, a compound of the formula $C_{13}H_{12}O_2$ is obtained.

M. Isnard addressed a note on the determination of the equivalent of aluminium. The process employed consisted in attacking the metal by hydrochloric acid. He found that 9 grammes of aluminium attacked by pure hydrochloric acid gave invariably, after calcination, 17 grm. of alumina, whence he concludes that 9 should represent the equivalent of aluminium, hydrogen being unity.

Solubility of Amorphous Silica in Ammonia.—Silica which has been liberated in a hydrous form, when allowed contact with a cold aqueous solution of ammonia for a short time, is sensibly dissolved, as therein demonstrated by the gelatinous precipitate the solution affords to chloride of ammonium, insoluble in cold solution of potash. When the silica is reduced to the anhydrous state, even by strong ignition, it is still partly soluble in ammonia. If for the last experiment a glass vessel is used which has just previously been in contact with hydrochloric acid, it is necessary to allow it contact with ammonia or caustic alkali for some time, for the purpose of removing the silica thereby liberated on the surface of the glass.—W. Skey.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Neurin and Sincalin.—Claus and Keesé have made some experiments on sincalin with a view of elucidating the nature of its relation to neurin. After a careful comparison of the various derivatives of the two, particularly of the chloro-platinate and aurate, the authors come to the conclusion that they are identical.—(*Journ. pr. Chem.* cii. 24.)

Naphthalene.—H. Vohl. Perfectly pure naphthalene has the specific gravity at 19° C. = 1·15173; it fuses at 79°·25, and boils at 217° to 218°. The fused material absorbs large quantities of air,—that is to say, a mixture of nitrogen and oxygen containing nearly 50 per cent of oxygen,—which escapes again on cooling just before solidification takes place. Fused or boiling naphthalene is a powerful solvent and medium for crystallisation for a variety of substances, as sulphur, phosphorus, sulphides, iodine, indigo, &c. As regards the detection of naphthalene the author makes use of the following reaction; to naphthalene is added nitric monohydrate, the mixture diluted with much water, the precipitate washed with water, finally with diluted alcohol (1 alcohol of 90 per cent, 3 water), the residue mixed with a few drops of aqueous potassic hydrate and sulphide, and evaporated to dryness; this residue on addition of alcohol gives a brilliant violet tincture.—(*Ibid.* cii. 29.)

Double Chloride of Thallium and Iron.—Wöhler. When freshly precipitated and still moist thallic chloride is added to a concentrated solution of ferric chloride containing much strong chlorhydric acid in excess, a red precipitate is formed, the composition of which is $3TlCl + Fe_2Cl_3$. Another way for preparing this compound consists in fusing thallic chloride in the vapour of ferric chloride. The double chloride dissolves in hot strong chlorhydric acid, and separates on cooling in red prismatic crystals. Water decomposes it immediately, throwing down white thallic chloride.—(*Am. Chem. Pharm.* cxliv. 250.)

Cerium.—Wöhler. On fusing the chlorides of the cerite metals with sodium, globules of reduced metal are obtained which seem to consist principally of cerium; the colour of the metal is between that of iron and lead, and it is nearly as soft as lead; its specific gravity is about 5·5. If a globule is heated suddenly to a high temperature in a blow-pipe flame combustion takes place, accompanied with an explosion, and sparks of most intense luminosity are thrown out. From the portion of the flux in which the metal is found imbedded, a glittering dark purple crystalline powder may be isolated by digestion with water; this compound is an oxychloride of cerium, its composition being represented by the formula, $CeCl + 2\,CeO$.—(*Ibid.* cxliv. 251.)

Determination of Ammonia.—O. Meister shows that ammonia or its salts when in a greatly diluted state, as in waters for example, may be successfully determined by evaporating one or two litres of the solution in question with the addition of about 5 grammes of sulphuric acid, and distilling the residue with a solution of sodic hydrate, previously boiled, into sulphuric or chlorhydric acid of known strength.—(*Naturf. Gesellsch. Zurich*, 1867, 172.)

Distillation.—P. Pellogio describes a contrivance by means of which the troublesome "bumping" peculiar to certain liquids when under distillation may be entirely prevented. It consists of a glass tube as wide as practicable, inserted through the tubulus, and reaching nearly to the bottom of the retort, and having the upper end bent at a right angle, and drawn out to nearly capillary dimensions, thus establising a communication between the outer air and the interior of the retort. With the help of this arrangement such liquids as methylic alcohol, sulphuric acid, petroleum residues, &c., distil as smoothly as alcohol or water.—(*Zeitschr. Analyt. Chem.* vi. 396.)

Acrolein.—A. Claus. If a solution of potassic hydrate, alcoholic or aqueous, is saturated with acrolein, and sulphuric acid be added, a precipitate of hexacrolic acid is obtained, and from the mother liquor acrylic acid may be distilled off.—(*Naturf. Gesellsch. Freib.* i. Br. 1867.)

Oxidation of Amylic Alcohol.—A. Claus. In a cylinder were placed, without mixing, nitric acid (specific gravity 1·5), water, and amylic alcohol. After about four months the smell of the alcohol had disappeared, that of amylic valerianate having taken its place. The mixture was then diluted with water and half of it distilled off; the distillate consisted chiefly of the ether, and the residue on further concentration gave off much nitric acid vapour, and on cooling separated crystals of oxalic acid.—(*Ibid.* 1867.)

CORRESPONDENCE.

PRESERVATION OF MEAT.

To the Editor of the Chemical News.

SIR,—Having worked in conjunction with Prof. Gamgee at the development of the meat preserving process, I can answer for him the inquiries of your correspondent. The sheep sent over from England were preserved in substantially the same way as the meat mentioned in the CHEMICAL NEWS, vol. xv., p. 135, viz., by treatment with carbonic oxide and sulphurous acid. The carcases were whole, and merely packed in wooden boxes containing soft material to prevent bruising. The details of the process are of course given in the English, United States, and other patent specifications, to which I must refer all seeking information,—I am, &c.,

WALTER NOEL HARTLEY.

Marah 30, 1868.

ROYAL SCHOOL OF MINES.

To the Editor of the Chemical News.

SIR,—In his last letter, "A. L. E." proposes, *en passant*, a change in the curriculum of this School, to which, it appears to me, much greater prominence is due. I refer to the passage in which he mentions the desirability of increasing the present staff of professors by two, viz., on botany and mathematics.

When the late Professor Forbes was alive, the lectures of general natural history were rightly named, inasmuch as they included both zoology and botany; but Professor Huxley, his successor, does not include botany in his course. Now, I suppose none would blame the Council for preferring such a man as Professor Huxley to another of inferior talents who would include both subjects; and had the Council supplemented their election of Professor Huxley to the chair of zoology by another election to the botanical chair, all would have been satisfied. As it is, however, the whole of the botany taught in this School is limited to the lectures on palæontology, where it is, *ex necessitate*, only briefly treated.

Such a step would be a boon, not only to the students of this school (for then I should have little excuse for thus trespassing on your columns), but to the whole mass of London students. For as the present course on zoology, yearly delivered by Professor Huxley, is unequalled, I think I may with safety say, in the British Islands, so, I suppose, would it be the case with the botanical lectures. The difficulty of obtaining, at the present time, any course of lectures on this subject, which goes into the science at all deeply, is too well known to those who have tried to find such lectures, to need comment from me.

I find that the length of my letter forbids me entering into detail on the subject of the mathematical course; I therefore leave it, and with less regret, since the relations of mathematics to physics and mechanics are so very apparent.

I will not waste your space by apologies for trespassing thereon; and thanking "A. L. E." for raising the discussion, and yourselves for so kindly opening your columns thereto,—I am, &c.,

AN EXHIBITIONER, R.S.M.

THE PERMANGANATE WATER-TEST.

To the Editor of the Chemical News.

SIR,—Will you permit me to make a few observations on the paper which appeared in your number of the 27th of March, on the nature and examination of the organic matter in potable waters? It is from the sanitary point of view that I propose to consider the subject, because, as was remarked by the author of that paper, it is the fitness or unfitness of water for drinking purposes, and the quality and effects on the health of the organic matter contained in water, rather than its quantity, which are the points that give importance to the subject.

Some seventeen years ago Professor Forchhammer, of Copenhagen, proposed to estimate the quantity of soluble organic matter in water by permanganate of potash. After a considerable lapse of time his proposal was extensively adopted and relied upon by some chemists, as a means of volumetrically determining the amount of organic impurities present in water. Subsequent observations have, however, demonstrated that for the purpose of estimating the quantity of organic matter contained in water, permanganate of potash cannot be relied upon.

In the meantime (some twelve years ago) Mr. Condy, of Battersea, having discovered the disinfecting properties of the alkaline permanganates, proposed to apply his solution of those salts (Condy's Fluid) to the determination of the quality of the soluble organic impurities of potable waters, and succeeded in satisfactorily demonstrating that his method was not only a quick and ready, but likewise a reliable sanitary test for soluble organic matter in water. Having been adopted in several of the public services, and used extensively among medical men during a period of many years, his process must be considered firmly established. I could myself detail many striking proofs of the sanitary value of this test which have come under my notice in the course of my experience as a lecturer on hygienic subjects at various institutions throughout Great Britain, but will confine myself to a recent instance. Having been supplied, through the kindness of Dr. Gimson, with specimens of water from all the wells of Terling, I easily detected the infected waters by this test alone, and on handing him my results they were found to be exactly borne out by his experience of the course taken by the serious epidemic of typhoid fever which has been prevalent there during the last three or four months.

While, therefore, permanganate of potash has been found comparatively worthless for estimating the quantity of organic matter in water, its value as a quick and ready sanitary test for the quality of the organic impurities of potable waters has been placed beyond doubt.

It would, consequently, seem to me that the way in which the question of the utility of the permanganate test has been dismissed, in the paper under consideration, is calculated to propagate error and to discredit one most useful and reliable application of it, for showing inadequate reason that another and totally different application has proved to be faulty and comparatively worthless.

The study of the effects of permanganate on organic matter shows that, as a rule, it acts with great rapidity on

such matter when in a putrescent or offensive state, but slowly on sound or harmless organic substances. Thus its complete action upon water mixed with pure organic matter would be a matter of days; whereas, on the other hand, if the sample be allowed to stand till decomposition sets in, the permanganate would act with great rapidity on the decomposed portion. Dr. Angus Smith, in a pamphlet on this subject privately printed and circulated (but not published, so far as I am aware), says: "The organic matter which decomposes the chameleon in a minute or two, must be carefully noted; but generally there is a greater quantity which decomposes very slowly: the result obtained for the latter is, I believe, of less value. Generally considerable permanency is obtained in ten or fifteen minutes; then the slow decomposition begins of quite another quantity of organic matter, requiring hours or even days. The amount decomposed instantly is a true measure of the putridity." It is a most important matter, therefore, in considering the action of permanganate on water, to keep clearly in view these several actions which actually give the real practical value to that substance as a test for, and destroyer of, dangerous organic matter. It affords the most rapid and ready mode of detecting the offensive and dangerous substances, while it leaves the inoffensive and harmless matters comparatively untouched. It is evident that when a speedy test for the existence in water of impurities to which the origin of disease in a household may be due, a reagent is not wanted which would expend its chemical force on such harmless matters as sugar, gum, starch, and the like, but one which can seek out and reveal offensive organic substances, which, in the words of the paper in question, are often of "such deleterious nature, that our ideas of the most virulent matter fall short of the horrible results that these invisible poisons can accomplish."

By turning to the Chemical News of the 7th of February last, it will be seen that, at a meeting of the Manchester Literary and Philosophical Society, Dr. Angus Smith confirmed his above-stated views, by declaring that "the condition of organic matter in water can be estimated for sanitary purposes sufficiently by permanganate of potash," which is precisely the proposition first advanced by Mr. Condy ten years ago.

Some little time since Professor Attfield published, through the medium of the *Times*, a somewhat ingenious extemporary method of recognising pollution in water, by means of the sense of smell. But the time required by this method is sometimes so great, and the nose, especially among persons who have not been used to discriminate faint odours by the olfactory organ, is so inferior to the eye, that were it even more accurate and reliable than it is, Professor Attfield's test, though well worth knowing as an excellent make-shift in certain exceptional cases, may be set aside as having nothing to recommend it for ordinary occasions.—I am, &c.,

John Muter.
Author of "The Alkaline Permanganates and their Medicinal Uses."

Richmond Terrace, March 30th, 1868.

DETECTION OF ADULTERATED FLOUR.

To the Editor of the Chemical News.

Sir,—Your querist, "Q.," asks for a method of detecting the adulteration of wheaten flour with the flour of the seeds of the leguminosæ, *i.e.*, beans, lentils, peas, &c., other than the detection of the difference of the starch globules of the latter meals by means of the microscope. The wheaten flour suspected to be thus adulterated is made into a paste with water, the paste is placed in a clean linen cloth and is kneaded under a constant stream of fresh water, until the water runs off quite limpid; in other words, all the starch and matters soluble in water are washed out, and the gluten is retained in the cloth. The following points deserve to be noticed:—

(*a*) Whether the paste does not emit a peculiar smell not met with in paste made with unadulterated flour.

(*b*) Whether it exhibits a peculiar fatty appearance.

(*c*) Whether the water does not exhibit a soapy appearance.

(*d*) Whether the gluten which remains on the cloth exhibits the proper degree of toughness and elasticity, so characteristic of the gluten from pure, sound, wheaten flour.

The water which has served for washing, is collected, and after having been well and vigorously stirred up, is divided into two equal portions. One of these is left standing exposed to a temperature of from 70° to 86° F., in order to try whether foul fermentation does not set in; this does *not* take place at all, in case the flour under examination, and treated as described, were sound wheaten flour, since in that case only lactic acid is formed under the conditions just alluded to; the other portion of the water is first diluted with some distilled water, rendered alkaline by the addition of some liquid ammonia. The fluid is then left quietly standing, until all the starch has settled down, is next filtered, is then submitted to evaporation on a water-bath, until a kind of pellicle, or skin, is observed to be formed on its surface; it is then cooled down, filtered, in order to separate coagulated albumen, while to the clear filtrate next acetic acid, in slight excess, is added. If the addition of this acid causes a precipitate, it is possible that such might be due to the presence of legumin, owing to the adulteration of the wheaten flour by means of the ground-up seeds of leguminosæ; but since the precipitate might be due to immixtures in the wheaten flour of buck-wheat meal, colza-cake meal, indian-corn meal, or even accidental presence of chloride of sodium, it is requisite to collect the precipitate on a filter, to wash it with warm distilled water, and to submit it to the following tests:—

(*a*) To observe whether it is colourless, and devoid of smell and taste.

(*b*) Whether on drying it becomes horny, hard, and translucent.

(*c*) Whether it is tinged blue by iodine.

(*d*) Whether it is, or is *not*, soluble in hot or cold water.

(*e*) Also, insoluble in alcohol (pure alcohol, not methylated spirit).

(*f*) Soluble in a weak solution of caustic potassa in ammonia, and precipitable therefrom by means of hydrochloric, nitric, and acetic acids, all of which reactions refer to legumin.

The precipitate or sediment of the washings, of the flour under examination is next divided into two portions; the smaller of the two portions is again divided into two parts, and to one of these is added a solution of caustic potassa, containing 10 per cent solid alkali, while to the other is added some dilute hydrochloric acid; in both instances the starch is by these means dissolved, and if some of the fluid is placed, under proper conditions, under a microscope with a magnifying power of 300, there will, in case any meal of peas, beans, lentils had been present, be seen the remnants of the broken-up cellular tissue peculiar to these seeds, and exhibiting a peculiar net-like texture. The larger portion of the sediment may be carefully washed out with water, so as to obtain six different wash-waters, all depositing sediments of starch, the last, or sixth of which will contain the peculiar starch of the seeds just alluded to. Fresenius has called attention to a peculiar difference in the ash of the wheaten meal, or flour suspected to be adulterated with leguminosæ seeds. Such ash is—1st, deliquescent; 2nd, its aqueous solution tinges turmeric paper brown; 3rd, there will appear, on addition of nitrate of silver solution to such ash, a precipitate of chloride of silver which, on exposure to daylight, becomes

discoloured. Pure and sound wheaten flour does not contain any chlorides at all, and though the solution of its ash yields a precipitate with nitrate of silver, that precipitate remains unchanged by exposure to daylight. Reddened litmus paper is rendered blue when brought into contact with the solution of the ash of pure, sound wheaten flour, but turmeric paper remains unchanged. Louyet further observes that the quantity, also, of the ash may serve as a criterion ; dried old wheaten flour only yields 1 per cent of ash, rye meal about the same; but the meal of peas and beans yields never less than 3 per cent; the addition therefore of even 10 per cent, by weight, of meal of peas or beans to wheaten flour will sensibly alter the quantity of ash the latter ought to leave behind on incineration executed with proper care.

It may therefore be assumed that, if wheaten flour yields more than one, but below two per cent of ash, it is adulterated with meal of peas, &c., since an intentional adulteration with metallic or earthy matters will almost always be so carried out as to bring the ash far above 1 per cent.

Donny states that if wheaten flour is adulterated with the meal of white beans (haricot beans), or with that of lentils, provided, however, the quantity thereof be not below 5 per cent, and such meal is so placed in a deep porcelain basin as to cover with a slight thin layer the sides of such vessel, while the bottom is left uncovered, that then when first a few drops of strong nitric acid are allowed to evaporate from the bottom, and immediately after some ammonia, reddish specks will make their appearance in the flour along the sides of the vessel, which red specks indicate the presence of the meal of these two substances. A better test is to exhaust the suspected flour with pure warm alcohol, to evaporate the latter, to treat the alcoholic extract with ether, and to expose the residue thereof, while air has free access to the vapours first of nitric acid next of ammonia, the residue will, if either the meal of white beans or lentils had been present exhibit a beautiful amaranth pink colouration,—I am, &c.,

Dr. A. Adriani.

MISCELLANEOUS.

Select Committee on Scientific Education.—In the House of Commons on Friday last on the motion of Mr. Samuelson it was agreed that the Select Committee on Scientific Instruction do consist of 18 members :—Mr. Acland, Mr. Akroyd, Mr. Bagnall, Mr. Bazley, Mr. Henry Austin Bruce, Mr. Berecroft, Lord Frederic Cavendish, Mr. Dixon, Mr. Graves, Mr. Gregory, Mr. Thomas Hughes, Sir Charles Lanyon, Mr. M'Lagan, Lord Robert Montagu, Mr. Edmund Potter, Mr. Powell, Mr. Read, and Mr. Samuelson.—*Times*.

Modern Physical Science.—The following forms the conclusion to a course of lectures on "Heat," recently delivered at Eton College by Mr. G. F. Rodwell. "I have said much in the foregoing lectures concerning the motions possible to particles of matter, because I have wished to impress upon you the fact that physical science is daily becoming more and more a science of kinetics,—a science which resolves the direct acting causes of phenomena into motions of particles, variety of form being induced by variety of motion. We have done with imponderable fluids, their media, subtle essences, and with all the meaningless terms which have from time to time been proposed to designate the so-called Physical Forces. We now regard them as attributes of matter, inseparable from matter; as actions possible to small particles, whether they be extremely extended as in the ether which pervades space, or comparatively close together as in the metals. Forces differ from each other either because the velocity of the motion which constitutes them varies, or because the motions themselves differ in form. All matter possesses motion. We have seen that if we increase one kind of motion to a certain extent the substance possessing it becomes what we call hot, while if that motion be further increased light appears ; hence the inference that the motion called light is an intensified form of the motion called heat, that is that the difference is one of velocity, not of form or character. Again we have seen that the motion of heat interferes with the propagation of the motion called electricity; hence the inference that the one motion differs in form and character from the other, and not simply (as in the case of heat and light) in velocity unaccompanied by change of form. We can readily comprehend this if we bear in mind that the particles of a mass of matter moving with a certain kind of motion, cannot so readily assimilate a motion of a different kind as if they were at rest at the time of its inception, or as if the new motion were similar in form, and differed only in velocity. In the study of physical science we must apply that which we know of the movements of visible masses of matter to the movements of unseen particles : we must construe occult and invisible actions by the aid of visible and known actions; it is thus alone that we can form any ideas of the nature of the motions which appertain to the small particles of matter. This is the basis of a true kinetic system. In connection with the formation of such a system I am constrained to mention one who is too little recognised in the present day. A man who possessing illimitable capability and almost universal knowledge had yet learned to guide his intellect by recognised laws of thought : to make it accurate, and subtle, and profound, and infinitely calm in its immensity. A man habituated to intense meditation, to exact mathematical reasoning, to a careful observation of nature. Lucid in his mode of thought, sound in his judgment, he was actuated by a pure love of truth, by a desire to produce a legitimate familiarity between mind and matter, by a grand admiration amounting almost to reverence for the wonders wrought in the Universe. I speak of René Descartes, the founder of the first comprehensive kinetic system; the founder of a physical philosophy which is the ancestor of that into which we are now merging. Our recent physical philosophy may almost be called neo-Cartesianism. In conclusion, I will ask you to bear in mind, that although the forces of Nature undergo perpetual transformation, there is in all cases an equivalence of transformation. We know that if we have a mass of matter, A, moving with a definite velocity, we can so dispose this action that it may appear in a mass, B, as motion of another kind, but of precisely equal value; we know also that if a mass moves with a certain velocity, the sum of its action may be truly represented by a smaller mass moving with a greater velocity, or by a larger mass moving with a less velocity. It is thus with the forces of Nature : a definite amount of a force, A, if it be transmuted into a force, B, appears in one definite amount truly representing A, and competent to assume the form of A ; or let a force, A, be transmuted into other forces, B, C, D, and although A has assumed so many different actions, the totality of its action still prevails,—the diverse forms are capable of being re-transmuted into the original form. A latent force is like a compressed spring, it is in a condition of potentiality. Whether a force be acting or at rest it represents the same amount of action it differs only in this, that while it acts it yields up its individuality of form, whereas while it is at rest it maintains its individuality of form. The forces which produce the phenomena of the Universe move in a circle which is never broken. There is an eternal round of change. A force altered in form, and apparently caused to disappear in inducing natural phenomena becomes force of another kind, sometimes actual, sometimes potential. The phenomena of the Universe are the measures of transformed force."

Torpedo Experiments.—A short time ago, a trial was made on board H.M.S. *Lightning*, at Pembroke, in the presence of some of the members of the Floating Obstruction Committee, of Captain Harvey's new sea torpedo, which gave great satisfaction. As a distinct private experiment, a small torpedo, charged with 20 lbs. of a highly explosive agent (equal to about 100 lbs. of gunpowder), the invention of Mr. Horsley, scientific chemist, of Cheltenham, but formerly of Portsmouth, was brought in contact with a vessel, which was instantly and totally destroyed with a grand effect, the report of the explosion being heard for miles round Pembroke. There is no doubt about its being one of the most destructive engines of war yet invented, and will make short work of an enemy. —*Hants Telegraph and Sussex Chronicle.*

Applying Charcoal to Sewer Ventilators.—In a recent number we gave an abstract of the report to the Metropolitan Board of Works "On the Ventilation of Sewers," by Dr. Miller. Our attention has since been called to the fact that so early as 1862, the proposal of Dr. Miller had already been successfully carried out in the City of London by Dr. Letheby and Mr. Haywood. The district experimented upon was in the Eastern portion of the City of London, comprising a space of about fifty-nine acres, with about 1,700 houses, and about 14,000 inhabitants. Wood charcoal was employed, broken into pieces of the size of a filbert. It was packed closely, but without compression, upon the various trays; and each tray held about 1$\frac{1}{2}$ lbs. of charcoal, making altogether 6$\frac{1}{2}$ lbs., distributed over the six trays of each air filter. The experiment was commenced on the 14th of July, 1860, and continued for a period of rather more than eighteen months. The general conclusions from these experiments, and from the consideration of collateral evidence are:—that dry charcoal in the presence of atmospheric air is a powerful means of destroying the mephitic gases and vapours of sewers and house drains; that the charcoal filters may be used with efficacy in the course of the air channels from the drains and closets of houses, as well as in the ventilation of the public sewers; that in applying the charcoal, those contrivances should be used which offer the least resistance to the free passage of the air; that the situation of the filters is best, when the charcoal is protected from wet and from dirt, and is easily accessible; that from the ascertained efficacy of charcoal in destroying the dangerous emanations from sewers, the system may be generally applied with great advantage; and that from the experience derived from this extensive practical inquiry, they are satisfied that the expense of the system might be considerably reduced below that indicated by the cost of the experiment.

Professor Gamgee's Method of Preserving Meat.—The necessity of some plan for preserving meat has long been felt. Hence it is that every plan, as soon as announced, is seized by the anxious public. If we may believe late reports from London, this desire is at last soon to be gratified, and in a manner which will leave nothing desirable unaccomplished. It seems that Professor Gamgee, President of the Albert Veterinary College of London, author of several works upon the cattle plague, and a recognised authority in such matters, discovered a new process for preserving meats, which he has patented in Europe and America. The process is simple and quite inexpensive. The animal, when practicable, is caused to inhale carbonic oxide gas. Before it is quite insensible it is bled in the usual way. When dressed the carcase is suspended in an air-tight receiver, the air exhausted, and the receiver filled with carbonic oxide gas; a small quantity of sulphurous acid gas is also added. After remaining here for from 24 to 48 hours, meat may be removed, and hung in a dry atmosphere; it will keep for one, two, or three months, or longer, with no perceptible change in taste or appearance. The tests of the method thus far applied have been attended with success. Beef killed in London in March last was sent to New York in June, and as late as the middle of July was shown to a prominent butcher in Fulton market, who did not discover that it was other than ordinary beef, and expressed the opinion that it had probably been killed about two days. Mutton killed in London last July, and sent to this city soon after, is now perfectly fresh, and one piece of beef kept for ten days in a can surrounded by water at a temperature of 90 to 100 degrees, came out perfectly fresh. The process, in the opinion of eminent chemists, does not injure the meat in the least, which is an advantage very difficult of attainment, even in the case of transportation of live stock, which is liable to the bad effects of confinement and the length of the journey. Among the beneficial results of the adoption of this scheme would be a better supply in our markets of wholesome meat and at a desirably cheaper rate. It is expected that Prof. Gamgee will soon visit this country for the purpose of inaugurating his project.—*American Journal of Mining.*

Production of a Fragrant Substance from Resin (probably an essential oil).—Common resin, lac, or kauri gum, in a state of powder, is gently heated with somewhat dilute nitric acid for a few hours; the mixture or the solution, as the case may be, is then evaporated to dryness, or nearly so, and treated with an excess of a strong solution of common soda, caustic potash, or lime in water; the resulting liquid is then transferred to a retort, and distilled; at first the distillate has an odour of garlic, but this gradually gives way to an odour decidedly fragrant. On redistilling the portion last drawn over from concentrated sulphuric acid, a strong aqueous solution of this odourous substance is obtained, the solution itself has a warm aromatic flavour, and the odour assimilates to that of peppermint mixed with lavender. Bichromate of potash with sulphuric acid, also, may be used for the oxidation of the resin employed. —W. Skey.

Champagne from Petroleum.—It is no longer a secret of the chemist's laboratory that clear golden syrups can be made from starch and sulphuric acid; that delicious wines and brandies can be made from beet-root; that a barrel of peanuts can be transformed into excellent coffee; that lard can absorb an enormous quantity of water in certain conditions; that in fact there seems no limit to the adulterations that an intelligent and dishonest chemist can practice upon his fellow-men. All these marvels of chemical science have in these latter days become degraded into mere tricks of trade, and their chief beauty is in their capacity to enable unscrupulous dealers to lighten the pockets and destroy the stomachs of the confiding and consuming public. Concerning the article of champagne, a writer in the Portland (Maine) *Star* tells us that it is made from a thousand different substances—even from refined petroleum. Yes, from the fiery benzoles a sparkling, bubbling, foaming champagne can be produced which will delight the eye, tickle the palate, gladden the heart momentarily—but quicken our paces toward the graveyard. This is a new use for petroleum, which those who have been experimenting with it as an agency for generating steam have little dreamed of. Who can say that the Pennsylvania oil territory, now considered mostly worthless, may not some day be regenerated into the great champagne-producing country of the world.—*Cincinnati Journal of Commerce.*

Patents by Scientific Men.—"Wollaston was fond of amassing money. There have not, indeed, been wanting accusations to the effect that if he had sought less after wealth, he would have done more for science. How far these charges are true, we have no means of judging, as it does not appear from the published accounts in what exact way he made his money. That it was chiefly by the platina process is certain; but whether he engaged in the manufacture himself, or only superintended it, we do not know. On this point we would only remark, that there is something, to say the least of it, very partial and unfair in the way in which obloquy is cast upon men of

science, if they appropriate to themselves some of the wealth which their discoveries procure for others. If a successful naval or military hero is lavishly pensioned out of the public purse, no one complains. It is not thought strange that a great painter or sculptor, while he justly declares his productions are worth untold gold, should, nevertheless, demand a modicum of coin from his admirers. Neither is the poet or musician blamed who sells his works to the highest bidder. But if a chemist, for whom there are few pensions and no peerages, thinks to help out a scanty or insufficient income, by manufacturing gunpowder, like Davy, or magnesia, like Henry, or malleable platina, like Wollaston, or guano, like Liebig, the detractors assail him at once. He has lowered the dignity of his science, and, it would seem, should starve rather than degrade his vocation. That vocation, so far at least as the practical fruits of his own labours are concerned, is to be a kind of jackal to start game which others are to follow—a beagle to hunt down prey which others may devour. Surely there is but scanty justice here, and some forgetfulness of a sacred text:—'Thou shalt not muzzle the mouth of the ox that treadeth out the corn, &c.'"—*From the British Quarterly Review, No.* vii. *(August,* 1846), *Article* 3, *p.* 104.

CONTEMPORARY SCIENTIFIC PRESS.

Under this heading it is intended to give the titles of all the chemical papers which are published in the principal scientific periodicals of the Continent. Articles which are merely reprints or abstracts of papers already noticed will be omitted. Abstracts of the more important papers here announced will appear in future numbers of the "Chemical News.")

Archives des Sciences.
November 25, 1867.

M. DELAFONTAINE, "On some new and little known Molybdates, and on the principal Fluoxymolybdates." J. L. PREVOST, "Researches on the Poisonous Action of Veratrine."

Journal des Fabricants de Papier.
December, 1867.

E. BOURDILLIAT, "On Testing the Chemical Products used in Paper Making. (Continuation.) Vegetable Colouring Matters." MANSTA, "A new Composition for rendering Canvas Waterproof."

Comptes Rendus.
December 9, 1867.

A. SECCHI, "On Stellar Spectra, and on Shooting Stars." E. BOURGOIN, "On the Electrolysis of Acetic Acid."

Monatsbericht der Koniglich Preussischen Akademie der Wissenschaften zu Berlin.

C. A. MARTIUS, "On Binitronaphthol."

Sitzungsberichte der Kaiserlichen Akademie der Wissenschaften zu Wien. (Mathematisch-Naturwissenschaftliche Classe.)
June, 1867.

E. BORICKY, "A Contribution to the History of the Formation of the Phosphates of Iron: Dufrenite, Beraunite, and Kakoxene from the Hrbek Mine near St. Benigna, Bohemia." V. VON ZEPHAROVICH, "Mineralogical Notes, Part II.: 1. Barrandite from Cerhovic and Sphærite from Zajecov. 2. Boulangerite and Jamesonite from Pribram. 3. Mispickel. 4. Löllingite and Leucopyrite."

Bulletin de la Société Chimique de Paris.
November, 1867.

BERTHELOT, "On the Simultaneous Formation of Homologous Bodies in Pyrogenous Reactions." E. PERRET, "On Refining Crude Camphor."

Annalen der Chemie und Pharmacie.
November, 1867.

E. LINNEMANN, "On the Transformation of Amine Bases into the corresponding Monatomic Alcohols." A. SIERSCH, "On the Transformation of Ethyl-Alcohol into Propyl-Alcohol." A. SAYTZEFF, "On the Action of Iodide of Methyl on Sulphide of Amyl-Ethyl." "On the Action of Nitric Acid on Sulphide of Methyl and Sulphide of Ethyl." N. MORGUNOFF, "On Stannidimethyldiethyl." F. BEILSTEIN and U. KREUSLER, "On Para-nitrotoluylic Acid and its Derivatives." C. SCHORLEMMER, "Contributions to the Knowledge of the Hydrocarbons." M. FLIESCHER, "On Thionessal." L. SCHELLER, "On some Double Salts of Sulphate of Uranium." F. WOHLER, "On a Compound of Chloride of Thallium with Perchloride of Iron." "Contributions to the Knowledge of Cerium."

Supplement.

L. MEYER, "On the Molecular Volume of Chemical Compounds." H. SCHIFF, "Researches on the Boracic Ethers." J. ERDMANN, "On the Composition of the Wood of *Pinus abies*." H. L. BUFF, "On the Transformation of Monochlorhydrin into Propyl-glycol and Lactic Acid, and of Bichlorhydrin into Isopropylic Alcohol and Acetone."

NOTES AND QUERIES.

Estimation of Chlorine.—Can any one inform me of any more ready and reliable mode of estimating the feebly combined chlorine in bleaching powder, than the old protosulphate of iron test?—S. DUNN.

Palm Oil for Softening Dyed Yarns.—Would any of our kind friends inform me if there is any method to make palm oil mix with water, without the use of alkalies; if not, the best means to mix the above, so that it will not be injurious to colours?—NEEDFUL.

Electro-deposition of Iron.—Having occasion to electro-deposit iron on to a metallic surface of copper, and having at hand a solution of ferrous sulphate, slightly acidulated with sulphuric acid, and consisting of 1 part of the crystalline salt to 5 parts of water, I desired to ascertain, before making a stronger and, perhaps, more suitable solution for the purpose, whether the deposit from this solution would be reguline. On applying the galvanic current from three of Smee's battery cells (arranged in series) to this solution in the cold, I found that a reguline deposit of white, silvery-looking iron was obtained, but with the evolution of hydrogen gas in considerable quantity. Although certain alkaline solutions are known that will readily give up their metal in a reguline form during the rapid evolution of hydrogen, thus forming exceptions to "Law I." in "Smee's Electro-Metallurgy" (3rd ed., p. 150), this is the first instance that has been met with, to my knowledge, of a solution similar to the above, and containing no alkali or other metal than the one to be thrown down, giving, by electrolysis, a reguline metallic deposit during the evolution of hydrogen. Each Smee's cell had 18 square inches effective area of positive surface; but the area of the anode and cathode, respectively, was 2 square inches. The battery exciting liquid was 1 part of oil of vitriol to 20 parts of water. As evidenced by a galvanometer in the circuit, the addition of a solution of sulphate of ammonium to the solution increased its conducting power; with this latter solution a very serviceable coating was obtained, but still with the evolution of hydrogen gas. I give these details in order that others may try the experiment, if they feel so inclined.—W. H. WALENN, F.C.S., 19, Talbot Road, Tufnell Park West, N.

MEETINGS FOR THE WEEK.

MONDAY.—Royal Institution, 2. General Monthly Meeting.
WEDNESDAY.—Geological, 8. "On the Affinities and probable Habits of the extinct Australian Marsupial, *Thylacoleo carnifex*, Owen." By W. H. Flower, Esq., F.G.S., "On the Thickness of the Carboniferous Rocks of the Pendle Hills," by E. Hull, Esq., F.G.S. "On the relative Ages of the leading physical Features of the Carboniferous districts of Lancashire and Yorkshire," by E. Hull, Esq., F.G.S. "On a Saliferous Deposit in St. Domingo," by D. Hatch, Esq. Communicated by Sir R. I. Murchison, Bart., F.R.S., &c.
—— Microscopical, 8.

TO CORRESPONDENTS.

Helta.—Use ammonia in preference to potash for precipitating the earths. You can always get rid of ammoniacal salts by ignition towards the close of an analysis.

John S. M.—The Chemical Society's Journal is no longer published by Baillière, but by Van Voorst.

Assayer.—German silver cannot be assayed in the dry way owing to the difficulty of removing the nickel. In assaying brass and gun-metal the oxides of zinc and tin are so difficultly fusible that the dry process is also inapplicable to these alloys. An alloy of copper and silver, or copper and gold, may be assayed by cupelling with lead and determining the copper by the loss.

Walter H.—A private letter will be sent if you forward your address.

Mineral.—The ore contains copper in considerable quantity, and also some silver. If you wish to have a quantitative analysis of it, will you be good enough to communicate with the Editor?

Communications have been received from T. S. Muter; Dr. Adolph Ott; W. Angell; W. F. Barrett; H. Henderson (with enclosure); H. W. Lloyd Tanner; W. N. Hartley; M. Stanilas Meunier (with enclosures); J. Hargreaves (with enclosures and newspapers); T. F. Higginson; John Horsley (with enclosure); Charles Cochrane; W. N. Smythe; Nicholson, Maule, & Co.; Dr. Adriani (with enclosures); A. P. Hurlstone; W. Selton; W. Skey (with enclosures); J. Williams; J. Stevenson; W. Bockhart Smith; Messrs. Longmans & Co.; Neil Matthieson; A. Coppins; J. Robertson & Co.; J. J. Lundy & Co.; W. Hall; Rev. R. Kirwan; J. C. Burrell, Sydney, N.S. Wales (with enclosure); Magnesium Metal Co.; Dr. Watts; Dr. Dupré; C. M. King; Rev. B. W. Gibsone, M.A.; J. H. Atherton.

Books received.—"Official Record of the Intercolonial Exhibition of Australasia." Melbourne: Blundell & Co.; "A Treatise on the Metallurgy of Iron," by H. Bauerman, F.G.S. London: Virtue & Co.; "Popular Science Review;" "Pharmaceutical Journal;" "Scientific American;" "American Journal of Mining;" "American Artisan."

THE CHEMICAL NEWS.

Vol. XVII. No. 436.

ON NAPHTHA AND ILLUMINATING OIL FROM HEAVY CALIFORNIA TAR.

AND ON THE PROBABLE ORIGIN OF PETROLEUM.

By Prof. B. SILLIMAN.

Having lately had an opportunity to examine a specimen of "surface oil," so called, from Santa Barbara county, in California, I present the following experimental results in the hope that they may not be without interest, as an addition to our knowledge of one extreme of that class of hydrocarbons, which occur in nature in the fluid form, and of every density, from those which are but little lighter than water, down to the lightest naphtha found in a natural state.*

It is proper to state that the chemical examination of this sample had chiefly a technical object, to prove whether or not illuminating oil of good quality could be obtained from the distillation of so dense a body. The experiments were conducted on quantities of from five to ten gallons each. The crude oil was very dark, almost black, transmitting yellow brown light in thin films. At ordinary temperatures (60° F.) it is a thick viscid liquid resembling coal tar, but with only a very slight odour.

Its density at 60° F. is 0·980, or 13½° Baumé. It retains, mechanically entangled, a considerable quantity of water, which is neutral in its reaction. The odour of sulphydric acid, which is very decided in this product, as I have noted in its locality, had entirely disappeared in the specimen under consideration.

The tar froths at the commencement of distillation, from the escape of watery vapour. It yields by a primary distillation no product having a less density than 0·844 or 37° B. at 52° F.

Distillation to dryness produced in two trials an average result as follows:—

Oil having a density of 0·890 to 0·900	69·82
Coke, water, and loss	30·18
	100·00

In one of these trials the product was divided as follows:—

Oil of density 29° B. at 52° (885 sp. gr.)	50·
Oil of density 24·75 B. at 58° (908 sp. gr.)	17·5
Coke, water, loss, &c.	32·5
	100·0

The coke is very large in quantity, strong, and is a good fuel, resembling gas-house coke. The odour of ammonia is given off towards the close of the distillation. It is well known to distillers of petroleum that by the process called "cracking," heavy oils unfit for illumination are broken up into bodies of less density, from light naphtha to the heavier illuminating and lubricating oils. This process is simply the application of a carefully regulated heat producing a slow distillation. By this treatment the molecules apparently re-arrange themselves into groups of different density, which by a subsequent distillation are divided into fractions (or "heaps," as Mr. Warren calls them) of tolerably constant boiling points.

The first distillate, having a density of about ·890 at 6° F., treated in this manner, yielded a product having a density of about ·885 at 60°, or only 1° Baumé lower than before distillation. After treatment with sulphuric acid and soda and redistilling from soda, it had a density of ·880 at 60° F. Upon distilling, 100 measures of this last distilate yielded:—

Light oil having a density of about ·835 at 60° F	21·58
Heavy oil having a density of about ·880 at 66° F.	37·41
Heavy oil having a density of about ·916 at 64° F.	34·53
Coke &c.	6·48
	100·06

In another experiment undertaken with a view to "cracking," &c., treating and re-distilling with soda, the products were as follows, stated in percentages of the whole quantity operated on, the several steps being as before:—

Naphtha,† sp. gr. about ·760 at 60° F.	11·33
Oil,‡ sp. gr. about ·836 at 60° F.	66·22
Oil, sp. gr. about ·893 at 60° F.	12·67
Oil, sp. gr. about ·921 at 60° F.	3·56
Loss	6·22
	100·00

The illuminating oil from both these experiments, after treatment with suiphuric acid and soda in the usual manner, acquired an agreeable odour, a light straw-yellow colour, and burned as well in a lamp as good commercial oil.

With a view to test the effect of heat aided by pressure in breaking up the heavy hydrocarbons—a method of treating heavy hydrocarbon oils patented in 1866 by James Young, of Glasgow—a portion of the first distillate from the crude oil was subjected during distillation to a pressure of 10 to 15 pounds to the square inch, in an apparatus adapted to the purpose, the distillate thus obtained being about the same density as in the first-named experiment, ·890 at 60° F.

From this distillate were obtained, after the ordinary treatment with sulphuric acid and soda, the following products:—

Light oil, sp. gr. ·825 at 60° F.	19·2 per cent.
Heavy oil, sp. gr. ·885 at 60° F.	25·86 ,,
Heavy oil, sp. gr. ·918 at 60° F.	38·14 ,,
Coke, loss, &c.	16·80 ,,
	100·00

The illuminating oil from the last experiment flashed at 80° F., and lighted on the surface at 85° F., showing the presence of naphtha or some very light body, the quantity of which cannot be very considerable. The light oil could, with care, be taken off in practice without materially diminishing the yield of illuminating oil. It would be rash to conclude that there may not be an important economical advantage in employing in the large way, Mr. Young's method of treatment, under pressure, over that of "cracking" by a regulated heat alone. It is highly probable that there would be found an important saving of time, as under a regulated pressure, and a corresponding increase of temperature, the transformation of the heavy oils into a mixture of less density will occur more speedily. The experiments herein mentioned gave nearly the same result, whether pressure was used or not; a certain loss, all falling upon the lighter portions, was found to result from leakage of the apparatus under

* I am indebted to A. J. Corning, formerly assistant to Mr. Warren, for conducting this research under my direction; and to Messrs. Downer and Merrill, of the kerosene works in South Boston, I am under many obligations for the permission to employ their operative laboratory in conducting this research. For the crude material operated on I am indebted to the Californian Petroleum Company, from whose estate it was derived.

† This naphtha caught fire from a match at an atmospheric temperature of 56° F.

‡ This oil flashed at 113° F., and ignited at 124° F.

pressure, which in the larger way of operating commercially could be avoided.

No paraffine could be detected by refrigerating the heavy oils obtained in these distillations in a mixture of salt and ice. It is, no doubt, the absence of this body from the series of products obtained from the California oils generally, that accounts for the illuminating oil burning well at a density considerably below the commercial standard for oil obtained from Pennsylvania petroleum—a difference enhanced also by the absence of any considerable quantity of light naphtha. The lubricating oils of this series, likewise free from paraffine, retain on this account their fluidity at low temperatures.

The light oils obtained in this series of experiments correspond respectively to 12·96, 14·56, and 18·96 per cent of the crude oil. The total commercial products are about 60 per cent of the crude body, which likewise yields sufficient coke to supply the fuel required in the distillations.

In the large way of returning the lightest oils to the heavier portions in the successive distillations, and employing Mr. Young's method by pressure, it is probable the product of light or illuminating oils may be raised in these very heavy natural products to 30 per cent, and for those of less density the proportion will be correspondingly greater.

It is evident from these experiments that heavy hydrocarbon oils containing no naphtha are convertible into oils of the naphtha series under the action of heat by molecular transformations, the excess of carbon being left behind as coke; each successive distillation eliminating a new, bnt always diminished, portion of carbon. It may, therefore, be confidently affirmed, that even the heaviest of the California hydrocarbons belong to, and are derivatives from, the petroleum series. The transformation of light oils into denser products ending with tar, like that which is the subject of this research, results not, as has been assumed by some, from the addition of oxygen producing an oxidised body, but on the contrary, by the removal of successive atoms of hydrogen in the form of water, thus leaving the carbon in excess, that excess being left behind in the form of coke when the crude product is distilled.—*From the San Francisco Bulletin.*

ON THE

PRECIPITATION OF COPPER AND NICKEL BY ALKALINE CARBONATES.

By WOLCOTT GIBBS, M.D.,

Rumford Professor in Harvard University.

The precipitation of copper by zinc, or by the electrolytic method, requires that the metal should be present in the form of sulphate or chloride, and does not succeed with the nitrate. As stated before, the employment of the hypophosphites is limited to the case in which the metal exists as sulphate. The old mode of precipitating copper as oxide by caustic potash has disadvantages which are familiar to all chemists, but on the other hand is independent of the nature of the solution of copper employed, so long at least as no organic matter is present. According to Rose,* the alkaline carbonates precipitate copper less completely than caustic alkalies. This statement, however, is not accurate for *all* the conditions under which the experiment may be performed; and I have found that copper may be completely precipitated from the sulphate, nitrate, or chloride when the solutions are boiled together for a sufficient time and are sufficiently dilute. Mr. E. R. Taylor, who has made a careful study of this method of determining copper, has arrived at the following as the best method of conducting the process. The solution of copper is to be diluted with water until the liquid contains not more than about 1 grm. of the metal in one litre. A solution of carbonate of potash or soda is then to be added in small excess, and the whole boiled for about half an hour. The boiling proceeds quietly, and without succussions; the blue green carbonate soon becomes dark brown, and has a fine granular character which renders it extremely easy to wash. After washing it is to be ignited in an atmosphere of hydrogen, and the copper weighed as metal; it will be found to be free from alkali. In this manner Mr. Taylor obtained, in five analyses, the following results:—

1·8384 gr. pure sulphate of copper gave 0·4688 gr. metallic copper = 25·44 per cent.

1·7144 gr. pure metallic copper dissolved in aqua regia gave 1·7161 gr. copper = 100·09 per cent.

1·3860 gr. pure metallic copper dissolved in aqua regia gave 1·3853 gr. copper = 99·93 per cent.

1·4657 gr. pure metallic copper dissolved in nitric acid gave 1·4670 gr. copper = 100·09 per cent.

1·4685 gr. pure metallic copper dissolved in nitric acid gave 1·4634 gr. copper = 99·65 per cent.

The filtrate is perfectly free from copper if the process has been well conducted.

The ignited oxide is in a state of great subdivision, and the ignition must therefore be conducted with much care to avoid loss. A small portion of the oxide or basic carbonate usually adheres to the sides of the vessel in which the boiling takes place. This is to be re-dissolved, and again precipitated, but great care must be taken not to add a large excess of the alkaline carbonate, which gives a solution from which the copper is not precipitated by boiling.

Nickel may be completely precipitated from its solutions by precisely the same process. The green basic carbonate may be washed much more readily than the oxide precipitated by caustic alkali; it is to be ignited and weighed as oxide. In two analyses Mr. Taylor obtained the following results:—

1·9808 gr. anhydrous sulphate of nickel gave 0·9551 gr. NiO = 37·79 per cent.

1·4601 gr. anhydrous sulphate of nickel gave 0·7008 gr. NiO = 37·64 per cent.

The formula $NiSO_4$ requires 37·69 ($Ni = 58$). Dr. F. A. Genth informs me that he has also used the alkaline carbonates in precipitating nickel, and with most satisfactory results.

The precipitation of cobalt by an alkaline carbonate can only, with much difficulty and by long boiling, be made complete. As a means of determining cobalt it is not to be recommended. On the other hand, Mr. F. W. Clarke has found that cobalt is completely and easily precipitated by the process of oxidation, first given by Popp,† which consists in neutralising the solution with carbonate of sodium, adding acetate of sodium, and then boiling with an excess of an alkaline hypochlorite, taking care to keep the solution alkaline. The hydrated sesquioxide (?) of cobalt thrown down may be readily washed. After reduction in hydrogen, the metal is found to be free from alkali. Nickel may, as Popp has also shown, be precipitated in the same manner, but the process given above seems to me preferable.

In this connection I may be permitted to state that the method of separating cobalt from nickel by means of peroxide of lead attributed to myself in the new edition of Rose's‡ "Handbuch der Analytischen Chemie," and also ascribed to me by Gauhe‖, was never even proposed by me.

Cobalt and nickel may be precipitated from neutral solutions of their sulphates, nitrates, and chlorides by adding first an excess of oxalic acid to the concentrated

* Handbuch der Analytischen Chemie, ii, 175. Sechste Auflage.

† Zeitschrift für Analytische Chemie.

‡ Sechste Auflage, Bd. ii., p. 143.

‖ Zeitschrift für Analytische Chemie.

solution, and then a large excess of strong alcohol. After standing a few hours the filtrate is perfectly free from metal. The oxalates are very easily washed. This method is, however, rarely available for analytical purposes, since it fails entirely when salts of ammonium or of the alkaline metals are present. The oxalates are also in such a state of subdivision that it is almost impossible to ignite them without loss. The oxides of copper, cadmium, zinc, manganese, and magnesium, are also completely precipitated from their sulphates by oxalic acid and alcohol, but not in presence of alkaline salts. The same is true of both mercurous and mercuric nitrates. In the few cases in which this mode of precipitation will find application in practice, it will probably be best to determine the oxalic acid in the oxalate by hypermanganate of potash.

In a former paper I have stated that the sulphides of cobalt and nickel thrown down from boiling solutions by a boiling solution of sulphide of sodium may be washed without oxidation upon the filter. The difficulty of preparing pure sulphide of sodium has, however, been an objection to this method.§ This difficulty may easily be removed by dissolving the crystallised tetrahedral sulphide, $Na_2S + 9aq$, in alcohol of 90 per cent, filtering, and allowing the solution to crystallise. After two or three crystallisations, the pure sulphide may be dried over sulphuric acid in vacuo, and the white effloresced mass preserved in a well stoppered bottle. The sulphide is chemically pure.—*Amer. Journ. of Science and Arts.*

ON CRYOLITE AND ITS PRODUCTS.

By EVAN T. ELLIS.

This remarkable mineral, which is partially transparent, of a vitreous lustre, and brittle texture, is a fluoride of sodium and aluminium, containing

13	per cent	aluminium,
34	,,	sodium,
53	,,	fluorine,
100		

It is found in an immense deposit in Greenland, at Iviktout, at the head of Arksut Bay, near Cape Farewell. The first discovery was made by one of the missionaries, who carried a specimen with him to Copenhagen. Its true composition was determined by Vauquelin. There is a bed 80 ft. thick, and 300 ft. long, at the above-mentioned place.

It is frequently associated with the salts of metals, and beautiful crystals, of galena, or sulphide of lead, chalybite, or brown spathic carbonate of iron, resembling spar in lustre; copper pyrites with silver, iron pyrites, &c., are found therein, arranged in masses segregated from the white, transparent, ice-like cryolite.

It remained for the Pennsylvania Salt Company to introduce to our country this valuable material. This energetic Company, whose works are in western Pennsylvania, has secured the privilege of using a large part of all that is mined, and has, within two years past, imported into Philadelphia thirteen cargoes, or 9,000 tons, which have been sent to their works for manufacture.* The greater portion of this has been used for their patent saponifier. They are now devoting their attention to the preparation of caustic soda, carbonates, and other salts of soda, sulphate of alumina, &c.

Soda is obtained from cryolite by simply mixing with lime, and subjecting to heat. The fluorine combines with the calcium, forming fluoride of calcium; while the remaining metals absorb oxygen from the air, and become alumina and soda. Carbonic acid is then passed through the solution, forming, with the sodium, a carbonate of soda, which remains suspended, while the alumina, being insoluble, is deposited at the bottom of the vessel. The carbonate of soda is deprived of its acid by means of lime in the usual manner, and thus rendered caustic, and fitted for the use of the soap-maker.

One hundred pounds of cryolite yield—

	44	lbs.	dry caustic soda,
or	75	,,	,, carb. ,,
or	203	,,	crystal carb. ,,
or	119½	,,	bicarb. ,,
and	24	,,	alumina.

The sulphate of alumina contains 2·82 of sulphuric acid to 1 equivalent of alumina, therefore this is more than a neutral salt (3· being neutral), which is very desirable for manufacturers of paper, calico printers, &c.‡ It is also entirely free from iron, another very important characteristic.

There is another very important use to which cryolite can be applied. By a fusion of 1 part of cryolite with from 2 to 4 of pure silex, a beautiful glass is formed, susceptible of mould and polish, and capable of being manufactured into an endless variety of useful and ornamental articles, and probably many utensils for chemical and pharmaceutical use will be made of it. A company has been operating in Philadelphia for some time past, on an experimental scale, entitled the "Hot Cast Porcelain Company." The results have been so satisfactory that they have now taken a large establishment, and will be prepared to carry on the manufacture quite extensively. The cost is, at present, from 10 to 20 per cent higher than ordinary flint glass. The ware seems to be stronger than glass.—*Proc. Am. Pharm. Association.*

§ American Journal of Science and Arts, vol. xxxvii, p. 350; also Zeitschrift für Analytysche Chemie, Bd. iii, p. 392.

* They imported last year (1867) eight thousand tons.

‡ The English often contains as high as 3·27 of acid.

* Journ. Chem. Soc., vol. xii. p. 6.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, April 2, 1868.

Dr. Warren de la Rue, F.R.S., &c., President, in the Chair.

The minutes of the last ordinary meeting were read and confirmed, and the library donations were acknowledged. The following gentlemen were ballotted for and duly elected Fellows of the Society, viz.: John Tyndall, LL.D., F.R.S., Lecturer on Natural Philosophy in the Royal Institution of Great Britain; Frederic Guthrie, Ph.D., F.R.S.E, Lecturer on Chemistry in the Royal College of Mauritius; William Brantingham Giles, Old Swan, Liverpool. For the first time was read the name of Mr. Thomas Bournes, Teacher of Chemistry, 47, Rigby Street, St. Helen's Lancashire; and for the second time, Francis C. H. Clarke, Lieutenant Royal Artillery, Staff College, Farnborough Station.

Mr. W. H. Perkin read a paper "*On the Constitution of Glyoxylic Acid*," of which Mr. Duppa and himself were joint authors. The starting-point in the formation of this body was dibromacetic acid, and this converted into the silver salt and heated under water, furnished a product described in the original research (1859) as "a new acid having the formula $C_2H_4O_4$."* This body has since been regarded as glyoxylic acid. The authors now resume the description of this acid, and quote analysis in proof of the correctness of its formula. They have further ascertained by comparative examination of the calcium and silver salts that this acid

is in all respects identical with the glyoxylic acid obtained by Dr. Debus as a product of the oxidation of alcohol. The method followed in the purification of the dibromacetic acid is fully described in the paper, and consists in the etherification of the crude acid, conversion into amide, and repeated crystallisation of the latter, when all the mono-bromacetamide is left in solution. The purified amide is then decomposed by hydrate of potassium, added by small portions at a time, and in a vessel surrounded by ice-water. The ammonia liberated is neutralised by dilute nitric acid, and the solution mixed with nitrate of silver, when the dibromacetate of that metal is precipitated. This is said to be not affected by light, although Dr. Debus asserts the contrary. The silver salt is diffused through a considerable quantity of water, and exposed to the temperature of 100° C. until no more yellow bromide of silver is formed, which filtered off leaves bromoglycolic acid in solution. This, in turn, is again converted into its silver salt, and decomposed in a similar manner, yielding bromide of the metal and pure glyoxylic acid in solution. The authors have discovered a very characteristic test for this acid dependent upon the ease and rapidity with which the aniline salt is decomposed. This combination, at first colourless, lets fall a bright orange-coloured precipitate, either on standing at rest for some time, or immediately upon heating. It is impossible within the limited space at our disposal to do justice to the authors' arguments in support of their views respecting the constitution of glyoxylic acid, which occupy more than half the length of their paper.

Dr. Odling read a paper "*On a Glyoxalic Amide.*" In the course of an examination into the properties of the remarkable compound $NOCl_2$, obtained by Gay-Lussac as the chief product of the reaction of nitric and hydrochloric acids, he had made preliminary experiments on the action of this substance upon a variety of compounds, with the view of introducing the group NO in exchange for hydrogen; or of otherwise obtaining some evidence from the reactions of the substance in favour or disfavour of Gay-Lussac's formula, which he did not consider as being at present satisfactorily established. From the result of a preliminary experiment, he had been induced to study the reaction of Gay-Lussac's body with alcohol more in detail. His results were at present in an incomplete state, and he would not have ventured to bring them before the Society just yet, but for the bearing they had upon the experiments of Messrs. Perkin and Duppa, with whom he had been in communication.

Absolute alcohol absorbed Gay-Lussac's body abundantly in the cold, apparently without decomposition; but after a time the temperature rose rapidly, and an unmanageably violent reaction set in. At the temperature of 40° or 50°, however, there was no mere absorption of of the chloro-nitric vapour, but it acted continuously upon the alcohol, with copious evolution of hydrochloric acid. The product of the reaction was heated on a water-bath, whereby the excess of alcohol, containing apparently some chloral, was distilled off, and a syrupy liquid was left, which was further heated for some time on a water-bath, while being treated with a current of dry carbonic acid gas.

On moderate dilution, this syrup yielded a watery solution and an oily deposit. The latter was first examined, but he would now refer, first, to the solution. Being extremely acid, it was neutralised with chalk, and the solution of the resulting lime salts evaporated, whereby what appeared to be a magnificent crop of crystals was obtained, but the apparent crystals were in reality masses of crystalliform jelly. The jelly was dissolved in water, the solution precipitated by alcohol, the precipitate redissolved in water, and reprecipitated by alcohol once or twice until obtained free from chlorine. From the aqueous solution of the final precipitate, crystals were obtained, which an ultimate analysis and examination of their properties showed to be glyoxalate of calcium. In particular, they gave the interesting aniline reaction which Messrs. Perkin and Duppa had just described. Dr. Odling was of opinion that the treatment of alcohol with Gay-Lussac's body constituted the most productive process yet described for the preparation of glyoxalic acid. He had not ascertained whether the jelly he had referred to was or was not a definite compound of glyoxalate and chloride of calcium. The results of his analysis of the pure glyoxalate corresponded with those of Dr. Debus and of Messrs. Perkin and Duppa, and accordingly the salt might be represented by either of the formulæ

$$\frac{Ca}{2}HCO_3.H_2O \quad \text{or} \quad \frac{Ca}{2}H_3CO_4.$$

The oil having been washed with dilute potash and water, was dried over chloride of calcium, and distilled. A portion boiled between 100° and 160°, without giving an indication of any fixed point; by far the larger portion boiled between 180° and 200°; and another portion boiled between 240° and 250°. The portion boiling between 180° and 200° had alone been submitted to examination. Its rectification was very troublesome, owing to the principal constituent being mixed with one or more substances decomposible by distillation. At length a liquid was obtained, boiling at 189°, which was thought to be pure. Analysis, however, proved that it was very far from pure. Its behaviour with potash having shown it to be an ether, it was accordingly treated with ammonia, in the first instance with alcoholic ammonia in sealed tubes, afterwards with strong aqueous ammonia and sufficient alcohol to cause the two liquids to dissolve in or mix with each other. On spontaneous evaporation, a beautiful crystalline substance was obtained, having much the appearance of chlorate of potassium or nitrate of silver, the perfect crystals occurring as rectangular plates. It melted at about 77°, and was capable of being boiled and distilled; but on attempting to take its vapour density it underwent decomposition. It was very soluble in water and alcohol, forming neutral solutions, which did not evolve ammonia when treated with potash in the cold. The numbers obtained by its analysis were in accordance with the formula $C_6H_{13}NO_3$. From a consideration of its properties and mode of formation, it might be regarded as an amide of Messrs. Perkin and Duppa's acid, in which the two alcoholic hydrogens were replaced by ethyl, thus:—

$H_4C_2O_4$	Glyoxalic acid.
$H_5C_2O_3N$	Its unknown amide.
$Et_2H_3C_2O_3N$	The diethyl-amide.
$Et_2H_2C_2O_4$	The unknown corresponding acid.
$Et_3H\ C_2O_4$	Its ether.

The further examination of the ether from which the amide had been prepared was not complete, but enough had been done to show that in all probability it was the compound $Et_3HC_2O_4$.

It would thus be seen that Dr. Odling's experimental results were in perfect harmony with those of Messrs. Perkin and Duppa. The interpretation of both sets of results, however, he considered still an open question—and a most important one—which further experiment alone could positively solve. But with the imperfect materials at present available, he would state what he considered to be the arguments for and against each view of the constitution of glyoxalic acid. Starting from aldehyd and alcohol, he believed that all chemists entertained the same notion as to the constitution of those compounds, although some chemists expressed their notion by means of reasonable, and others by means of unreasonable formulæ (Laughter). Aldehyd contained two marsh-gas residues, one of which was in its primitive state, while the other had undergone the aldehydic modification, or had lost two atoms of hydrogen in exchange for one atom of oxygen, thus:—

$$HOC.CH_3.$$

Similarly, alcohol was composed of two marsh-gas residues; one of which was in its primitive state, while the other had undergone the alcoholic modification, or had lost a single atom of hydrogen in exchange for an atom of oxygen, the excessive equivalency of which was supplemented by addition of an atom of hydrogen, thus :—

$$(HO)H_2\underset{\frown}{C}.CH_3.$$

According to Debus, glyoxalic acid was composed of two marsh-gas residues, one of which had undergone the acid modification, or had lost three atoms of hydrogen in exchange for two of oxygen, the excessive equivalency of which was counterbalanced by addition of an atom of hydrogen, while the other marsh-gas residue had undergone the above-described aldehydic modification, thus :—

$$HO\underset{\frown}{C}.CO_2H.$$

But according to Perkin and Duppa, glyoxalic acid was composed of two marsh-gas residues, one of which had undergone the acid modification, while the other had undergone a sort of glycol modification, or had lost two atoms of hydrogen in exchange for two atoms of oxygen, the excessive equivalency of each of which was supplemented by an added atom of hydrogen, thus :—

$$(HO)_2H\underset{\frown}{C}.CO_2H.$$

In favour of Perkin and Duppa's view might be urged, 1st, the complete accordance of their formula with the ascertained composition of glyoxalate of calcium, of diethylated glyoxalic amide and ether, and of most glyoxalates; 2nd, its accordance with the ingenious transformations and re-transformations which Messrs. Perkin and Duppa had just described. Against it might be urged, 1st, its *à priori* improbability, on the ground that two atoms of hydrogen cannot be replaced by hydroxyl in marsh-gas itself, and are not known to be so replaced in any constituent marsh-gas residue, although subjected to processes similar to those by which glyoxalic acid is produced; 2nd, its want of accordance with the remarkable aldehydic characters of glyoxalic acid; 3rd, its want of accordance with the composition of crystalline glyoxalate of ammonia.

In favour of Dr. Debus's view might he urged, 1st, the necessary existence of the body represented by his formula, and its necessary possession of the joint aldehydic and acid characters exhibited by glyoxalic acid :—

$HO\underset{\frown}{C}.COH$	.. ..	Glyoxal
$HO\underset{\frown}{C}.CO_2H$	.. ..	Glyoxalic acid.
$HO_2\underset{\frown}{C}.\underset{\frown}{C}O_2H$	.. ..	Oxalic acid.

2nd. Its complete accordance with the aldehydic characters of glyoxalic acid, as shown by its power of reducing oxide of silver, of combining with the acid sulphites, and of breaking up under the influence of alkalies into alcohol and acid, just as does aldehyd itself:

$$2HO\underset{\frown}{C}.CH_3 + H_2O = (HO)H_2\underset{\frown}{C}.CH_3 + HO_2\underset{\frown}{C}.CH_3$$

$$2HO\underset{\frown}{C}.\underset{\frown}{C}O_2H + H_2O = (HO)H_2\underset{\frown}{C}.\underset{\frown}{C}O_2H + HO_2\underset{\frown}{C}.\underset{\frown}{C}O_2H.$$

3rd. Its accordance with the composition of glyoxalate of ammonia, a salt made by decomposing glyoxalate of calcium with oxalate of ammonia. Against Dr. Debus's view might be urged, 1st, its necessitating the representation of glyoxalate of calcium as containing an atom of water not removable at 160°, a very suspicious circumstance, and the representation of the speaker's amide as containing an atom of ether; 2nd, its less direct accordance, but by no means positive discordance with the metamorphoses described by Messrs. Perkin and Duppa.

It must be remembered, however, that the aldehydic marsh-gas residue in aldehyd itself, and in benzoic aldehyd, &c., has the property of uniting with chloride of ethyl and chloride of acetyl, with oxide of ethyl and oxide of acetyl, and that in aldehyd with ammonia also. Hence it is not altogether surprising that the aldehydic residue of Debus's glyoxalic acid, combined as it is with a saline instead of a hydrocarbon residue, should have the property of uniting with an atom of oxide of hydrogen. Viewed in this way, the speaker's ether would be a sort of acetal, and be formed under the same circumstances as acetal, namely, by the oxidation of alcohol.

$$Et_2O.HO\underset{\frown}{C}.CO_2Et.$$

$$Et_2O.HO\underset{\frown}{C}.CH_3.$$

Assuming that the ether of acetal has reacted with the aldehydic marsh-gas residue to form an unstable di-ethylated glycol residue $(EtO)_2HC.$, of course the question at issue between Dr. Debus and Messrs Perkin and Duppa would become, in great measure, a verbal one.

Dr. A. W. Williamson, who at this period of the evening occupied the chair, referred to the anomalous constitution of the glyoxylate of ammonia, which did not appear to contain the additional atom of water; but the view advanced by Messrs. Perkin and Duppa received support from the fact that the silver-salt, like the acid itself, contained four atoms of oxygen.

The Chairman moved a vote of thanks both to Dr. Odling and the gentlemen already named, for the interesting theoretical considerations elicited in the previous discussion.

Mr. W. Chandler Roberts read a note "*On the Occurrence of Organic Appearances in Colloid Silica obtained by Dialysis.*" The interesting observations which formed the subject of this paper were elucidated by a series of specimens, both of artificial and natural origin, the structures of whlch were demonstrated by the aid of a microscope and illustrative drawings. In experimenting upon somewhat large quantities of soluble silicic acid prepared in Graham's dialyser, a portion of the liquid product was evaporated slowly in air to compare with the forms of hydrous silica left by a more rapid operation conducted *in vacuo*. All the specimens of jelly dried in air exhibited dendritic forms, varying in size from 0·2 to 0·5 m.m.; these were at first supposed to afford indications of the passage of colloid into crystalloid silica, but when magnified 90 linear they appeared as radiating fibres, and upon being further magnified 700 times each fibre resolved itself into a collection of elongated beaded cells with clusters of circular cells at intervals. Such a structure would indicate a vegetable growth, and the author concludes that the markings, which are similar to those seen in moss agates and Mocha stones, are due to the growth of fungi or mildew in the partially solidified jelly. The spores of organic life were probably derived from the air, since no evidence of similar structure was visible in the specimens of hydrous silica obtained in the desiccator. These last-named products were very like the opal from Zimapan, but contained 21·4 per cent of water.

A short note "*On the Solubility of Xanthin (uric oxide) in dilute Hydrochloric Acid*," by Dr. H. Bence Jones, was next read. Xanthin is usually stated to be insoluble in hydrochloric acid, but the author finds it to be soluble, and had no difficulty in obtaining "six-sided crystals" upon evaporation of the acid. By microscopic examination alone xanthin would be mistaken for uric acid.

In contination of his recent "*Researches on New and Rare Cornish Minerals*," Professor A. H. Church describes the mineral *Cornwallite*, and gives several analyses by which it is shown to consist of arseniate and hydrate of copper with a small proportion of phosphate. Neglecting the latter its formula may be written thus :—

$$Cu_3 2AsO_4, 2CuH_2O_2, \text{aq.}$$

Previous analyses make this mineral appear to have in all *five* atoms of combined water, but the author believes that the error in excess is accounted for by want of care

in drying the samples previously to their chemical examination.

The formula of Cornwallite, thus amended, makes it stand in nearly the same relation to Erinite amongst the arseniates, as Ehlite stands to Dihydrite amongst the phosphates. This will appear by the following comparison :—

Cornwallite ..	$Cu_3 2AsO_4$	$2CuH_2O_2$ aq.
Erinite	$Cu_3 2AsO_4$	$2CuH_2O_2$
Ehlite	$Cu_3 2PO_4$	$2CuH_2O_2$ aq.
Dihydrite	$Cu_3 2PO_4$	CuH_2O_2

A vote of thanks having been passed to the authors' the meeting was adjourned until Thursday, 16th instant' when the following papers will be read :—"*On Graphic Formulæ*," by Dr. Guthrie ; "*On the Tetra-phosphoric Amides*," by Dr. J. H. Gladstone ; "*A New Reaction for the Formation of Isomeric Cyanides*," by Messrs. E. T. Chapman and Miles H. Smith ; and, if time permits, one or two other papers.

GLASGOW CHEMICAL SOCIETY.

The inaugural meeting of the Glasgow Chemical Society was held on Monday evening last, in the Hall of the Philosophical Society. There was a very large attendance.

The President, Professor Thomas Anderson, M.D., F.R.S.E., occupied the chair.

After the minutes of the former meeting were read and approved of, and nine new associates proposed and admitted into the Society,

Dr. Anderson, in a few introductory remarks, thanked the members for having appointed him to the office of President of a Society of whose future success he felt great confidence. He felt assured that the new Society would have a long career of usefulness, and that its members were very fortunate in having, as the first communication to the Society, the paper to be submitted to their notice that evening, by M. Ludwig Mond, on his remarkable process for the recovery of sulphur from the black-ash waste of the alkali works. The President then called upon

M. Mond, who is at present practically putting his process in operation in the alkali department of Messrs. Charles Tennant and Company's Chemical Works, St. Rollox. The paper of M. Mond gave, in clear and intelligible English, an elaborate account of his recovery process from the commencement of his labours. It also referred to the other processes which have been brought under the notice of alkali manufacturers from time to time, to effect the same object, and showed wherein they had failed to meet with the success which had in such a marked degree attended the application of his process. In the outset M. Mond referred to the vast importance of the alkali trade, and characterised the St. Rollox Alkali Works as the most important and interesting of their kind in the world, not only on account of their vastness, but because a very considerable number of the most valuable improvements in the manufacture of alkali and its cognate industries have originated or been first adopted in them. The manufacture of bleaching powder, which has become so extensive that it can hardly be now called a secondary product, was invented by the founder of the firm, Mr. Charles Tennant, and is still carried out in the St. Rollox Works on a larger scale than in any other similar establishment in the world. Among the many other improvements which have first been applied in Messrs. Tennant's Works, M. Mond instanced the now famous apparatus for the lixiviation of black-ash, on which the final success of his process altogether depends, and regarded it as remarkable that the first apparatus ever put up for this purpose is at present employed for his sulphur-recovery process. He then mentioned some rather astonishing details illustrating the great development of the alkali trade in Great Britain within the last four years. In the year 1864 the quantity of common salt decomposed in this country was about 288,000 tons, and it rose to about 400,000 tons in the year 1867, or about 40 per cent. This quantity of salt requires about 320,000 tons of oil of vitriol for its decomposition, and this amount contains nearly 100,000 tons of sulphur. At present the sulphur is nearly all obtained from iron and copper pyrites, which are supplied at a much cheaper rate than brimstone, owing to the successful working of one of the largest mines in Spain, by the Tharsis Mining Company, which has been principally formed amongst Glasgow gentlemen, and especially by the intelligence and perseverance of Mr. William Henderson, who has brought his process of copper extraction from the residual burned ore to an unprecedented pitch of perfection. Notwithstanding the extensive use of pyrites in the vitriol manufacture, Sicily still enjoys a sulphur monopoly, and exports annually a very large quantity of that substance—a quantity which last year amounted to upwards of 200,000 tons, of which about 50,000 tons was consumed in Great Britain. M. Mond considered that by his recovery-process British alkali manufacturers might make themselves independent of Sicily as the source of their sulphur supply, inasmuch as they have a material which has hitherto been a source of inconvenience and outlay to those manufacturers in whose operations it is unavoidably produced, and from which the sulphur can be obtained at a much cheaper rate than that at which it can be imported. He then, in a clear and intelligent manner, described the apparatus in which the process is conducted, the *modus operandi* of the process, and the chemical changes which are involved in it. He also practically illustrated the process in the presence of the members, and produced a very decided quantity of sulphur. A variety of specimens, illustrating the various stages of the process, were shown to the members. (The details are essentially the same as those contained in the paper by M. Mond, which appeared in the Chemical News for 19th and 26th of July of last year. To this paper our readers are referred.) M. Mond regretted very much that the chemistry of the polythionic acids so intimately connected with his process, had hitherto received so little attention from chemists.

At the conclusion of the paper the President congratulated the author on the great success and simplicity of the process which he had given to the alkali manufacturers, and on the interesting manner in which he had brought it under the notice of the Society, and then at some length gave an account of a sulphur-recovery process which he had seen in operation at Dieuze.

Mr. E. C. C. Stanford and one or two other gentlemen spoke, but as the time was far advanced there was very little opportunity for discussing M. Mond's interesting and valuable paper. The author was awarded a hearty vote of thanks.

ROYAL INSTITUTION OF GREAT BRITAIN.

Weekly Evening Meeting, Friday, March 20, 1868.

His Royal Highness the Prince of Wales, K.G., in the Chair.

"*On Alloys and their Uses*," by Professor Augustus Matthiessen, F.R.S.

The object of this discourse was to show experimentally why alloys are used in preference to their component metals.

Alloys may be, chemically considered, divided into three classes :—

1. Chemical combinations.

2. Mechanical mixtures.

3. Solutions of the one metal in the other which have become solid ; or, for shortness sake, solidified solutions of the one metal in the other.

Under the term chemical combination such alloys may be considered which are the result of the combination of two metals when these unite together with great energy and evolution of heat, producing an alloy the physical and chemical properties of which we cannot foresee. As an example of such alloys those of gold, with tin, lead, or zinc may be quoted ; for if to melted tin, lead, or zinc, gold be added, the two metals unite together with great energy and produce an alloy which is exceedingly brittle and totally unfit for practical purposes.

It is for this reason that the more expensive metals, silver and copper, are used for alloying gold for the purposes of coinage, &c.

With regard to such alloys which may be looked upon as mechanical mixtures, like oil and water, or rather as ether and water, for no two metals are known which, like oil and water, do not dissolve at all in one another, but a few metals are known which, like ether and water, dissolve slightly in one another, for ether will dissolve a certain amount of water, and water a certain amount of ether. If ether and water be mixed together, say in equal parts, two layers will be formed, the top one being ether containing a little water, the lower one water containing a little ether. Two metals, for instance, which behave in exactly a similar manner to ether and water are lead and zinc, for lead when fused with zinc will dissolve 1·6 per cent zinc, and zinc in its turn will take up 1·2 per cent lead.

If these two metals be fused together, say in equal parts, they will separate into two layers, like ether and water, the top one, being the specifically lighter, zinc, with a small percentage of lead, the lower one lead, with a small percentage of zinc. If such an alloy be made and cast in a mould, the difference in the behaviour of the two ends may be easily shown ; for the top one is so brittle that it cannot be bent without breaking, whereas the lower one may be bent with ease.

Such chemical combinations and mechanical mixtures are, however, comparatively rare ; and for alloys in common use, practice has almost invariably chosen such alloys as may be considered as belonging to the third class, rejecting those of the first and second as worthless for practical purposes.

Under the term solidified solutions of the one metal in the other, such alloys may be considered, which, like the chlorides of potassium and sodium when fused together, produce a mass having some of the physical properties totally different from those of the component salts. It cannot be assumed that the chloride of sodium enters into chemical combination with the chloride of potassium. One important property of a solidified solution is, that the components are homogeneously diffused in one another, so that even under the most powerful microscope they can no longer be distinguished from one another.

Alloys are used because they possess certain physical properties to a far greater extent than their component metals. The physical properties may be divided into two classes.

1. Those which in all cases are imparted to the alloy, approximately in the ratio in which they are possessed by the component metals.

2. Those which in some cases are, and in others are not, imparted to the alloy in the ratio in which they are possessed by the component metals.

To the first belong Specific Gravity, Specific Heat, and Expansion due to heat. It is easy to show this experimentally; the specific gravity of an alloy may be shown o be equal to the means of those of its component metals, by hanging on the one side of a balance the alloy and on the other side the metals composing it unalloyed, and then placing them both in water.

The specific heat of an alloy may be proved equal to that of its components by placing the alloy and its components in boiling water, and then in equal volumes of cold water ; when the rise of temperature in the two cases will be found the same, as may be shown by a differential-air thermometer.

A brass bar placed in any apparatus for showing expansion by heat is seen to expand exactly as much as a composite bar, of which one portion is of copper, the other of zinc. The length of the zinc portion being proportional to the amount of zinc in brass.

To the second class of physical properties belong, Conduction for Heat and Electricity, Hardness, Tenacity, &c.

As a basis for the conclusion which will be drawn, the electric conducting power for alloys may be taken. Researches into this subject have shown that when tin, lead, zinc, or cadmium are alloyed together, such alloys conduct electricity in the ratio of the relative volumes of the component metals, whilst in all other cases no such simple relation exists between the conducting power of the metals and their alloys. If, for instance, gold be alloyed with silver, say in equal volumes, the conducting power of an alloy will be 15, that of silver being 100, and that of gold 80.

If curves be drawn to represent the conducting power of different series of alloys, three typical forms will be observed : the first represented by nearly a straight line, the second by the letter *L*, and the third by the letter U.

Wiedemann and Franz have proved experimentally that the values obtained for the conducting power of metals and alloys, for heat and electricity, are identically the same : and the truth of this statement may be shown by the following experiment :—If bars of gold and silver and some gold-silver alloys be fixed so that one end of all of them is in a hot-water box and the other end in the bulb of a small air-thermometer, the depression in the columns of the liquid in the tubes of the air-thermometers will indicate the relative conducting powers (approximately) of the several bars ; and if through the tops of the columns of liquid a line be drawn, such line will form a curve similar to that referred to as obtained for the electric conducting power.

That this is true is thus shown :—

By the side of this apparatus is placed another of this construction :—Into the bulbs of several air-thermometers are fixed wires of the same size and length, and of the same materials as were used in the heat-conducting experiment. One end of each wire is soldered to one thick copper wire, and the other end to another similar wire. These two wires are connected to the poles of a battery. The current will then divide itself, and a portion will pass through every wire proportional to the conducting power of that wire. This current will heat the wire and cause the liquid in the tubes connected with the air-thermometers to descend, and the line drawn through the top of the columns will be nearly similar to the curve already mentioned, which is formed by the bulbs in which the heat-conducting bars are fixed.

The analogy between the relation existing in this case and in some others may be shown experimentally as follows :—

Sonority. When bars of alloys and their component metals are struck, a great difference will be found in the note produced ; and in almost every case where the experiment has been made, the most sonorous alloy was found to correspond in composition approximately with that at the turning point of the electric conducting power curve.

Tenacity. When wires of the same diameter of metals and alloys are broken by traction, those of the alloys will require a much greater force than their component metals ;

and it may be deduced from what is known, that those alloys, the composition of which corresponds to the turning point of the conducting power curve, are more tenacious than any other alloy composed of the same metals.

Elasticity. When spirals of wires of metals and their alloys are weighted to an equal extent, the alloys will be found on removing the weights to possess the property of resuming their original form in a much higher degree than their component metals. Here again the alloys corresponding in composition to those of the turning point of the conducting power curves are the most elastic.

From what has been said, and from the experiments described, the conclusion may be drawn that the chemical composition of the practically-used two-metal alloys correspond to those situated at the turning points of the heat and electric conducting power curves, and that if a two-metal alloy of a special physical property be required, it would be as well to try that alloy, the composition of which would correspond to the turning point of the curve representing the electric conducting power of the alloys of the two metals.

FOREIGN SCIENCE.

PARIS, APRIL 6, 1868.

Volumetric method of estimating carbonic acid in natural waters.—Aniline marking ink.—Glaze for crystallising pans.—Preservation of saccharine juice. ACADEMY OF SCIENCES :—Skeletons of cellulose.—New mode of forming organic sulphacids.—Transformation of uric acid into glycocol.—Oxychloride of silicium.

M. Barthélemy, Professor of Physics at the Lycée de Pau, has published a volumetric method of estimating carbonic acid in natural waters. His process depends upon the reaction of the protonitrate of mercury upon the alkaline and earthy carbonates; by means of the same reagent he is enabled to estimate small quantities of acid —for example, the nitric acid present in rain water after a storm. The crystals of neutral protonitrate of mercury are soluble in water, which at the same time decomposes the compound into insoluble sub-nitrate, and acid nitrate which remains in solution. The supernatant liquid, in the presence of mercury, may be kept a long time without undergoing any decomposition. The reagent is prepared by treating mercury with cold dilute nitric acid. Upon adding to a dilute solution of an alkaline or earthy bicarbonate, protonitrate of mercury, a precipitate forms, which is at first white, afterwards orange, and often greenish : this precipitate is soluble in excess of the reagent ; also in sulphuric and nitric acids, in urine, and other organic matters.

In a solution of neutral carbonate the same reagent produces a brown precipitate which, when the alkaline carbonate is mixed with bicarbonate, takes a more or less deep green tint ; this brown precipitate is insoluble in excess. By passing carbonic acid into the solution, (it is sufficient to breathe through a tube), the reaction indicated for the bicarbonate is produced. The amount of acid nitrate which it is necessary to add for complete precipitation and resolution, is proportional (1) to the quantity of carbonate, (2) to the degree of concentration of the reagent, (3) to the quantity of carbonic acid engaged in the solution. M. Barthélemy prepares his normal solution by dissolving ·5 grm. of bicarbonate of potash (equal to ·241 of carbonic acid), previously heated in a current of dry carbonic acid, in a litre of distilled water. It is necessary before pouring out the solution of nitrate of mercury from the burette to agitate well, since the mercurial solution is very dense. When waters contain chlorine, the determination of the carbonic acid cannot be made exactly. Approximate results may, however, be obtained by acidulating 100 c.c. of the water with nitric acid, decomposing thus the carbonates, and then noticing the number of divisions required to precipitate the chlorides and produce a definite grey tint ; afterwards the same given volume of water is treated with the solution of nitrate of mercury, until the yellowish orange first produced has disappeared, and the tint of the chloride alone remains. The first experiment serves as a standard of colour, and the addition of fluid from the burette is arrested, when the tints appear identical.

The process also admits of the separate determination of earthy and alkaline carbonates. By boiling 100 c.c. of water, maintaining the volume by adding distilled water, leaving to deposit, filtering, and passing a current of carbonic acid, matters are arranged for the volumetric determination of the carbonic acid combined with the alkalies ; knowing already the amount combined with alkalies and alkaline earths conjointly, there is no difficulty in finding that due to carbonic acid combined with alkaline earths only. Here is another method, and one to which M. Barthélemy gives the preference :—A solution of potash (containing ·5 grm. of potash in a litre of water) is added in definite volume to 100° c.c. of the water ; the simple carbonates are deposited on the sides of the vessel. At the end of a few days the solution is decanted and saturated with carbonic acid. The carbonic acid in solution is then determined, and that in the same volume of potash solution saturated with carbonic acid also ; the difference between the amounts of solution poured from the burette, in the first experiment and the second, is the amount required by the alkaline carbonates in 100 c.c. of the water.

An indelible marking ink is prepared from aniline by mixing the two following solutions : *a*, cupreous solution—8·52 grm. of crystallised chloride of copper, 10·65 grm. chlorate of soda, and 5·35 grm. of chloride of ammonium are dissolved in 60 grm. of distilled water ; *b*, aniline solution—20 grm. of hydrochlorate of aniline are dissolved in 30 grm. of distilled water, and 20 grm. of a solution of gum arabic (1 of gum to 2 of water) with 10 grm. of glycerine are added. By mixing in the cold four parts of the aniline solution with one part of the cupreous solution, a green liquid is obtained which can be used immediately for tracing characters upon linen ; the marks, however, alter after the lapse of a few days. It is necessary to keep the solutions separate until required for use. If the fluid does not flow easily from the pen, it may be diluted without fear of diminishing the intensity of the tint, which at first green, gradually darkens and becomes black. Heat causes the change to take place instantaneously ; a steam heat is sufficient, and is better for the fabric than a hot iron. Afterwards the linen is washed in warm soap and water. This ink resists acids and alkalies, and is remarkably permanent.

Some remarks upon the glaze of vessels used for crystallising in chemical works, have been published by M. Stinde. By his experience, the majority of glazes proposed for iron vessels do not fulfil their purpose well, either they become detached, or traversed by rust when the vessel remains empty for a few days. The mixture of oxide of zinc and soluble glass adheres to the iron well enough, but it does not prevent rust. Of all materials proposed, that of a mixture of oil and minium of iron (peroxide of iron mixed with alumina) is unquestionably entitled to preference.

After having thoroughly pulverised the minium, it is mixed with linseed oil, rendered pasty with manganese. This mixture is applied to the iron surfaces carefully cleaned, and deprived of rust by means of pumice-stone.

MM. Périer, Possoz, Cail & Co. have patented a process for the preservation of saccharine juices. Lime, it would appear, has been long known as a preservative substance ; in applying it directly to the saccharine juices of plants, saccharate of lime is formed, and this can be preserved unaltered for a great length of time. Upon decomposing the compound, however, for the recovery of the sugar, it is found that the foreign matters existing in the juice have

undergone such changes as to impede the extraction and crystallisation of the sugar. The patentees of the present process propose therefore to apply the lime no longer to the juice of the plant, but to the juice after removal of the foreign matters. They consider also that the lime ought to be employed in larger proportion than it has been heretofore. While M. Kuhlmann, in 1833, indicated as sufficient for the raw juice, ·3 to ·5 per cent, they consider 1 per cent necessary for the purified juice, and when required to be preserved during some months, 2 per cent, the density of the juice being ·1040.

The memoirs relating to chemistry, brought before the Academy of Sciences at the meeting on the 16th of March, were the following :—"On a tissue or skeleton of cellulose, directly extracted from an epidermis," by M. Payen. "On a meteorite which fell on the 9th June, 1867, in Algeria," by M. Daubrée. "On a new mode of formation of organic sulphacids, and on the transformation of uric acid into glycocol," by M. Strecker. "On an oxychloride of silicium." "The reduction of nitrates and sulphates in certain fermentations," by M. Béchamp. "On the cultivation of beet-root for sugar," by M. Mehais. M. Maumené addressed some observations on the subject of potash extracted from suint.

M. Payen referred, in commencing, to the numerous examples of vegetable substances he had made known, in former researches, where the skeletons of cellulose at first are easily obtained, and where the cellulosic substance is possessed of the properties as well the composition of cellulose, and yet afterwards, during growth, foreign matters mask these properties. When nitrogenous matters fatty and saline, have thoroughly penetrated the cells, the difficulty experienced in separating them is so great that some have considered the mixture of substances free from cellulose, and have in fact believed in the presence of an entirely new proximate principle. M. Payen in very cold weather snbmitted several tubercles of potato to refrigeration. After thawing, the epidermis was easily removed. By careful treatment with various acids and potash solution, during many days, as well as by washing with water, alcohol, and ether, the membranous substance was obtained in a supple condition and white: specimens were exhibited to the Academy. The substance thus purified gave the reaction—the blue tint with a very weak solution of iodine when acidified with sulphuric acid—due to cellulose.

M. Strecker's method of forming organic sulphacids consists in reacting upon the chlorides of the radicals with sulphites. Several compounds of the sulphacids have been obtained in M. Strecker's laboratory. Iodide of methyl heated to 150° with a solution of sulphite of soda, yielded methylsulphite of sodium (methyl-dithionate), according to the equation

$$CH_3I + Na_2SO_3 = CH_3SO_3Na + NaI.$$

Bromide of ethylene and sulphite of potash gave disulphethylenate of soda and bromide of potassium. A new acid, which may be called trisulphoglycerilic acid, is produced when trichlorhydrine is reacted upon by sulphite of potash

$$C_3H_5Cl_3 + 3K_2SO_3 = C_3H_5(SO_3K)3 + 3KCl.$$

The chlorinated acids comport themselves in an analogous manner; monochloracetic acid is transformed by ebullition with a solution of an alkaline sulphite into alkaline sulphacetate. The chlorhydrate of oxide of ethylen furnishes, under the same conditions, isethionic acid. M. Strecker states that all the chlorine, bromine, and iodine, directly united to the carbon, is usually replaced by an equivalent quantity of the radical (SO_3K). At the same time it often happens that only a portion is replaced, while the rest remains unattacked. In heating chloroform with a solution of sulphite of potash, the potash salt of sulphodichloromethylic acid was obtained, according to the equation, $CHCl_3 + K_2SO_3 = CHCl_2SO_3K + KCl$. M. Strecker remarked that his experiments showed that the sulphacids contained the residue SO_3H united directly to the carbon by the sulphur; he thought it probable that the isomeric ethyl-sulphurous acids contained likewise this group, but united to the carbon by the interposition of oxygen.

M. Wurtz presented the foregoing, as well as another note by M. Strecker, "on the transformation of uric acid into glycol." When uric acid is heated with a concentrated solution of hydrochloric acid, or hydriodic acid, preferably the latter, in a sealed tube, to a temperature of 160-170°, it is completely transformed into glycol, carbonic acid, and ammonia. Upon opening the cold tube, a continuous current of carbonic acid is seen to be disengaged. The solution, treated with hydrated oxide of lead, evolves abundance of ammonia, and after removal of the lead by sulphuretted hydrogen yields, upon evaporation, a crystalline residue of glycol. Analysis showed the substance to be identical with that obtained from hippuric acid; the crystalline form and the chemical properties were also in perfect accordance. If, then, hippuric acid be considered as a glycol joined to benzoic acid, uric acid may, in the same way, be figured as a combination of glycol with cyanuric acid; these two acids, characteristic of the urinary secretions of herbivorous and carnivorous animals, are now seen to present more resemblance than could have been supposed.

MM. Friedel and Ladenburg have observed that in passing chloride of silicum through an empty porcelaln tube, or one filled with fragments of felspar, heated to a temperature approaching the point of fusion for this mineral, and distilling, the product condensed at the extremity of the apparatus, is a liquid less volatile than the chloride. By repeating the operation a great number of times with the more volatile portions, a notable amount of a liquid boiling above 70° is obtained. This product submitted to fractional distillation is easily separated into chloride of silicium and a liquid chiefly boiling between 136° and 139°. Limpid and fuming in the air, this liquid bears great resemblance to chloride of silicium; it is likewise decomposed by water energetically. Analyses were made by introducing weighed bulbs, full of the liquid, into flasks containing a certain quantity of water; breaking the bulbs afterwards, almost the whole of the silica, when sufficient water was present, remained in solution. The acid liquid, saturated with ammonia, was evaporated on the water-bath; the residue dissolved in water and filtered gave on the one side silica mixed with the glass of the bulb, on the other a solution in which the chlorine was determined. The numbers obtained lead to the formula Si_2OCl_6, from which the new body is seen to be an oxychloride of silicium.

NOTICES OF BOOKS.

Chemical Notes for the Lecture-room On Heat. By Thomas Wood, Ph.D., F.C.S. London: Longmans & Co.

It is a pleasure for us to record that this text bock has met with the success that we predicted for it. As a consequence a second edition has been published in a comparatively short space of time, and Dr. Wood has by careful revision considerably improved it. As we could never attach any very clear or definite meaning to the word "*cram*," as applied to tuition, we forbear to discuss in what way such an expression could attach itself to a work like that under notice. It is, however, within the experience of every observer that cramming, in teaching, not unfrequently is used as synonymous with method, conciseness, and want of verbiage, by persons whom we presume have never followed such a highly reprehensible course.

An expression which *is* definite may convey clearly the avowed object of this book, which is one that we may boldly say is a legitimate—even more, a desirable one—

and that is *drill*. Many minds require the same fact to be placed before them over and over again in the same light and in the same words; but this is not equivalent to saying that such a *drilling* is applicable to leading and superior minds in which originality is to be hoped for. Dr. Wood certainly can with justice claim to have carried out thoroughly a plan that he has clearly sketched out for himself, and this we take to be no small merit in a writer of science. The majority of our readers will we think acknowledge the justice and truth of what Dr. Wood asserts in his preface.

"Many years' experience in teaching has convinced me that the average boy can only be provided with a very limited amount of producible information on any subject for an examination. A small book, therefore, with which he may become so familiar as to be able to refer to it with ease and rapidity, and which he can almost get by heart, is the thing required, and the present edition is offered as such."

In the next edition we would suggest that the author should properly punctuate the title of his book. Many who have not had the advantage of Dr. Wood's explanation might be puzzled to know the meaning of a "Lecture-room On Heat."

Scientific Blue Books. No. I. *Abridgments of Specifications of Patents.*

It may be urged with justice, we are afraid, that scientific men in general, with the exception perhaps of authors, are totally ignorant of the fact that there exist scientific blue books, the value of which is very great, as by consulting such we invariably get evidence the accuracy of which no one can fairly dispute. Thousands of valuable scientific blue books are destroyed as waste-paper, owing mainly to the want of appreciation by the scientific public.

The original books of specifications of patents are far too costly and cumbersome for any but a large public library; thus the specification of patents filed during the operation of the Patent Law Amendment Act, from Oct. 1st, 1852, to June 30th, 1866, are comprised in 43,955 blue books, or 1,428 thick volumes imperial octavo, to be obtained at a price of £1,290.

The very first idea that is impressed upon the mind by this really impressive, not to say oppressive, fact, is that some of all this mass of matter must be of value; and if this value exists, the papers would be well worth wading through once and for ever by competent authorities, with the view of sifting the grain from the chaff in the first place, and in the second of converting that grain into material that may be easily digested. The second of these processes is continually being carried on by authors of technological works in every department of science; but the short digest of cumbersome works is not so well adapted to the needs of these, as to the students of such works who may desire to have authentic records of any technological process which may in turn serve as a reference to a still more detailed description if such be necessary. This work is now being conscientiously and thoroughly done in a systematic manner, and we are only discharging an obvious duty in doing the utmost in our power to prevent a premature close to a good project.

The abridgements of specifications of patented inventions carefully classified are miniature blue books, of duodecimo size; each affords at once a chronological, alphabetical, subject-matter, and reference index to each class, the work being done by well qualified compilers; omissions that are wholly unavoidable from the mass of the material will be supplied in second editions. In addition we find an introduction to each volume, which further gives a short digest of the various discoveries made from time to time in each branch. The prices of these works are extremely moderate, and a copy of the more important ones should be possessed by all practical chemists who devote themselves to the various branches of technology. At present twenty-nine classes have been published, and among those of more immediate interest are those of "Preservation of Food," "Manufacture of Iron and Steel," "Bleaching, Dyeing, and Printing," "Electricity and Magnetism; their Generation and Applications," "Production and Applications of Gas," "Metals and Alloys," "Photography," "Plating and Coating of Metals," "Oils, Animal, Vegetable, and Mineral." The following we learn with numerous others are in course of preparation: "Preparation and Combustion of Fuel," "Steam Engines," "Stone, Marble, and Cements," "Acids, Alkalies, Oxides, and Salts."

If due appreciation attend these efforts, doubtless others will appear in course of time. Valuable as these abridgements are to all scientific men, they will prove to be invaluable to those who desire to know what processes really are patented and what are not. We would urge upon the authorities, however, that as these volumes bear evidence of great industry, considerable powers of accuracy, and require the rather rare quality of condensation with judgment, and without important omissions on the part of the compiler, it would only be a matter of justice to give the credit due to the author, in every case, by appending his name as such. The practice of working by deputy is only to be encouraged when the said deputy is exposed to a just criticism of his own share in the work, which criticism should be limited to such personal work. By a subdivision of labour, again, it so frequently happens that any responsibility is easily lost sight of, and inaccuracy is a frequent result.

We are told by Mr. Woodcroft that the most recent chemical names of substances are placed in italics after the names that have been obtained from the respective specifications; "this addition is rendered necessary by the universal adoption of the new chemical nomenclature."

We hope that this addition will not at present be made to the substance of the specifications themselves; even the most ardent radical in chemistry would not care to have a string of synonyms after every mention of such a substance as, say, calomel, which, if burdened with aliases, after the manner of legal definitions, would, we are afraid, present a very criminal appearance indeed.

CORRESPONDENCE.

WATER-TESTS.

To the Editor of the Chemical News.

Sir,—Your correspondent, Mr. J. Muter, gives me the credit of having published, through the medium of the *Times*, an extemporary method of recognising pollution in water by the sense of smell. Mr. Muter has misread the letter to which he alludes. I merely reminded the country householder that he usually contented himself with a glance and a sniff at his water-bottle, and suggested that eyes and nose would be better detectives if he previously well shook the water or even placed it in a warm place for a few hours. Mr. Muter's letter to you is similar to one he sent the previous week to the *Medical Times and Gazette;* the sentences in which I am mentioned are word for word the same. In answer to that letter a kind friend has replied, this week, as follows,—I am, &c.,

John Attfield.

Testing Water for Organic Impurities.

Sir,—In connection with remarks on "country wells," Professor Attfield simply stated, in his letter to the *Times*, that "polluted water does not generally betray its condition till possessed of a strong odour; earlier intimation

may, however, be obtained by the following tests:—Half fill a common water-bottle, cover its mouth with the hand, violently shake for a minute, and quickly apply the nose. If nothing unpleasant is detected, tightly cork the bottle, set it aside in a warm place at about the temperature of one's body for a couple or three days, and repeat the shaking, &c. Water of very bad quality may thus be recognised without the trouble and expense of analysis." Of course householders would get still earlier intimation, or else the comforting assurance that the water contained no organic impurity, by seeking professional assistance; but such a statement by an analyst in a leading newspaper would have been scarcely ethical. I have found Professor Attfield's hints of very great use, and am convinced that few persons besides your correspondent, Mr. J. B. Muter, could possibly have received from them the impression that any water free from odour is fit to drink,—I am, &c.,

SANITAS.

ROYAL SCHOOL OF MINES.

To the Editor of the Chemical News.

SIR.—Will you kindly allow me to draw attention to one question which appears to have escaped notice, viz—What is the meaning of the designation Royal School of Mines?

The term R. Mining School, or School for Miners, I could understand; but this one I cannot. I know what a school of boys is, and I have heard of a school of whales, but what is a School of Mines? It is true that there exists an Ecole des Mines, but I do not know why our corresponding institution should bear a name which is a literal translation of the above title.

Now that the School is just beginning to be known, it seems almost a pity to propose any alteration, still I really believe that it would be preferable to call it the "College of Science," as suggested by "Delta," than to let it still retain its present inappropriate and absurd name, more especially as only a small fraction of the students ever have anything to do with mines or mining in after life.

My apology for again encroaching upon your valuable space must be the deep interest which I take in the Institution and everything connected with it.—I am, &c.

A. L. E.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Isomerism of the Hydrocarbons C_2H_{10} and C_4H_8.—A. Butlerow. There can be only two isomers of the composition C_4H_{10}; the one is ethyle

$$\begin{matrix} CH_2(CH_3) \\ CH_2(CH_3) \end{matrix}$$

the other trimethylformene $C(CH_3)_3H$, obtained by the author from trimethylcarbinol (tertiary pseudo-butylic alcohol) by the action of zinc on trimethylcarbinylic iodide. The reactions of the two compounds prove them to be isomeric, not identical. The action of chlorine, for instance, gives rise to the formation of oily products, of which that derived from trimethylformene is lighter than water, that from ethyle heavier; and on heating those chloro-derivatives with water to 100° C. trimethylcarbinol is formed in the one case, and scarcely any action is observed in the other.

The number of isomeric butylenes according to the author is nine; that derived from trimethylcarbinol by the action upon it (its iodide) of alcoholic potassic hydrate, has the formula,

$$C\begin{matrix} (CH_3)_2 \\ CH_2 \end{matrix}$$

and is converted into pseudopropylcarbinol (primary pseudobutylic alcohol) on oxidation with hypochlorous acid.—(*Ann. Chem. Pharm.* cxliv. 1.)

Synthesis of Alcohols.—E. Linemann. The synthesis of fatty alcohols from the lower members of the series by way of successive conversions of the alcohol (methylic) into cyanide, amide of next higher alcohol, and alcohol, is of little practical value on account of the great loss experienced in the last stage of the process. The conversion of the amide into alcohol by means of an excess of nitrous acid (Hofmann), whereby nitrite of alcohol is produced, is accompanied by a rapid evolution of nitrogen which carries of most off the volatile alcoholic nitrite. The author has discovered a process by which more than one-fourth of the amide is obtained as alcohol. It consists in boiling the nitrite of the amide with slightly acidulated water, whereby its splits up into nitrogen and alcohol, the alcohol being prevented from evaporation by the water present. The conversion of the amide into nitrite is effected by decomposing its chlorhydrate with argentic nitrite. A. Siersch by means of this method converted ethylic alcohol into isopropylic alcohol.—*Ibid.* cxliv. 129. 137.

Stannic Diethyl-dimethyle.—N. Morgunoff. Methyl-caproyl and acetyl-amyle, $C_7H_{14}O$, according to Popoff's experiments are identical, which fact proves the equality of the four carbon affinities. Morgunoff has from the same point of view examined the two stannic diethyl-dimethyle

$$Sn\begin{matrix} (CH_3)_2 \\ (C_2H_5)_2 \end{matrix}$$

as obtained either by acting upon stanndiethylic diiodide with zincic methide or upon stanndiethylic diiodide with zincic ethide, and he has found that both methods lead to the same result, and that therefore the four affinities of the tetratomic tin, like those of carbon, are of equal value.—*Ibid.* cxliv., 157.

MISCELLANEOUS.

Sir David Brewster's Last Words.—Sir J. Simpson says:—"It seems to me that I carry almost a mission from him to us—from the dead to the living; for when I last visited him at Allerly, when he was within a few hours of death, when he was already pulseless, his mind was perfectly entire, and perfectly composed; and on asking him, among other matters, if he wished any particular scientific friend to take charge of his remaining scientific papers and notes, he answered me, 'No; I have done what every scientific man should do—viz., published almost all my observations, of any value, just as they have occurred."

A Model Scientific Writer.—Professor Fraser says with regard to Sir David Brewster's great precision, energy, and determination of thought—that during the seven years that he (Professor Fraser) was editor of the *North British Review*, Sir David Brewster contributed an article to every number; and that he did far more—that he stated the day when his first slip of paper would come, and the day when it would be finished. His manuscripts came as they were written—day after day, and sheet after sheet—and without the necessity of the revisal of those preceding. He thus worked with the precision and regularity of a mechanical rather than a mental machine.—*Scientific Review.*

M. Panizzi and Men of Science.—In a letter to the *Times*, defending a statement made in Parliament that Mr. Panizzi, principal librarian of the British Museum, "had not scrupled to express his contempt for men of science," Mr. W. H. Gregory, M.P., writes:—"Three short passages from Mr. Panizzi's evidence before the select committee on the British Museum in 1836 will prove the correctness of my expressions. In answer 4,929 Mr. Panizzi gives his opinion of scientific men in these words:—'Scientific men are jealous of their authority; they are dogmatical and narrow-minded, and as they think themselves infallible they would never consult an officer. I speak from what I have known of them.' '4,930. The scientific men would spoil the men of rank or drive them away from the Board. I speak seriously, and from experience. An officer would have no chance against a scientific man who should take a crotchet, and they are all crotchety.' '4,933. I never saw scientific men go right or view things as other people do. I think the trustees would be much better without them.'"

Chemical Nomenclature.—M. Dumas, the new secretary of the Académie des Sciences, observes:—"If every one of us took the fancy of combining with his name that of his great-grandfather, of his grandfather, of his father, and his mother, a singular complication would be found in our registers of births. A lifetime would be passed in learning the names of the persons with whom we were acquainted in our own neighbourhood. As to knowing the names of the inhabitants of a town, that would be an utter impossibility. This is, however, what our *savants* who pursue organic chemistry have to accomplish, so that their language has now arrived at a point of barbarism that cannot be surpassed. Now, would it not be desirable, in all points of view, to adopt a generic word, and to group around such word the names of species in proportion as science extends her conquests? I am particularly interested in organic chemistry, but I declare that time is entirely wanting to me to peruse, while comprehending them, the various memoirs on the science which come under my notice. The complication and insupportable length of the names employed are the sole causes of this."—*Medical Times and Gazette*, March 21st.

Rattlesnake Poison.—The following are the conclusions at which Dr. Weir Mitchell has arrived:—1. One-fourth of a drop of the venom is fatal to pigeons under the age of four months. One-eighth of a drop is frequently a fatal dose. 2. The venom is absolutely harmless when swallowed, because (*a*) it is incapable of passing through the mucous surfaces; (*b*) it undergoes change during digestion, which allows it to enter the the blood as a harmless substance, or to escape from the canal in an equally innocent form. 3. Twenty-four hours after it has been swallowed, the contents of the bowel contain no poison. 4. The rectum of the pigeon does not absorb the venom, and it causes no injury when placed on the conjunctiva of animals. 5. The venom passes through the membranes of the brain, and more swiftly through the peritoneum and pericardium. 6. When the venom passes through the peritoneum it so affects the walls of the capillaries as to allow of their rupture and of the consequent escape of blood. The same phenomena appear on the bare surface of muscles thus poisoned. This, together with the defect of the coagulability of the poisoned blood, accounts for the excessive hæmorrhage about the fang wounds. 7. The blood globules are unaltered in venom poisoning. 8. The rattlesnake is not susceptible of injury by the venom of its own species. 9. The sulphites or hyposulphites of soda or lime have no antidotal power. 10. Carbolic acid sometimes delays the fatal result, and usually lessens local hæmorrhage. 11. These effects are due to no influence of the acid on the venom, but to a direct effect upon the local circulation of the envenomed part. 12. Carbolic acid has no value as true antidote, and when given internally does not affect the ordinary fatal issue.—*New York Medical Journal.*

Steel Billiard Balls.—Among other new uses of steel, one of the latest is that of the employment of this metal for billiard balls instead of ivory. They are very elastic, and are not liable to crack like those in present use.

Important to Chemists.—This advertisement appears in a Paris paper: "A young lady of forty-eight, having a moderate income, but possessing a patent for a new invention, wishes to marry a gentleman of sixty-five well versed in chemistry."

Stopper Cord.—Stopper cord consists of conical rolls of very elastic rubber, about 4 feet in length, and varying in diameter from one-half an inch at one end to an inch and a half at the other. Stoppers of any diameter between these limits may be cut from the roll and bored with a common brass cock borer, which must be moistened with water to prevent adhesion to the rubber. The stoppers are found to be air-tight under the pressure of 15 lbs. to the inch, provided the contact between the tube and stopper is at least half an inch in length.

The Rusting of Iron.—Perfectly pure water will not rust iron until it is heated to redness, when the contact with the metal instantly forms a red crust. Water oxidises iron more rapidly when it receives small quantities of mineral acids, while on the other hand an alkali or caustic lime destroys the oxidising faculty of water, a fact which is easily explained when we consider what strong affinity carbonic acid has for those bases. We are indebted to Fayen for the determination of the limits of this veto power which alkalies possess over the oxidation of iron in water. He ascertained that a saturated solution of potassa lye diluted with from 1,000 to 2,000 parts of water could still protect iron, but not when diluted with from 3,000 to 4,000 parts. Saturated lime water, when diluted three times, protected iron, but not when diluted four times. Saturated carbonate of soda, diluted with from fifty to fifty-four volumes, protected iron, but not so when diluted with even fifty-nine volumes. The finest cast steel was protected perfectly by even less potash. Rust is porous, and like all porous bodies, absorbs gases. White pig metal scarcely oxidises; grey iron with more facility, bar iron still easier, especially when red hot. Cold-short iron rusts least and slowest. Polish is the best preventive of rust, particularly when the article is kept in dry air.—*Scientific American.*

To Make Plaster of Paris Harder.—With one exception, all admixtures impair the hardness of the plaster. The exception is iron filings. When these are mixed with plaster they rapidly oxidise, and the coherent mass of oxide of iron formed, adds its own strength to that of the plaster, making a very firm mass, which has also the advantage of strongly uniting itself to surfaces of iron. I have not observed what proportion of the filings is best, but suppose they should form about one-fifth the whole weight.—F. Bowly.

NOTES AND QUERIES.

Safflower.—This material contains, in its natural condition, two colouring substances, one insoluble in water, known as carthamin, the pink dye; the other, soluble in water, a yellow colouring matter. In order to obtain the pink dye, it is in the first place requisite that the safflower should be as free as possible from immixtures, as, for instance, seeds, the leaves of the plant, or other flowers, hay, straw, and similar substances. The carthamin is obtained in the following manner:—Safflower is exhausted with a very weak solution of carbonate of soda; in this solution pieces of cotton wool are placed, and the alkali is next neutralised by dilute acetic, or sulphuric acid; the cotton wool thus becomes pink dyed, and the dye is removed from it again by means of weak solution of carbonate of soda, which solution, after the removal, of course, of the cotton, which has given up its colour, is neutralised by means of a dilute acid; a precipitate thus ensues, which is the carthamin; this may be purified by repeating the last treatment. The pink dye thus obtained is very beautiful, and especially applied to silk; only the dye is one of the most fugitive known. Repeatedly purified carthamin, mixed with French chalk, is often used by women to colour their cheeks.

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A NEW PRECIPITANT FOR POTASH, &c.

RESEARCHES ON THE COMBINATIONS OF MOLYBDIC ACID WITH PHOSPHORIC ACID.*

By M. H. DEBRAY.

At the beginning of this century, after Berzelius had determined by numerous and delicate analyses the composition of most of the then known mineral substances, chemists were struck with the simplicity with which their composition could be expressed by means of the proportional numbers given by his researches. This character of remarkable simplicity to which we have now been long habituated, appeared to distinguish Mineral from Organic Chemistry, in which complicated formulæ consequent upon the infinite variety of bodies formed from a small number of elements are the general rule.

The distinction is disputed by most of the eminent chemists of the present day. There is indeed no essential difference between the reactions of organic and those of inorganic chemistry, neither have the compounds in the latter that degree of simplicity which some like to attribute to them.

The discovery of the silico-tungstic acids and their salts, by M. Marignac, has recently furnished a very remarkable illustration of a series of bodies of very complex composition, and yet possessing a sharpness of reactions and a beauty of crystalline form at least as great as the simple products of our laboratories. The study of the combinations of phosphoric and molybdic acids has led me to the discovery of bodies of the same order, of a still more complicated composition, but as well defined and as perfectly crystallised as the compounds of silico-tungstic acid.

I. It is known that the solution of molybdate of ammonia in nitric acid possesses the property of precipitating ordinary phosphoric acid, giving a yellow body almost insoluble in all acids. This precipitate contains about 89 per cent of molybdic acid, a little over 4 per cent of phosphoric acid, the rest being ammonia and water. Upon boiling this in an excess of *aqua regia*, the ammonia is destroyed, and a yellow liquid obtained which yields by spontaneous evaporation beautiful doubly oblique prisms of a yellow colour, which consist of a combination of one equivalent of anhydrous phosphoric acid with twenty equivalents of anhydrous molybdic acid, and a certain quantity of water, corresponding to 13·3 per cent of the weight of the hydrate.†

These crystals, very soluble in water, can form two other hydrates; the one containing 23·4 per cent of water, that is to say, double that contained in the first for the same quantity of anhydrous acid; the other only 19·6 per cent. The hydrate with 23·4 per cent is obtained by the spontaneous evaporation of aqueous solutions of phosphomolybdic acid in large regular octohedra; the hydrate with 19·6 per cent is deposited from concentrated solutions strongly charged with nitric acid; these crystals, less beautiful and stable than the preceding, belong to the rhombohedral system.

The small quantity of phosphoric acid which unites in these compounds with molybdic acid (3·7 to 4·1 per cent) suffices to modify profoundly its properties. True molybdic acid may give a soluble hydrate which was first isolated by Mr. T. Graham in the dialysis of acid solutions of molybdates, and more recently by M. Ullik from molybdate of baryta and sulphuric acid,‡ but this hydrate gives colourless solutions and is uncrystallisable, while the hydrates of phosphomolybdic acid are yellow and easily crystallisable. Moreover, the reactions of this acid differ essentially from those of molybdic and phosphoric acid: Thus whilst all molybdates are soluble in acids, we find that phosphomolybdic acid precipitates from their strongly acid solutions, potash, the oxides of rubidium, cœsium and thallium, ammonia and the nitrogenous organic alkaloids. Soda and lithia, which give no precipitate under these conditions, separate themselves by this reaction, as by many others, from potash and its congeners, whilst thallium approaches it in a decided manner.

The metallic oxides are not precipitated by phosphomolybdic acid in a sufficiently acid solution.‖ Oxide of bismuth is not an exception, although it forms with phosphoric acid a compound almost insoluble in nitric acid, as M. Chancel has shown; moreover, the mixture evaporated deposits crystals of phosphomolybdic acid in the acid solution of bismuth.

If it is demonstrated that a well-defined body can result from the combination of one equivalent of one substance with twenty of another, there is no reason why there may not some day be discovered still more complex combinations. It will, therefore, be important to examine if the substances which we constantly find, in minute quantity it is true, in a great number of minerals, do not form an integral part of those minerals, the same as more abundant substances, and do not communicate to them special characters. This fact, demonstrated as far as the association of fluorine and chlorine with phosphates, may extend to many other combinations. I may be permitted to remark that if there exist definite combinations of iron and carbon, it will not be necessary to imagine a relation very different to that governing the combination of molybdic acid with phosphoric acid, to obtain bodies having nearly the composition of iron and steel. Thus the compound CFe_{30} would contain only 0·72 per cent of carbon (Fe = 28, C = 6.)

II. The composition of the yellow phosphomolybdates of potash, thallium, and ammonium, obtained by precipitating these bases in acid solutions, may be represented by the general formula,

$$3RO, PO_5, 20MO_3;$$

the salts of potash and ammonia also contain 3 equivalents of water of hydration.

These are well-defined compounds, and not mixtures, for it is easy to obtain them crystallised. It is sufficient to fuse, at a dull red heat, the salts of potassium and thallium, to obtain an oily liquid, solidifying on cooling to a mass of crystals; in the thallium salt these crystals are sufficiently distinct and brilliant to admit of the hexagonal pyramid which terminates them being distinguished with the naked eye. A few grammes of material are sufficient for these experiments.

The ammoniacal salt is obtained in small yellow brilliant crystals by mixing two solutions of pyrophosphate of soda and acid molybdate of ammonia; the precipitate is produced slowly in consequence of the transformation

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* This memoir, recently read before the French Academy, appears so important that we have decided to give it uncondensed, notwithstanding its length and the numerous demands upon our space at this season.—Ed. C. N.

† I have already pointed out this reaction in a former paper on Molybdenum (*Comptes Rendus*, xlvi., 1098), but I did not continue the study of these bodies, not having then a convenient method of analysis.

‡ Ann. d. Chem. u. Pharm., cxliv., 204.

‖ I have already shown this property of phosphomolybic acid to the Chemical Society of Paris (*Bull. de la Soc. Chim.*, 2, v., 405), but without studying the compounds so formed. Long before my researches M. Sonnenschein had pointed out for the precipitation of organic alkaloids, a reagent obtained by treating with an excess of soda the precipitate of phosphomolybdate of ammonia, to drive off the ammonia, and dissolving the residue in nitric acid; it is now clear that this reagent is nothing more than a salt of phosphomolybdic acid, which behaves like the acid itself.

of the pyrophosphoric acid into ordinary phosphoric acid under the influence of the acid liquid.

A solution of phosphomolybdic acid precipitates neutral nitrate of silver. The precipitate gradually changes to microscopic crystals, the composition of which may be represented by the formula:—

$$7AgO, PO_5, 20MO_3+24HO.$$

This salt dissolves in dilute nitric acid, and the liquid on evaporation furnishes small yellow brilliant crystals of a salt containing two equivalents of base,

$$2AgO, PO_5, 20MO_3+7HO.$$

III. Phosphomolybdic acid and its salts are only stable in the presence of acids; alkalies generally change them into ordinary molybdates and into phosphomolybdates, in which the two acids are united in the more simple proportion of 1 to 5,

$$(PO_5, 20MO_3)+Aq=PO_5, 5MO_3, Aq+15MO_3.$$

These phosphomolybdates are colourless or nearly so, and have a pearly appearance; they are soluble in water and easily crystallisable; an excess of acid reconverts them to the state of yellow phosphomolybdates, setting phosphoric acid free,

$$4(PO_5, 5MO_3)+Aq=3(PO_5, 3HO)+PO_5, 20MO_3+Aq.$$

I give below the formulæ of some of the beautiful salts of this new class of phosphomolybdates:—

Ammonia salt	$6(NH_4O)$,	$2PO_5$,	$10MO_3 + 14HO$,
Potash ,,	$6KO$,	$2PO_5$,	$10MO_3 + 14HO$,
Soda ,,	$6NaO$,	$2PO_5$,	$10MO_3 + 28HO$,
Silver ,,	$6AgO$,	$2PO_5$,	$10MO_3 + 14HO$.

It would appear that these formulæ could be simplified by dividing all the terms by two, but as the action of acids furnishes a fresh type of salts equally well crystallised, represented by the general formula,

$$5RO, 2PO_5, 10MO_3 + Aq,$$

it is preferable to retain for the first salts the formulæ assigned to them.

Some of these salts may also form double salts with nitrates: I give one of these compounds:—

$$[6(KO, NO_5) + 6KO, 2PO_5, 10MO_3] + 18HO.$$

The facility with which the phosphomolybdic acid giving white salts changes into yellow phosphomolybdic acid and phosphoric acid, has prevented me hitherto isolating it.

IV. The analysis of the preceding compounds presents very great difficulties when recourse is had to the hitherto known methods of separating the bodies composing them. I have used to effect this object two new processes which deserve notice, as they are susceptible of generalisation.

Separate the phosphoric acid from the molybdic acid by passing over a mixture of phosphomolybdic acid and lime, heated to incipient redness in a porcelain boat, first a current of sulphuretted hydrogen, then of hydrochloric acid. There are formed chloride of calcium, sulphide of molybdenum crystallised like the native sulphide, and chlorophosphate of lime, or apatite, also crystallised. The chloride of calcium is removed by water, and the apatite by hydrochloric acid, which does not attack the sulphide of molybdenum; the latter being easy to wash and collect, is carefully weighed. The phosphoric acid is easily estimated in the hydrochloric solution.

When alkaline phosphomolybdates are under examination, a portion of the alkali transformed into chloride volatilises at the high temperature of the operation in the gaseous current; to estimate the alkali recourse must be had to the following process:—Dissolve the phosphomolybdate in excess of ammonia, and add to the solution ammoniacal nitrate of silver; on ebullition crystallised tribasic phosphate of silver is first obtained, and then colourless molybdate of silver equally crystallised; the alkali remains in the liquid, where it is easy to determine it.

ON THE

FORMATION OF DOUBLE SULPHOCYANIDES OF CERTAIN OF THE ALKALOIDS, &c.

(CONTINUATION.)

By WILLIAM SKEY,
Analyst to the Geological Survey of New Zealand.

Since my last communication I have found that platinum, gold, iron, tungsten, and chromium should be added to the list of metals capable of forming insoluble double sulphocyanides with the alkaloids generally and sulphocyanogen, and others no doubt remain to be added, especially from the platinum group.

Annexed are short descriptions of some of the more characteristic of these salts—

Sulphocyanide of Platinum and Morphia precipitates as a red oily looking transparent substance, when a solution of ammonio-bichloride of platinum is brought in contact with a mixed solution of a salt of morphia, and a soluble sulphocyanide; it does not give any reaction of sulphocyanogen with the perchloride of iron, except it is first decomposed by an alkali. If quina is substituted for morphia a yellow crystalline precipitate falls, which melts at a temperature under 200° F. to a yellow oil. The nicotina salt with platinum is a dark red crystalline substance, which as in the case of the morphia salt has to be decomposd by an alkali before the sulphocyanogen reveals its presence by the iron test.

Sulphocyanide of Gold and Atropia, obtained by adding an atropia salt to a solution of sulphocyanide of gold in sulphocyanide of potassium, forms semi-spherical red oily looking drops, adherent to the vessel's sides; at a temperature under 212° they agglomerate to a viscid substance.

The Quina Salt with Gold appears as granular crystals, adherent to enclosing vessel. A morphia salt gives no precipitate with this solution of gold.

Sulphocyanide of Iron and Nicotina is an oil at common temperatures, blood red by transmitted, and green by reflected light; its lustre is metallic.

The morphia and quina compounds with iron are plastic and adhesive at common temperature, while veratria yields pale red crystals, readily soluble in water, the other salts of iron mentioned being but slightly soluble.

Sulphocyanide of Tungsten and Quina falls as a gelatinous yellowish precipitate when an alkaline tungstate with excess of a soluble sulphocyanide is acidified and brought in contact with a solution of this alkaloid; it fuses at a low temperature, gradually acquiring a green colour.

The other alkaloids generally furnish gelatinous precipitates with this solution of tungsten.

A solution of the red sulphocyanide of chromium also furnishes flaky or gelatinous precipitates with the alkaloids generally; with nicotina, however, a semi-solid translucent substance forms, adherent and continuous, of a purplish colour, insoluble in alcohol or ether; heated with solution of potash it fuses, turns to a pink colour and evolves nicotina; it also gives the reaction of chromium and sulphocyanogen.

Certain of the alkaline bases also appear to form double sulphocyanide salts with certain metals and sulphocyanogen, for when mixed with a soluble sulphocyanide and shaken up with ether, the ether is but slightly coloured, but on the addition of a salt of zinc or tin the whole of the colouring matter can be removed to the ether by further agitation; the etherial solution from the tin when removed on to water and boiled, furnished oily looking blue globules which sank through the water; when long heated it becomes solid, it gave abundant indications of tin and sulphocyanogen. The presence of water or ether has not been tested for.

As before stated, these double sulphocyanides are generally characterised by their comparative insolubility in

water; the degree of this, however, has only been determined in the case of the strychnia compounds with zinc, which requires about 30,000 parts of water to every part of strychnia present; it is therefore as insoluble in water as tannate of strychnia.

The behaviour of veratria with iron leads to the supposition that in those cases where solutions of metal fail to afford precipitates when mixed with the alkaloids in presence of hydrosulphocyanic acid, double sulphocyanides may still be formed.

As being connected with the subject in hand the following particulars relative to the formation of other double salts are noted here.

A double sulphocyanide of mercury and zinc falls as a crystalline precipitate when a salt of zinc is mixed with a solution of sulphocyanide of mercury; it is almost insoluble in water; no precipitate is formed with cadmium or tin in place of the zinc. Possibly a good process for the separation of zinc, cadmium, and tin from each other could be based upon their respective deportments with mercury or the alkaloids in presence of sulphocyanogen.

Double iodides of mercury or platinum with the alkaloids also form when the proper solutions for the same are mixed together; the sulphocyanides of mercury with the alkaloids are light coloured, and insoluble in excess of iodide of potassium. The double sulphocyanide of platinum and strychnia is a dark bluish red crystalline substance, feebly soluble in water, more soluble in sulphocyanide of potassium. The other sulphocyanides are generally still more insoluble in water or sulphocyanide of potassium; their colour is also bluish red.

January 17, 1868.

ON THE CHEMICAL REACTIONS IN THE ROASTING OF PYRITES.*

By J. H. TIEMANN, JUN.

Ever since pyrites has been used in the manufacture of sulphuric acid, it has been frequently noticed that instead of the roasting chamber being filled with an invisible mixture of atmospheric air and sulphurous acid, visible white fumes arise from the glowing pyrites. This has been noticed more particularly in muffle furnaces (where the pyrites is roasted in a chamber heated from the outside), and especially in Spencer's muffles, which are fifteen metres long. It is hardly possible that this occurs only in muffle furnaces; its occurrence here is more readily perceived than in other furnaces—"kilns," for instance. These white vapours, as is known, are anhydrous sulphuric acid. The condensation of liquid sulphuric acid in the flues connecting with the leaden chambers, is undoubtedly owing to this cause. The fact that these fumes are frequently, if not always formed, induced Mr. Fortman, at the laboratory of the Collegium Carolinum, in Brunswick, to institute a number of experiments on the subject.

The material used was a piece of nearly pure iron pyrites, with well-defined crystals, interspersed with quartz; analysed in the ordinary way, it yielded 50·21 per cent of sulphur. The pure pyrites FeS_2 requires 53·3 per cent sulphur. For all the experiments, the pyrites was reduced to a very fine soft powder.

The powder was placed in a hard glass tube ¾ inch wide, which was placed in a Liebig's combustion furnace (the same as for an organic analysis), and brought to a red heat, while air was drawn through the tube by an aspirator. With careful and gradual heating, the point at which the pyrites ignites may easily be seen at the same moment with the first evolution of sulphurous acid; the white fumes make their appearance, and continue without interruption during the whole time of roasting. In the first experiment, the tube was connected with a flask containing a solution of caustic soda, and a tube with dry caustic soda between it and the aspirator. It was at once apparent that the white fumes passed through both absorbents into the aspirator; the absorption was, therefore, very incomplete, although all the sulphurous acid was absorbed. It is known that anhydrous sulphuric acid mixed with air or other gas, is very difficult to condense. It is, therefore, not surprising that in this case the white fumes were imperfectly retained; the quantity of the uncondensed portion (principally anhydrous acid) was remarkably large, as shown by analysis. 1·5 grammes of powdered pyrites were roasted; the dry caustic soda was dissolved in the solution of soda in the flask; this was oxidised with chlorine, and precipitated with chloride of barium, which gave 3·657 grammes sulphate baryta—corresponding to 33·49 per cent sulphur. According to which, 16·72 per cent of the sulphur in the pyrites was lost—as anhydrous sulphuric acid [50·21—33·49 = 16·72.] The roasted pyrites retained a small portion of sulphur at those points where it was in direct contact with the combustion tube. This evil was corrected by frequently turning the tube during the roasting, and the mass was heated gradually from the front of the tube towards the rear; in this way a residue was obtained entirely free from sulphur, and of a bright red colour.

In the above experiment, it was found that the dry caustic soda absorbed more rapidly than the solution; the soda tube was therefore changed for one three times larger, and the experiment again made, giving the following results:—Roasted pyrites, 1·366 grammes. The gases, as before, first passed through soda in solution, and then through the tube with dry soda. At the end of the operation, the soda in both was neutralised; the solution divided into two portions; one portion was immediately precipitated with chloride of barium; the other half was first oxidised with chlorine and then precipitated. The first gave 2·485 grammes, the latter 1·986 grammes, of sulphate of baryta, so that in 100 parts pyrites—

Sulphur, absorbed as sulphurous and anhydrous sulphuric acid	48·39 pts.
Sulphur, absorbed as anhydrous sulphuric acid alone	38·31 ,,
Sulphur, absorbed as sulphurous acid	10·08 ,,

So that from 50·21 per cent sulphur in the pyrites, 1·82 parts were lost by incomplete absorption (50·21—48·39 = 1·82.) According to which, the amount of sulphur eliminated as anhydrous sulphuric acid, was almost four times as great as that which passed off as sulphurous acid. An apparently unlikely result, especially when we consider that the insolubility of the sulphate of baryta in a concentrated solution, or an imperfect roasting of the precipitate, may have easily caused an error.

A more perfect result was expected by determining the sulphurous acid with iodine solution by the burette. In so far as the roasted pyrites retains more of the sulphur, and the sulphurous acid is entirely absorbed, this method must give a perfect way of determining the relation between the two acids. The anhydrous acid can easily be determined by the difference between the sulphur in the pyrites and in the sulphurous acid formed.

1·549 grammes pyrites was roasted, as before; all the caustic soda was united in one solution, and the solution was diluted to exactly 1,000 cubic centimeters. 10 c.c. treated by the well-known method, required 1·75 c.c. iodine solution (1 c.c. iodine solution = 0·00032 sulphurous acid); so that again, according to this experiment, the amount of anhydrous sulphuric acid is much greater than the amount of sulphurous acid.

These preliminary experiments, which will be continued, show that the amount of anhydrous sulphuric acid, formed in roasting pyrites, is very important, and much greater than has been supposed. They show further, that the amount varies, depending upon the temperature and other circumstances which are still to be investigated.

* Dingler's Polytechnic Journal, vol. 187, part 2.

ON THE

DETECTION OF METHYLATED SPIRITS BY CHEMICAL REACTIONS.*

By Dr. J. W. GUNNING, Professor of Chemistry at the Athenæum Illustre at Amsterdam, and Scientific Adviseur to the Netherlands Ministry of Finance.

In consequence of the passing of an Act by the Netherland's States General in July, 1865, changing and greatly enhancing the excise duty on spirits, it became necessary to provide the industry of Holland with an alcohol denaturised in such manner as to be unfit for drinking purposes, and for the manufacture of liqueurs, such as Curacao, &c. The excellent success obtained with methylated spirits in Great Britain induced the Netherlands administration to allow in a similar manner the use of methylated spirits in the Netherlands. The wood spirit, or methyl alcohol of commerce is a mixture of various substances which may be met with therein in larger or smaller quantity according to differences in the manufacture, and divers methods of purification. English made wood spirit may, and often does, contain ammonia in consequence of the method which obtains here of saturating the rough pyroligneous liquor with chalk previous to distillation. The methyl-alcohol which is supposed to be chiefly present in wood spirit is very often only met with in really subordinate quantity, beside this, wood spirit contains aceton, acetate of methyl, and probably also formiate of methyl. The presence of compound ethers in wood spirit is easily proved by treating it with caustic potassa, or, better yet, by heating it in a sealed glass-tube with ammonia, and subsequent distillation, when there will be left behind in the retort salts of the acids of the previously existing compound ethers. Aceton can be proved to exist, since if wood spirit is treated with fuming nitrous-nitric acid and a salt of silver, fulminate of silver is formed. The peculiar odour which wood spirit emits, and which is rather pungent, is not owing to the substances just alluded to, but to peculiar volatile empyreumatic oily substances present in very small quantity, and not separately known in consequence of the difficulty of separating them. It is clear from what has been just stated in reference to wood spirit, that it would be utterly futile and impossible to find for such a compound as a whole a ready and perfect chemical test. One must not lose sight of the fact, moreover, that the compounds met with in wood spirit are neither of them such as are prominent by some or other conclusive and strictly characteristic test, or yield products of decomposition which are highly characteristic; while in methylated spirits, moreover, wood-spirit is mixed with ethyl-alcohol, which latter both in chemical and in physical properties bears a great likeness to the former. We are therefore obliged to have recourse to empirical reagents for detecting wood spirit in alcoholic liquids. Although it is true that methyl-alcohol yields on oxidation formic acid and ethyl-alcohol on the contrary acetic acid, and both these acids admit of being readily distinguished from each other, the plan to apply this property as a test and reagent is a too cumbrous mode of working to be adopted with ease and economy of time.

The application of the reagents known as Fuch's, to wit, a solution of iodide of potassium, and iodide of mercury in caustic potassa, and that of Reynolds', viz., chloride of mercury in caustic potassa, for the detection of wood spirit, owe their efficacy to the property possessed by the compounds met with in wood spirit to dissolve and keep in solution certain compounds of mercury which are insoluble in pure alcohol and in water. I have laboured in vain to obtain the precise composition of the precipitate which Fuch's reagent yields with pure alcohol, since I have not been able to obtain that precipitate in a sufficiently pure state. As regards the composition of Reynolds' precipitate, it is undoubtedly the yellow oxide of mercury; the following simple experiment proves this: add to a very weak solution of chloride of mercury (corrosive sublimate), a few drops of a solution of caustic potassa, and add afterwards wood spirit, when, on being shaken, it will be seen that the precipitate formed at first is entirely dissolved. While studying these reagents I have in the first place tried to determine whether this solvent action is due to all or only to some of the compounds met with in the wood spirit of commerce; it was of course also necessary to determine what influence is exerted by such substances as essential oils, fusel oil, and compound ethers, which in larger or smaller quantity may and often do occur in such alcoholic fluids wherein it is desired to detect the presence of wood spirit, and to determine whether or not they also exercised a solvent action. As regards the first point I have clearly made out that the reaction just alluded to is not caused by the pure methyl-alcohol, but is owing to other substances met with in commercial wood spirit, and especially to aceton. I have purposely prepared pure methyl-alcohol, both by decomposing pure crystallised oxalate of methyl, and by the well-known chloride of calcium plan, and on experimenting with this pure substance I have found it to behave towards the reagents above alluded to as pure ethyl-alcohol; but the precipitate which obtains is immediately dissolved on addition even of the slightest quantity of aceton. As regards the other point, I have found that many essential oils, fusel oil, and compound ethers exert a disturbing influence on the reaction of the above-mentioned tests, so that as many of these substances cannot be, either at all, or even only partly eliminated from fluids in which it is desirable to test for the presence or absence of wood spirit, the use of the test and reagents becomes either doubtful, or in many cases even impossible. I have therefore found it preferable to modify the composition and application of the reagents in some points, keeping, however, the main principle the same. Fuch's reagent is, as will be easily perceived, Nessler's ammonia test, but in this instance its use is not exactly to detect ammonia; if it so happens, however, that a fluid wherein one desires to test for wood spirit with Fuch's reagent does at the same time contain ammonia, phenomena will be observed indicating the presence thereof but it will be readily perceived also that then the reactions are more clearly defined and more strongly marked just for the very category of substances wherewith in this case one may have to deal; as a consequence hereof in the majority of instances there need not be the least uncertainty concerning the presence or absence of wood spirit. I give herewith the prescription for making and describe the use of the modified reagent as fit for the purpose for which it is here desired. Dissolve 16·66 grammes of iodide of potassium and 23·08 iodide of mercury in the smallest possible quantity of pure distilled water, this solution is then diluted with 500 cubic centimetres of alcohol, containing at least 92 per cent of pure alcohol. Also prepare a solution of ammonia, and one of caustic potassa in alcohol of the same strength; it is hardly necessary to say that the solution of caustic potassa in alcohol should be made extempore, *i.e.*, just when wanted. The alcohol to be applied for this purpose ought to be of the very purest quality; the purity may be recognised by applying the reagent just described as a test in the following manner: after the solution of the iodide of potassium and iodide of mercury has been diluted with alcohol, a few drops of the alcoholic ammonia solution should be added to a small portion of the alcoholic solution of iodides, and after that a few drops of the alcoholic solution of caustic potassa should be added; there should then ensue a beautifully brown coloured precipitate, slmilar in pigment to the well-known kermes mineral, without even the slightest yellow tinge. It is very seldom the case that alcohols met with in commerce are so pure, in fact I have found that and the presence of compound ethers and of fusel oil to some

* Translated from the Dutch by A. Adriani, M.D., Ph.D., &c.

extent so alters the reaction of the test just described, that the precipitate obtained is always more or less divergent from the true colour it should exhibit; in order to obtain alcohol fit to be used as above directed, the alcohol of commerce should be treated with animal charcoal, and repeatedly distilled after addition of some caustic potassa. My reason for applying alcohol as solvent [menstruum rather] is that, by its use, one is not disturbed in testing scents, *e.g.*, Eau de Cologne for wood spirit, by the essential oils and resinous substances met with therein, which, if water was applied as solvent for the reagents, would become precipitated. If the reagent just before alluded to be applied to methylated spirit, no precipitate is formed at all, and the fluid remains perfectly clear. It is also the aceton present in wood spirit which prevents the formation of a precipitate with the modified test; but it is clear that the solvent power of this substance has its limits, and that thus by adding excess of the reagent to the fluid to be tested, one might be brought to erroneous conclusions. It is best to take about 10 cubic centimetres of the fluid to be examined, to add thereto, first, one or two drops of the mercurial solution, next, as much of the ammoniacal solution; and finally, from six to ten drops of the solution of caustic potassa; if no precipitate ensues, then a fresh portion of 10 c.c. should be taken, and the experiment repeated with a somewhat larger quantity of the mercurial solution. If so it happens that the quantity of wood spirit be very small, *i.e.*, if, for instance, it is less than 1 per cent, the fluid under examination will then, after the addition of the caustic potassa, not remain clear, but exhibit either an opalescence, and with some kinds of wood-spirit, even become somewhat yellow, and at the same time turbid. I have good reasons to ascribe this phenomenon to a difference in the quantity of the aceton, or of the acetate of methyl usually met with in commercial wood spirit, since, as I will explain more fully presently, compound ethers have the tendency to render the precipitate yellow. I have found it quite possible and easy to estimate even quantitatively within pretty fair range the quantity of wood spirit present in a given sample of an alcoholic fluid. It is therefore, however, best to modify the order of adding the required reagents in this manner that one first adds the alcoholic ammonia solution, next the alcoholic potassa solution, and then drop by drop, and cautiously, and at intervals of time, the alcoholic mercurial solution, until a permanent turbidity sets in; experiments made by me with mixtures of pure alcohol and 1, 2, 4, 5, and 10 volumes per cent of wood spirit, have proved to me that the number of drops of the mercurial solution applied is pretty fairly proportional to the quantity of wood spirit present in the mixture under examination.

(To be continued.)

The Royal Society.—There are 53 applications for the Fellowship this year, eight of them being chemists of some standing.

Colourless Varnish with Copal.—To prepare this varnish the copal must be picked; each piece is broken, and a drop of rosemary oil poured on it. Those pieces which, on contact with the oil, become soft, are the ones used. The pieces being selected, they are ground and passed through a sieve, being reduced to a fine powder. It is then placed in a glass, and a corresponding volume of rosemary oil poured over it; the mixture is then stirred for a few minutes until it is transformed into a thick liquor. It is then left to rest for two hours, when a few drops of rectified alcohol is added and intimately mixed. Repeat the operation until the varnish is of a sufficient consistency; leave to rest for a few days, and decant the clear. This varnish can be applied to wood and metals.—*Journ. Applied Chem.*

TABLE

FOR ASCERTAINING THE WEIGHT OF A CUBIC FOOT OF ANY MINERAL ORE, METAL, EARTH, OR ANY OTHER SUBSTANCE, EITHER NATIVE OR ARTIFICIAL, FROM ITS SPECIFIC GRAVITY.*

By DR. LEWIS FEUCHTWANGER.

1728 inches comprise one cubic foot, and one cubic foot of water weighs at a temperature of 60° Fahrenheit, 62½ pounds avoirdupois. By ascertaining the specific gravity, and multiplying with 62½ pounds, the exact weight of one cubic foot is obtained.

	Sp. gr.	Pounds Avoirdupois. Cubic foot weighs.
Anthracite coal	1·5	94
Antimonial copper, tetrahedrite, or grey copper	5·0	300
Antimonial silver	9·5	600
Antimony ore, grey sulphuret	4·5	279
Antimony metal	6·5	400
Apatite, or phosphate of lime	3·0	186
Arsenical iron pyrites, mispickel	6·0	370
Asbestos	3·0	186
Asphaltum, mineral pitch	1·0	62
Baryta sulphate	4·5	310
Baryta carbonate, Witherhite	4·0	248
Bismuth	9·7	600
Bituminous coal	1·5	90
Black lead, graphite	2·0	125
Black jack blende, sulphuret of zinc	4·0	250
Bog iron ore	4·0	250
Brown hæmatite	4·0	250
Building stones, comprising granite, gneiss, syenite, &c.	3·0	186
Calamine	3·3	190
Chromic iron	4·5	260
Copper pyrites	4·0	260
Derbyshire spar, fluor spar	3·0	186
Feldspar	3·0	190
Flint	2·5	110
Loose sand	—	95
Franklinite	5·0	310
Galena	7·5	465
Gold (20 carats)	15·7	1000
,, (pure)	19·2	to 1200
Gypsum	2·3	130
Iron—cast iron	—	450
,, magnetic ore	5·0	310
,, spathic ore	3·0	200
,, pyrites	5·0	310
,, pyrrhotine, or magnetic pyrites	4·5	280
,, specular iron ore	4·5	290
,, wrought	—	487
Limestone, hydraulic	2·7	150
,, magnesian	2·5	130
Manganese, binoxide of	4·8	294
Malachite	4·0	248
Mica	2·8	160
Novaculite, or whetstone	3·0	186
Ochre	3·5	217
Platinum, metal and ores	16 to 19	1116
Porcelain clay	2·0	140
Pyrites, iron	4·5	280
Quartz, pure, compact	2·6	155
,, loose, angular, and round sand	—	100
Trap	3·0	186
Vitreous copper, copper glance	5·5	341
Wood tin, stream tin	7·0	434
Zinc, sulphide or blende	4·0	248
Zincite, red zinc ore	5·5	331
Zinc carbonate	4·4	268
Zinc silicate	3·4	200

* Condensed from the American Journal of Mining.

PROCEEDINGS OF SOCIETIES.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, March 31st, 1868.

Edward Schunck, Ph.D., F.R.S, &c., President, in the Chair.

"*Description of a Dolerite at Gleaston, in Low Furness.*" by E. W. Binney, F.R.S., F.G.S.

During the last thirty years the tract of land known as the Hundred of Low Furness has been investigated and described by several geologists. It was one of the earliest fields investigated by the venerable Sedgwick, who has left us a most valuable memoir of his labours in that district. Since then, Mr. Jopling, myself, and Sir R. I. Murchison, and Professor Harkness, have published descriptions of the silurian mountain limestone and permian formations of the country. Miss E. Hodgson has also given us information as to the drift deposits overlying the palæozoic strata. Still, notwithstanding what has been done, it may confidently be asserted that the peninsula comprising the southern part of the Hundred of Low Furness has yet to be carefully examined before its geology can be said to be thoroughly known.

None of the above-named persons appear to have been aware of the occurrence of any trap dykes in this district, judging from their published writings, with the exception of Mr. Jopling, who, in his sketch of the geology of Low Furness and Cartmel, comprehending the Hundred of Lonsdale north of the sands, published in 1843, when speaking of the geology of Gleaston, says :—"Carboniferous limestone abounds, and in the quarries near the castle are many fossils beautifully preserved in the shale beds between those of the limestone ; there is also a vein of trap." At page 72 the same author says "there are also appearances of trap near Gleaston, associated with limestone breccia."

In the month of October last, Miss E. Hodgson was so kind as to send me some specimens of rocks from Gleaston, which puzzled her a good deal. Some of the parties to whom she had sent them called them dolomites, whilst others named them traps and greenstones. To the latter opinion Miss Hodgson, I believe, was inclined to add the weight of her sanction. Not having previously seen, or even heard of, the occurrence of any such rocks in the district where they were said to be met with, I went over to examine them, and having been furnished with information by Miss Hodgson, easily found the place where they are exposed at Gleaston Green. At that time Mr. Jopling's book had not been seen by me. The space occupied by these singular rocks, at least so far as at present exposed, is so limited that all that can be seen is very soon ascertained. Specimens were collected, and a few observations made. The former, by the kindness of my friend Professor Roscoe, F.R.S., were analysed for me in the laboratory of Owens College. It is only by the labours of the chemist that geologists can with any certainty decide upon the age and origin of such rocks as those which are met with at Gleaston.

On approaching Gleaston Green from Scales, the mountain limestone appears to occupy the country so far as it can be seen. In a quarry below the old castle on the roadside, this rock in the northern part is very hard, and dips to the west at an angle of 25°, whilst in the southern part, where it is softer, it dips in the same direction at an angle of 16°. Owing to the covering of drift, the limestone is not seen nearer to the mill, but it probably extends further in that direction. At a short distance below the mill, dark-coloured laminated shales are seen in the bank on the roadside, dipping apparently at an angle to the N.N.W. We then come to the rocks at the end of the Green. They appear to run in an east and west direction, and are not now exposed for more than twenty yards. From north to south they may probably extend about forty yards, but certainly for more than half of that distance, towards the beck, they are not now seen until the land rises on the bluff south of the beck, where they reappear as a reddish and bedded trap ash, having an east and west direction, and dipping N.N.W. at an angle of about 60°. This ash is succeeded by a coarse breccia of a few yards in thickness, so far as exposed, which dips slightly north of west, at an angle of 25°, and then is covered up by grass so as not to be seen, but no doubt, from sections in the adjoining lane and borings made on the rise of the strata, dark-coloured shales occur again, and the dyke most probably intrudes through these shales, which are in every respect like limestone shales, but no organic remains were observed in them so as to make us certain of their geological age.

Returning to the north side of the beck, nothing is exposed of the district west of the hard rocks seen on the Green, owing to the thick covering of drift in that direction; but Mr. Hodgson has proved, by a series of boreholes, the occurrence of upper permian sandstone, red shale, and limestone shale—the first to the S.W., the second to the west, and the third to the N.W. of Gleaston ; and Mr. Ashburner has proved limestone shale and limestone in bores to the E. and N.E. of the locality where the rock is found.

In the bluff on the south side of the stream, as previously stated, the rock appears more like a trap ash of a reddish brown colour, and exhibits traces of bedding and white lines like carbonate of lime. Immediately adjoining trap ash, and on its rise, occurs the coarse breccia composed of fine-grained siliceous rocks, cemented together with quartz, and like a permian breccia ; but although the beds are near together, there was not evidence to show whether the trap ash gradually passed into the breccia or intruded through it, still the breccia appeared to dip in the same direction, but at a much less angle, namely, 25° to a little west of north. This is a very interesting fact to prove ; for if the rock graduates into the breccia, it would appear to be of permian age, and most probably a melaphyr, but if it is intrusive, as the evidence on the whole appears to prove, all we can say is that it is of later date.

This breccia is composed of angular pieces of a fine siliceous stone, of a pink colour, more resembling quartzite than anything else, cemented together by small quartz crystals, and containing minute quantities of protoxide of manganese. The form of the fragments is very like that of the rocks in the permian breccia of Rougham Point, near the mountain limestone of Humfray Head, and there consisting for the most part of the neighbouring mountain limestone, but no limestone has yet been met with in the Gleaston breccia as might have been reasonably expected ; and the pink quartzite is a rock hitherto unknown in the district. The permian breccia, so far as my experience goes, although sometimes containing volcanic ash, are composed of the rocks now found in the neighbourhood where they occur, and nearly always vary with the older geological rocks of the district. The composition of the Gleaston breccia makes me hesitate in designating it as permian, as it may be some rock altered by the dyke.

The rock in its best state of preservation is remarkably hard, of a reddish brown colour, has a moderately straight fracture, and a pinkish white streak, and its specific gravity is 2·92.

Three average samples of the rock were taken, two from the north and one from the south side of the beck. All of them were more or less decomposed by exposure to air and moisture, but No. 26 much less than Nos. 23 and 24. Professor Roscoe, on analysis, found their present chemical composition to be as follows :—

	South of Stream, No. 23.	North of Stream, No. 24.	North of Stream, No, 26.
Silica	45·54	50·96	51·10
Peroxide of iron	24·76	24·20	21·58
Alumina	7·70	14·48	9·40
Lime	13·84	7·32	6·24
Magnesia	0·57	0·55	1·33
Carbonic acid	2·78	1·90	2·70
Alkalies, water (by difference)	4·82	0·59	7·65
	100·00	100·00	100·00

The only rock which I know of a similar composition, is a probable variety of green earth, resembling a decomposed pyroxene, described by Macfarlane in the *Canadian Naturalist*, New Series, vol. iii., No. 1, page 5, in a paper on the cupriferous bed of Portage, Lake Michigan, which consists of—

Silica	46·48
Alumina	17·71
Protoxide of iron	21·17
Lime	9·89
Magnesia trace	—
Alkalies (by difference)	1·97
Water	2·78

Mr. David Forbes, F.R.S., to whom were forwarded small specimens of the rocks and the above analyses, kindly informed me that the rocks were so much decomposed that it was difficult to pronounce with certainty as to what they were, but he was inclined to think that they were an intrusive dolerite of carboniferous age rather than a melaphyr. The iron had been changed from a protoxide into a peroxide, and the lime had resulted from the decomposition of a lime felspar. As Mr. Forbes had found the presence of titanium united with iron in all the carboniferous dolerites he had examined, I took several ounces of the three samples above given, and having heated them in a crucible so as to convert the iron from a per into a protoxide, extracted it by a magnet. About half an ounce of this iron was very carefully examined by Mr. Thorpe in Dr. Roscoe's laboratory, especially for titanic acid, and no trace of that substance was found. He used the test with microcosmic salt, having separated iron and silica. The absence of titanium in the rock would lead us to believe that it was of later origin than the carboniferous age, but if traces of that metal had been found, it would not only have settled the question as to the age, but it would have shown a connection with the hæmatite iron ores of Whitehaven and Ulverston, all of which contain more or less of titanium as proved by the deposits of that metal found on the sides of old furnaces where hæmatite has been smelted. Is the rock of permian age? It is certainly not much unlike the melaphyr of the German geologists, and the breccia near the dolerite is not greatly different from that of Ballochmyle, described by Mr. A. Geikie, F.R.S., in the *Geological Magazine* for December, 1866, but we could not obtain direct evidence that the breccia gradually passed into the trap, the latter appeared to protrude through it, but certainly the trap and the breccia dipped in the same direction, the one at about 60° and the other at 25° a little West of North. This point can only be satisfactorily determined by cutting a trench and showing the contact of the breccia with the trap. The extent of the dyke can only be traced for a few yards east and west, as previously stated, and none of the hæmatite iron deposits, so far as known, have been found south of it. Its age also appears to be more recent, even supposing it to be permian, than those deposits which for the most part must be considered of carboniferous age. The occurrence of this trap might have been considered to have some connection with the deposition of the iron had it been of carboniferous age, but it is evidently more recent, and therefore could have had nothing to do with it further than disturb or displace it.

"*A Search for Solid Bodies in the Atmosphere*," by R. ANGUS SMITH, Ph.D., F.R.S., &c.

I have so frequently for many years attempted to find, and have found, organic substances which have passed from the air into liquids in which they were collected, that perhaps the Society will scarcely attend to another attempt, although it indicates, I think, some progress. It was in the year 1847 that I first collected what I believe was matter from the respiration and perspiration, and found that as it was kept it grew into distinct confirmed forms.

Whilst examining some matters relating to the cattle plague I found one or two remarkable points. I had before that time used aspirators to pass the air through liquids, except in the oxidation experiments. At that time I used simply a bottle which contained a little water. The bottle was filled with the air of the place, and the water shaken in it. The difference of air was remarkable. A very few repetitions would cause the liquid to be muddy, and the particles found in many places were distinctly organic.

It may, however, interest the Society to hear of a few of these previous attempts, the latest made till recently. I shall therefore read from a report to be found in the appendix to that on the cattle plague.

"Mr. Crookes also brought me some cotton through which air from an infected place had passed. It was examined at the same time. Taking cotton in the mass nothing decided was seen; but when it was washed some of the separate films were coated over with small nearly round bodies, presenting no structure, or at least only feeble traces of it, and perhaps to be called cells. I had not sent gun-cotton, as I intended, to Mr. Crookes, fearing the rules of the post; otherwise there would have been more certainty that the bodies spoken of did not exist previously on the cotton. However, Mr. Dancer, who has examined cotton with the microscope oftener than most persons, even of those experienced in the subject, had never observed a similar appearance.

"The liquid had also a number of similar bodies floating in it.

"It was then that Mr. Crookes sent a liquid which he had condensed from the air of an infected cowshed at a space a little above the head of a diseased cow. It was also examined, and it presented similar indications of very numerous small bodies. Not being a professed microscopist I shall not attempt a description, but add that they clearly belonged to the organic world, and were not in all cases mere débris. We found also one body a good deal larger than the rest; it resembled somewhat a paramecium, although clearly not one.

"We found no motion whatever, and only this latter substance could be adduced as an absolute proof of anything organised being present. Next day I examined the same liquid; and, whether from the fact of time being given for development or from other causes, there was a very abundant motion. There were at least six specimens in the field at a time, of a body resembling the euglena, although smaller than I have seen it. When these minute bodies occur it is clear that more may exist, and germs in this early stage are too indefinite to be described. The existence of vital sparks in the organic substances in the air alluded to is all I wish to assert, confirming by a different method the observations of others. It might, of course, be said that since the bottle was opened at Mr. Dancer's, the air at that place may have communicated them. I answer that, before it was opened, a good glass could detect floating matter, some of it, however, as in the microscope proved, indefinite enough.

"Finding this, and fearing that the long time needful to collect liquid from the atmosphere might expose it also to much dust, I used a bottle of about 100 cubic inches dimensions, and putting with it a very little water, not above five cubic centimetres, I pumped out the air of the bottle, allowing the air of the place to enter. This

was done six times for each sample, the water shaken each time, and the result examined. This was done with the same bottle that was used in my early experiments with permanganate, and by the same method, except that water instead of that salt was used. At first considerable numbers of moving particles were found; but it was needful to examine the water used, and here occurred a difficulty. It was not until we had carefully treated with chemicals and then distilled the water again and again that we could trust it. Particles seemed to rise with the vapour, and if so, why not with the evaporating water of impure places.

"Having kept an assistant at the work for a week, and having myself examined the air of three cow-houses, I came to the conclusion that the air of cow-houses and stables is to be recognised as containing more particles than the air of the street in which my laboratory is, and of the room in which I sit, and that it contains minute bodies, which sometimes move, if not at first, yet after a time, even if the bottle has not been opened in the interval. There is found in reality a considerable mass of débris with hairs or fine fibres, which even the eye, or at least a good pocket lens, can detect. After making about two dozen trials, we have not been able to obtain it otherwise. Even in the quiet office at the laboratory there seemed some indications.

"I found similar indications in a cow-house with healthy cows; so I do not pretend to have distinguished the poison of Cattle Plague in these forms; but it is clear that where these exist there may be room for any ferment or fomites of disease; and I do not doubt that one class is the poison itself in its earliest stage. It would be interesting to develope it farther.

"I have recorded elsewhere that I condensed the liquid from the air of a flower garden, and found in it, or imagined I found, the smell of flowers. I do not remember that I looked much to the solid or floating particles, thinking them to be blown from the ground, but it does not affect the result, whether they be found constantly in the air or are raised by the action of currents."

Lately I tried the same plan on a larger scale. A bottle of the capacity of 4990 c.c. was filled with air and shaken with water. The bottle was again filled and shaken with the same water, and this was repeated 500 times, nearly equal to 2½ million c.c., or 2,495 litres. As this could not be done in a short time, there was considerable variety of weather, but chiefly dry, with a westerly wind. The operation was conducted behind my laboratory, in the neighbourhood of places not very clear, it is true, but from which the wind was blowing to all parts of the town. I did not observe any dust blowing, but if there were dust, it was such as we may be called on to breathe. The liquid was clouded, and the unaided eye could perceive that particles, very light, were floating. When examined by a microscope the scene was varied in a very high degree—there was evidently organic life. I thought it better to carry the whole to Mr. Dancer and to leave him to do the rest, as my knowledge of microscopic forms is so trifling compared to his.

ROYAL GEOLOGICAL SOCIETY OF IRELAND.

April 8, 1868.

Dr. Emerson Reynolds read a paper "*On the Formation of Dendrites.*" He had some years since noticed that when solutions of salts, &c., were placed upon a plate of clean glass, and the glass placed between the poles of a Ruhmkorff's coil, the salts gradually work over the surface of the glass in beautiful moss-like forms, which in many cases were characteristic of the compound contained in solution. The state of dilution at the same time having some considerable influence. The authors proposed to call them "electric cohesion figures." To produce them we will say that a drop of a solution of cyanide of potassium is put in the centre of a plate of glass, which is then placed upon a sheet of tin-foil. One pole of the coil is then brought into contact with the foil (it is immaterial which), and the other pole is placed in the centre of the drop; immediately on passing the current the solution begins to creep over the surface of the glass in moss-like convolutions.

The dendritic markings on minerals the author believed were formed under a similar condition. He exhibited a beautiful manganous dendrite taken out of the museum. It was a slab of concoidal limestone, and in Dr. Reynold's opinion illustrated his electrical explanation conclusively. There was originally a flaw in the limestone which was exactly at right angles with the plain of cleavage. Through these flaws, as was evident by the marks, the manganous solution had percolated, and had perhaps ultimately been the means of making the stone part in two, not however in the direction of the flaws, but in the plain of cleavage. The dendrites which were formed upon the surface in this case were produced from the well known fact that the two surfaces at the instant of their separation are in opposite electrical conditions.

This phenomenon may be illustrated to a certain extent by inserting a drop of the fluid into the interstice of a plate of mica, and then on suddenly parting the plate the dendritic forms are shown. To fix them the author dusts some finely dried pigment over the surface of the still moist plate, and then fixes this by some transparent varnish.

The other paper read was by J. Scott Moore, Esq., J.P.D.S., "*On Man and the Glacial Epoch.*" A modified spectroscope for examining minerals was also shown.

FOREIGN SCIENCE.

Paris, April 13, 1868.

Examination of fatty matters.—Manufacture of porous carbon.—New method of preparing magnesium.—Industrial preparation of oxygen.—Academy of Sciences.—Meteorites.—New compound of platinum.—Eruption of Vesuvius.

An ingenious method of testing fatty matters, founded upon the solubility of rosaniline in certain fatty acids, has been devised by M. Jacobsen; it is applicable, among other things, to the examination of cod-liver oil. A little piece of dry rosaniline placed in a sample of perfectly neutral oil, agitated and heated upon the water bath, remains undissolved, but if placed in a rancid oil, a red tint is rapidly developed. Oleic acid and the other fatty acids dissolve rosaniline in large quantity, and become opaque from the depth of the tint, because oleate of rosaniline is soluble, in all proportions, in oils and other fatty substances. This property enables the presence of fatty acids in oils to be detected. For instance, in commerce we have had for some years pretended white cod-liver oils which are only fatty fluids from very young animals, or veritable cod-liver oil, but which has been agitated with potash, allowed to repose and filtered. Since the therapeutic effects of cod-liver oil depend essentially upon the amount of free fatty acids which it contains, neither of these white oils can be valuable. Genuine cod-liver oil agitated with a little rosaniline is promptly coloured red, in the cold, and if heated upon the sand bath, the colour is very deep, while the bad specimens already referred to remain perfectly uncoloured.

When an oil which is only slightly rancid, contains but a small amount of fatty acids, the colouration often does not become sensible at first. In this case it is better to prepare a solution of rosaniline in absolute alcohol, and add a few drops of this to the oil to be examined, and heat on the water bath until all the alcohol has been

evaporated. If no fatty acid exist, the rosaniline soon separates and rises to the surface, or when the oil is too thick, rests in suspension as a brown powder. Samples of ordinary oil occurring in commerce, have given the following results:—Olive oil and that of sweet almonds remained uncoloured by rosaniline; poppy oil became slightly red; linseed oil became strongly coloured, its natural colour rendering the tint brownish; palm oil gave a colouration still more intense. One more experiment, it has sufficed to mix the olive oil with 5 per cent of oleic acid, to obtain with rosaniline a tint equal to that of raspberry juice. It is not proposed to heat to more than 100° C., otherwise errors might occur.

The fabrication of porous carbon, in various shapes, engages the attention of no inconsiderable number of persons in the present day; this kind of carbon is made most advantageously, your correspondent is informed, by the following process:—A mixture of wood charcoal and animal charcoal is ground to a coarse powder, mixed with sawdust, and dried at a steam heat; as soon as the material is dried, and while still warm, 20 per cent of tar is added. When cold a certain amount of asphaltum is added, and the mass pressed into moulds. The proportions in which the ingredients are used vary according to circumstances. The moulded objects are placed in boxes of sheet iron, and covered all over with a mixture of sand and charcoal; afterwards they are heated on the sole of a furnace. Gases which are disengaged during the operation are burnt in the furnace. The entire operation lasts about twenty-four hours. Careful attention is required during the calcination; the properties of the carbon depend, in a great measure, upon the management of this part of the process.

M. Reichert has devised a new method of preparing magnesium. He takes 1,000 grammes of the anhydrous double chloride of magnesium and potassium, pulverises it, and mixes it with 100 grammes of finely powdered fluor spar; this mixture is fused with 100 grammes of sodium. The compound proposed for use occurs in the mineral kingdom in tolerable abundance as carnallite. White pieces of this mineral are available, and require no previous treatment; coloured fragments must be dissolved in water, the impurities allowed to settle, and the lixivium evaporated.

M. Gondolo has made some improvements in M. Boussingault's process of extracting oxygen from the air by means of baryta. M. Boussingault, in 1852, found that in passing a current of air over baryta, heated to dull redness, oxygen was subtracted from the air, and binoxide of barium formed, and that upon then raising the heat to bright redness the oxygen was set at liberty so easily that the oxygen might be first absorbed and then evolved *ad infinitum*. M. Gondolo has made, in carrying out the detail of the process, certain changes which admit of oxygen being prepared upon a manufacturing scale. For the porcelain tubes he substitutes iron ones, which may be made either of wrought or cast iron. Internally a coating of magnesia is applied, and externally asbestos, so as to diminish the porosity of the tube and the consumption of fuel. These tubes are arranged in a brick furnace having dampers, by means of which the temperature may be changed at will, and dull redness and bright redness easily obtained. To the baryta a mixture of lime, magnesia, and a small quantity of manganate of potash is added; this prevents fritting of the material. M. Gondolo says that he has made 122 alternate operations, and that the atmospheric oxygen and nitrogen are easily separated upon an industrial scale; the apparatus has been at work during six months, and fulfilled its purpose thoroughly. The process is patented.

There were few memoirs relating to chemistry at the séance of the 30th March. M. Daubrée gave some further account of various meteorites. M. Balard presented a note by M. Schützenberger on a new compound of platinum. M. Frémy presented a note by M. Terreil, "on the action of saline solutions on minerals." Besides these there was a geological paper entitled, "On the actual eruption of Vesuvius," from M. Silvestri. The first mereorite M. Daubrée referred to was one found in the Philippine Isles, not far from the village of Mexico, province of Pampanga, said to have fallen in 1859. It appears to be of the common type; it consists of a confused, stony, crystalline mass, chiefly composed of magnesian silicates, in which are disseminated bright particles, having a metallic lustre; some are grey, and are particles of nickeliferous iron, the others, black, are composed of chrome iron; the latter are very numerous. The meteorite is traversed by black veins, which give it a marble-like appearance; it presents great resemblance, both in the pale portions and in the dark veins, to the meteoric stone which fell on the 5th August, 1812, at Chantonnay. The density of the meteorite from the Philippines is 3·61, that of the meteorite from Chantonnay 3·67. Treated by boiling hydrochloric, the meteorite now studied by M. Daubrée leaves 28·5 per cent of a residue not at present examined; the solution contains magnesia, protoxide of iron, a little oxide of nickel, and a very small quantity of alumina. Another meteorite, which fell at Murcie, in Spain, on the 24th December, 1858, formed the subject of a memoir by MM. Daubrée and S. Meunier. This meteorite is a very remarkable one, and was exhibited at the Universal Exhibition of 1867. Its density is 3·546, and it weighs about 114 kilogrammes. The mass is nearly entire, that is to say, the crust is almost everywhere apparent. This crust does not present the aspect ordinarily presented by stony meteorites; it has evidently suffered profound alteration since its formation. Particles having a metallic lustre are rare, but there are some—they consist of nickeliferous iron. In other places bronze yellow particles are seen, which have the characters of troilite. There exist, also, in this meteorite very brilliant particles possessed of a kind of metallic lustre. These particles also form veins. A careful examination has shown them to be crystals of hyaline By the blowpipe flame they are melted to a greyish enamel, and give the reactions of silica and alumina. The black portion of the meteorite which was considered least altered, was analysed by M. Meunier. The magnet separated 14·99 per cent of magnetic matter formed of nickeliferous iron and a trace of phosphide. Foremost among the analytical results, sulphide of iron is observed as making 20·52 per cent; this is doubtless the cause of the dark colour. A silicate attackable by hydrochloric acid, of the nature of peridote, gives 38·69 per cent, and a silicate resisting this acid, of the nature of pyroxene, 24·64 per cent.

M. Schützenberger, in endeavouring to effect the synthesis of oxychloride of carbon without the intervention of light, made a mixture of dry carbonic oxide and chlorine pass over platinum sponge heated to 400°. Under these conditions, the formation of sensible quantities of oxychloride of carbon takes place, but the platinum does more than exert catalytic action. A solid and volatile compound of platinum is produced which passes away with the current of gas, and may be collected in the form of a clear yellow flaky powder in the cold part of the tube. As the platinic compound is destroyed at a temperature little above that at which it is formed, to succeed, the current should be rapid. The new substance melts at about 150°, yielding a transparent yellow liquid, which on cooling solidifies to a crystalline yellow mass: in other experiments, an analogous product has been obtained, melting at 125°, whence the substance would not seem to be homogeneous. At a temperature of 350 to 400° it boils and distils, but decomposes in great part into metallic platinum and chloroxycarbonic acid. The substance is decomposed by water immediately in the cold, an effervescence of carbonic acid being produced, at the same time that a fine black powder is separated, and the filtered liquid, quite colourless and free from platinum, gives the reactions of a solution of hydrochloric acid. The black powder is pure platinum (representing the

whole amount of platinum in the body) possessed of great catalytic power, and below redness, it is converted, sometimes with incandescence, into very coherent metallic platinum. The mode of formation, and the decomposition of this body, under the influence of heat, and with water, leave no doubt as to its constitution. It is a compound of platinum, chlorine, and carbonic oxide. The most simple formula would be $(CO)''$, $Pt''''Cl_2$, in which Pt = 66·55, Cl = 23·9, and C = 4·05. Analysis has not confirmed this formula. A specimen crystallised from tetrachloride of carbon, and very pure in appearance, was analysed, it gave Pt, 63·5; Cl, 22·9; C, 5·35. These numbers lead to the formula $(CO)3\ Pt_2Cl_4$, in which Pt = 63·5, Cl = 22·9, C = 5·8. The product first obtained was, as has already been mentioned, purified by chloride of carbon; the crystals obtained after several purifications contained less platinum, and gave 60·87 per cent upon analysis, afterwards a minimum of 58 per cent of platinum. M. Schützenberger proposes to examine this point further.

M. Terreil's note will be referred to again.

M. Silvestri has examined the phenomena connected with the eruption of Vesuvius closely, and analysed many of the volcanic products. The lava is dark grey coloured, sometimes greenish or reddish on the surface. The substance possesses a crystalline structure; it exerts an energetic action on the magnetic needle. M. Silvestri distinguishes four kinds of lava; the density of these varied between 2·46 and 2·81; in a complete analysis recorded of one, compact lava, water was found to be present to the extent of 2 per cent. Three distinct kinds of sublimates were noticed, viz., white, greyish brown, and green. The white sublimate contained chloride of sodium and chloride of potassium, besides minute traces of chloride of copper. Oxide of copper is present in the greenish brown sublimate, of which it forms 5·85 per cent. The green sublimate contained ·61 per cent of oxychloride of copper; all these sublimates are mainly composed of chlorides of sodium and potassium. A large quantity of the sublimates was dissolved in water and a series of crystallisations made, in the mother-liquor reduced to a small volume, iodine and bromine were sought for: neither were detected. A spectral examination of the mother-liquor revealed nothing but sodium, potassium, and copper. M. Silvestri made his observations at the end of December, about the time of the maximum activity.

CORRESPONDENCE.

PERMANGANATE OF POTASH AND ORGANIC MATTER IN WATER.

To the Editor of the Chemical News.

SIR,—I have just read a letter in your number of the week before last, in which, after all that has been said to the contrary, the above-named substance is still spoken of in terms of approval as affording a means of estimating organic matter in water. As my opinion on the point differs in all respects from that expressed by the writer, and finding, more especially, that he has left untouched a main element in the question (if it be one), I shall be obliged if you will permit me to say a few words thereupon in this week's number of the CHEMICAL NEWS.

Soon after this method was proposed by the Danish professor, I made a series of experiments with it on a variety of naturally impure waters, but on finding the results so utterly irreconcilable with those arrived at by careful incineration of other portions of the same samples, I gave it up; though I must add that I did not then arrive at the source of its leading fallacy.

After these experiments, happening to meet the late Professor Clark, of Aberdeen, who took considerable interest in most matters connected with water analysis, and mentioning my results to him, I soon found that he also had arrived at a similar opinion as to the fallacious character of this mode of estimation.

Though I have frequently had occasion to express my unwillingness to be bound in any way by results obtained by the method, yet continuing to hear of its being much in use, I began to think that, with all my care, I had possibly mistaken some important points; and hence I was led to make another series of experiments a few years after.

Meanwhile, I am bound to say that I never found permanganate in much favour amongst the hard working "laboratory men," but chiefly amongst officers of health, public lecturers, and others, too glad to find this ready though empirical mode of operation to be easily induced to give it up, or at any time question its accuracy too closely.

On several occasions where I have found results put forward in evidence as to organic matter, greatly disagreeing with those of others, it usually came out, on cross-examination, that they were obtained by permanganate; but on finding its accuracy doubted, I have heard some of the firmer men add—that they had estimated by incineration as well, and that the two processes substantially agreed—a result, I may add, which I have never been able to realise. However, discrepant statements of this class have greatly tended to shake the faith of legal functionaries, as well as the public, in chemist's conclusions.

Some years ago, and since the introduction of the permanganate mode, a chemist of some public standing, but who had a good deal of public lecturing to attend to, gave evidence to a government committee, on which I was engaged, as to the quality of a certain water. He stated that it contained seven grains of organic matter in a gallon, whilst I, on the other hand, had been unable to find more than one grain. Both of our samples were taken at the same place, and on the same day, all of which I had to state subsequently in my evidence to the same committee. This led to the water being analysed by other chemists, none of whom I may mention found the seven grains of organic matter at first stated. Though I strongly believed that the doubtful results in this instance had been arrived at by the use of permanganate, yet from the absence of counsel on government committees I was unable to have the question put. This, and some other circumstances of a like nature, induced me to enter on the second series of experiments on permanganate to which I have already referred.

On this, the second occasion, the samples of water were synthetised for the experiments, that is to say, given portions of the usual earthy salts were dissolved in doubly distilled water, to which were added measured portions of various impure as well as pure organic decoctions, so as to accurately represent varieties of impure water. In some of the experiments, the organic matter was represented by soakings of putrescent manure, but in all considerable care was taken that the precise quality of the water used in each should be accurately known, irrespectively of either of the modes of analytical examination employed.

At this moment I have neither time nor inclination to hunt up, or put into shape, my notes of those experiments, but which will, I am sure, be deemed of less consequence when I have cited one example of the fallacious action of this very beautiful salt of potash, and which it will be seen must necessarily apply to most if not all waters.

I need hardly say, in this journal, that all potable water in this and in most countries, contains some iron, and not unfrequently traces of manganese—though the latter, being so small, is often estimated as iron. At the same time, with the exception of so-called chalybeate waters, either of these metals are usually found so very sparingly as to render their presence inappreciable to all but the analyst. Notwithstanding which, a third or a

quarter of a grain of iron in a gallon is far from an unusual quantity; whilst in chalk waters, which are the freest from iron, may be found a tenth of a grain, though this quantity is often set down as "a trace." It is also to be remembered that iron in water is usually estimated as "oxide," but in which state I need not say it never exists in the water, but usually as a colourless super-protocarbonate; that is to say, the iron found in the residuum of our evaporation in the state of oxide, after incineration, existed in the water as white proto-carbonate, and which is held in solution as chalk is, by an extra atom of carbonic acid. Grains of the white proto-carbonate pervade sand and soils to a much greater extent than is supposed, and, while in this state, is readily taken up by the water.

Permit me now to recapitulate the action of the permanganate, which I need hardly repeat here is briefly this:—It acts in water by readily imparting a portion of its oxygen to bodies requiring the same. As soon, however, as this oxygen is given out, the permanganate loses its splendid colour, and hence, obviously, arose its employment as a test. Now, as organic matter when dissolved in water has an affinity, or, let us say, an appetite for oxygen, so that when permanganate is added to a water in which this impurity exists, this manganic salt loses its colour, and the quantity of it thus decolourised is said to stamp the quantity of the organic matter present in the water. In a word, the presence of the one is said to be in a direct ratio with the loss sustained by the other.

Now, this might be all very well if it were true that impure organic matter was the only class of substance incident to water, and to which the permanganate is disposed to give out its oxygen, and thus deprive itself of its beauty. Such, however, is very far from being so, for, as we shall see, the salt in question is far more disposed to give out its oxygen to a really salubrious body which is as much incident to water as organic matter. Indeed, much more so if we take organic matter of the worst class.

As I have already stated—and it is only repeating what all admit—every potable water may be said to contain some iron, and this usually as a proto-salt. Now, as a tenth of a grain of a proto-salt of iron or manganese dissolved in water, will rapidly decolourise as much permanganate as three or four grains of even putrescible organic matter, I leave it to your correspondent and those who so tenaciously hold to this mode of estimation, to say whether the decolourisation of permanganate which takes place in a given sample of water, is due to noxious organic matter or to really salubrious iron, either of which it may obviously contain? But surely all this must be already familiar to the readers of your journal.

However, should the author of the letter in question (Mr. Muter) entertain any doubt on the matter, he may easily improvise an experiment.

Let him take a grain of proto-sulphate of iron—being the easiest come-atable proto-salt—and dissolve it in a gallon of any water, hard or soft, though distilled will be the most reliable, and then let him add his permanganate. I venture to say that if he were operating on water with the quality of which he was unacquainted, he would set this distilled water down as unfit for human use, as being greatly contaminated with putrescible organic matter. I may remark that a grain of protosulphate of iron with its seven atoms of water, and its acid, will very nearly represent the tenth of a grain of oxide, so often found in most waters. In some of my experiments I used, for the sake of accuracy, mineral proto-carbonate dissolved in carbonic acid under pressure, but I find any proto-salt is affected similarly by the permanganate. But above all, I find that this very salt is used by the volumetric men, as a mode of estimating iron; but how, in the face of this, it ever could have been used for separating another body in which the first was so likely to be present, and for which it has an equal affinity, is I confess a puzzle to me, unless indeed there is some mode of masking the one, whilst the other performs the required duty.

Let me add, in conclusion, that this objection to the use of permanganate is far from being the only one though it forms a most striking one.—I am, &c.,

THOS. SPENCER.

32, Euston Square, London, April 15th, 1868,

P.S.—I omitted to state that the water said to contain seven grains of organic matter came from highly ferruginous gravel, and contained a considerable amount of iron. It is probable, therefore, that if tested by permanganate, this iron would figure in the analysis as the organic matter in question.

ROYAL SCHOOL OF MINES.

To the Editor of the Chemical News.

SIR,—We hear much about the School of Mines in Jermyn Street. Can any one give me a comparative sketch of the advantages and cost of entrance of the Ecole des Mines in Paris? I have never seen it, but I hear rumour of supplies of oxygen, of economy for students, and of the greatest courtesy to strangers, of which I would fain learn more.

A great want in London for students and experimentalists who have advanced beyond a certain point, is an upper class-room, so to speak, where they can work at peace, uninterrupted by very young students, and be supplied with reliable reagents, so avoiding the worry and loss of time involved in having to test everything used, and possibly make it for themselves after all, and a comfortable room for weighing both on the balance and in their mind.

I hear reports of a new laboratory in King's College, and of one at the Royal College of Chemistry. The class of men interested in investigations, but not prepared to set up their own laboratories, must be large, judging from the number I have met. Surely if their requirements were met, a large class would be formed who would make such a laboratory by no means a losing concern, especially with attendants who would do some of the "dog-work," and an assistant whose time was entirely devoted to the interests of the class,—I am, &c., AN ASPIRANT.

To the Editor of the Chemical News.

SIR,—I feel considerable interest in the subject of the School of Mines, having served an apprenticeship after the fashion common amongst the coal miners of the north of England, and having all my life studied how my mining education might have been more thoroughly carried out.

I have met with several French mining engineers, and thus had the means of comparing my own science and practice with theirs, and also in the same way I have come across gentlemen educated at the Royal School of Mines in London.

You and the majority of your readers know what it is to be able to form an opinion which is satisfactory to yourself, and upon which you have no hesitation in acting; it is a condition only arrived at by personal experience obtained in the observation of facts, assisted by the mind educated to appreciate the value of all things seen which have any bearing upon the matter in question.

Now as far as I have been able to judge from what I have seen of the college-made mining engineers, they have been supplied with the "educated mind," but have been allowed to get too old readily to absorb the practical experience; a man of twenty-one does not like to begin the rudiments of practical life, and most assuredly he cannot obtain them during the vacations; there must be a considerable amount of every-day life for a man to get practical experience.

What I have to suggest is that the system prevalent amongst doctors and lawyers should be adopted. Let

each mining student serve a regular apprenticeship to a mining engineer in practice, and combine it with the theoretical college studies.

In order to do this effectually, colleges should be instituted in the centre of mining districts. For a Doctor of Physic or Divinity it does not particularly matter where he is educated. The former has the London Hospitals and abundance of subject matter under his very nose, where he can work up his practice in special paths not open to him in his country life, but for the London mining student, what has he to study in practice? unless he perhaps indulges in a field day amongst the sewers or underground railways.

Do not for a moment suppose that I wish to depreciate the value of college studies; but I think a man can more readily pick up theory while in practice, than he can after becoming a man bring himself down to the details required to get practice; and I therefore say, bring your schools to the mines and don't continue to try to take the mines to the schools. Have a college for science in London, as suggested by "Delta" and "A. L. E.," if you like, but don't call and use as a College of Science a School of Mines.—I am, &c.,

A. L. S.

RULE OF THUMB.

To the Editor of the Chemical News.

Sir,—The following astounding mixture for tempering steel chisels is given by a man, signing himself *Practical Experience*, in the pages of one of your contemporaries. When such absurdities as this pass current every week for scientific information, can there be a doubt of the necessity of elementary chemical instruction amongst the working classes? If it did no other good, it would at all events disclose to them the depth of their own ignorance, and teach those with *practical experience* not to be so ready to sneer at *science*:—

"The following is the mixture I have used:—Soda 2 oz., saltpetre 1½ oz., prussic acid 1 oz., (or oil of vitriol 2 oz.), soft water 3 gallons."

I am, &c., F.R.S.

MISCELLANEOUS.

Royal Dublin Society and the Royal Visit to Ireland.—The Royal Society are to give a *Conversazione* to the Prince and Princess of Wales on their arrival in Dublin. The Society's House in Kildare Street is being prepared for the occasion. The objects exhibited will be of scientific and art interest, particularly the latter. Part of the *suite* of rooms used for the occasion will be the Natural History Museum, which so far has not been open to the public since the present fine building has been erected. The minerals, &c., have been entirely re-arranged.

CONTEMPORARY SCIENTIFIC PRESS.

Under this heading it is intended to give the titles of all the chemical papers which are published in the principal scientific periodicals of the Continent. Articles which are merely reprints or abstracts of papers already noticed will be omitted. Abstracts of the more important papers here announced will appear in future numbers of the "Chemical News.")

Journal für Praktische Chemie.
December, 1867.

C. O. Grass, "Researches on the Analysis of Inflammable Gases, with especial Reference to that of Illuminating Gas." C. Winkler, "Contributions to the Knowledge of Indium." C. Barfoed, "On Gallate of Silver.

Annales du Génie Civil.
December, 1867.

Lardani, "An Improvement in the Manufacture of Sulphuric Acid." Luhmann, "An Improvement in Blast Furnaces." Coste and Taupin de Rosnay, "A Method of Obtaining Ammonia from Sewage and from the Waste Waters of Manufactories." Perier and Possoz "On the Use of Lime for Preserving Beet Juice."

Bulletin de la Société d'Encouragement.
October, 1867.

Guerard-Deslauriers, "On the Use of Bisulphide of Carbon for Estimating the Quantity of Tar, Pitch, and Resin contained in Artificial Fuel." S. de Luca, "On some Important Products obtained rom the Olive and from the Australian Myrtle."

Comptes Rendus.
December 16, 1867.

A. Wurtz, "On the Synthesis of Neurine." C. G. Wheeler, "On the Action of Aqueous Hypochlorous Acid on Essence of Turpentine and on Camphor.

No. 26.

E. Demance, "On the Amalgamation of Zinc Plates for Voltaic Batteries." P. Van Tieghem, "On the Transformation of Tannic Acid into Gallic Acid by Fermentation."

Sitzungsberichte der koniglich Bayerischen Akademie der Wissenschaften zu München. (Mathematischphysikalische Classe).
March 2, 1867.

Voit, "On the Relations of Creatine and Creatinine to Urea in the Animal Body, and on the Nature of Uræmia.

May 4.

Frischmann, "On Twin Crystals of Chrysoberyl." A. Vogel, "Observations on the Solubility of some Silicates."

July 6.

Seidel, "A Contribution to the Knowledge of the Limits of Accuracy of Chemical Balances." C. Voit, "On the Deposition of Uric Acid from Urine."

NOTES AND QUERIES.

Prussian Blue Paste.—Pure blue, even in paste, provided it be not too thin, will always, when prepared with care, exhibit that particularly coppery gloss which is also possessed by some kinds of indigo of good quality. This property is caused by a peculiar reflection of the light, and appears to have its origin in the peculiar state of aggregation of the particles of these substances, both of which, as is well known, are very difficult to ground to an impalpable powder. As a rule, the Prussian blue of commerce is, in chemical parlance, a mixture of neutral and basic Prussian blue. The coppery gloss is a peculiar property of well made blue, and cannot be brought on by artificial means.

Extraction of Grease by Bisulphide of Carbon.—Can any of your correspondents furnish me with any information upon this subject, or with the name and address of any firm, in this country or elsewhere, who make the apparatus employed for the purpose?—C. L. W.

Production of Carbonic Acid.—I should recommend your correspondent, "G.," to use magnesite, a natural carbonate of magnesia, for obtaining carbonic acid required. The gas obtained by subjecting this mineral in a retort to a red heat, is pure and odourless, and the resulting magnesia will be found more valuable than the original material. But the supply of magnesite is not large.—J. A. X.

TO CORRESPONDENTS.

A. L. Steavenson.—We shall be pleased to hear further from this correspondent on the subject mentioned towards the conclusion of his letter. Such practical experience as he can furnish will be very valuable.

K.—1. Griffin and Sons. 2. Only the firm you have named. 3. Precipitate nitrite of silver by mixing nitrite of potash with nitrate of silver; wash and re-crystallise. 4. Consult the index. 5. We do not remember positively, but in some cases there was a slight colour.

W. S. S. W.—Use gun-cotton instead of powder for blasting the rock; that will enable you to get over the difficulties.

O. George.—This correspondent, who wrote respecting a trade process, is informed that the information can be sent if he will give his private address. We cannot publish it.

Querist.—It is a matter for a lawyer, rather than an editor, to advise about.

Tyro.—The mineral is iron pyrites.

Communications have been received from W. A. Simpson (with enclosure); F. A. Abel, F.R.S.; A. Bird (with enclosure); W. Lant Carpenter (with enclosure); A. A. Fesquet; Dr. A. Adriani; R. C. C. Lippincott (with enclosure); Professor Heaton; W. Blackril; J. M. Reid, Halifax, N.S.; J. Mayer (with enclosure); W. W. Reeves (with enclosure); A. L. Steavenson (with enclosure); W. H. Harrison; Howard Grubb; A. Le Sueur; T. Spencer (with enclosures); W. Bayliffe; J. Wilson; A. Wykes; Dr. Reimann; G. Bird, M.D.; J. Samuelson; H. Williams; T. Reader; J. G. Major; W. A. Smith; E. K. Muspratt; W. J. Morgan; J. E. Taylor; W. Blackie; Ernest Le Barbier; T. R. Fraser, M.D., Halifax (with newspapers); A. Vacher; T. Prentice and Co.; W. Bailey and Son; Harvey and Reynolds; D. W. Edwards; W. C. Deane.

THE CHEMICAL NEWS.

Vol. XVII. No. 438.

ON GUN COTTON TRANSPORT.

The accidents which occurred at Newcastle and elsewhere, in consequence of the disregard of precautions in the transport and handling of nitroglycerine, have created a feeling of distrust in the minds of the traffic managers of railway companies in connection with all explosive substances other than gunpowder. According to the *Pall Mall Gazette*, this distrust has now increased to such a degree that permission is frequently withheld for the transmission by railway even of the compressed gun-cotton charges used for blasting purposes, although the regulations which apply to the transport of gunpowder more than suffice to guard against the possibility of serious accident with gun-cotton.

With the object of investigating the risks incurred in conveyance of compressed gun-cotton charges by railway, Mr. Wilson, of the goods manager's office, North Eastern Railway, in conjunction with Mr. Prentice, the managing director of the Gun-cotton Company, has tried a series of experiments, of which the following is an abstract:—

A small box of cotton containing 125 charges, said to be equal in effects as a blasting agent to a quarter cask of gunpowder, was taken into the cricket field. A fuse was inserted and lighted. When the flame reached the gun-cotton there was a great blaze like the burning of a heap of loose straw, but no explosion; in less than half a minute there was no flame except from the burning of the brown paper in which the gun-cotton had been packed inside the box. The box was of wood about half-inch thick, and was nailed, but not bound with iron at the corners; it was one of the ordinary packages used for sending the cotton out.

Several charges were then laid on the rails near the coal depôts, and coal waggons were run over them: some of them were ignited, others were not. Some of them were placed so that an engine should pass over them, they were all ignited. Mr. Prentice took an axe and chopped one charge into several pieces, there was no explosion or ignition. Small pieces of gun-cotton placed on the iron rim of a wheel and sharply struck with a hammer exploded, or rather detonated.

In all the cases where ignition was produced by concussion, whether of a hammer on iron, or of the wheels of an engine or waggon on the rails, it was very evident that only so much as was actually struck exploded or detonated, the part not struck firing from the explosion, and burning like so much straw or flax.

To make sure that they were dealing with the article which produces such an effect when exploded in close confinement, a hole was bored into a large block of hard tough wood, in which Mr. Prentice placed a charge of gun-cotton with a fuse attached to it; he then filled up the hole with broken slate tightly rammed, and fired the fuse. When the gun-cotton exploded the block of wood was shivered to pieces, each piece being blown several yards away.

Mr. Wilson says that the results of these experiments convince him that they may safely carry gun-cotton along with other goods in ordinary waggons, adopting the same rules as now apply to the conveyance of cartridges.

Death by Electricity.—A writer in *Harper's Weekly* recommends a new form of capital punishment. The method is "death by electricity." He says:—"It is perfectly painless and absolutely instantaneous." Another writer says:—"Why is not this in every way preferable, and a thousand times less shameful to a civilised people than the slow strangling now practised upon our criminals?"

ORGANIC APPEARANCES IN COLLOID SILICA OBTAINED BY DIALYSIS.

In the report of the meeting of the Chemical Society held on April 2nd, given in our number for April 10th, we gave a brief account of some interesting observations on the occurrence of "Organic appearances in Colloid Silica obtained by Dialysis." The Author of the paper, Mr. W. Chandler Roberts, has kindly forwarded the accompanying drawing of these remarkable appearances, together with some further details.

The dendritic forms in the air-dried gelatinous silica, vary in size from 0·2 to 0·5 millimetre.

When magnified 90 times they appear as radiating fibres; and when magnified 700 times each fibre resolves itself into one or other of the following forms, as shown in the figure:

1. The most common form.
2. The end of each fibre surrounded by an apparently vacuous space indicating its growth in the partially solidified jelly by abstracting water from the mass.
3. Apparent fructification of the organic forms.

ON THE ESTIMATION OF MANGANESE AS PYROPHOSPHATE.

By WOLCOTT GIBBS, M.D.,
Rumford Professor in Harvard University.

The existence of an ortho-phosphate of manganese and ammonium, corresponding to the well-known salt of magnesium, was long since ascertained by Otto.* The subject has more recently been studied by Debray,† who has described a series of analogous phosphates, all of which are remarkable for their insolubility. Otto's salt, $P_2O_8Mn_2(NH_4)_2 + 2H_2O$, from its highly crystalline structure, the facility with which it is formed, and its insolubility, appeared well adapted to the quantitative estimation of manganese, and the following analyses show that this metal, like magnesium, may be advantageously precipitated as ammonio-phosphate and weighed as pyrophosphate.

To the solution of manganese, which may contain salts of ammonium or of the alkaline metals, disodic orthophosphate is to be added in large excess above the quantity required to precipitate the manganese as orthophosphate. The white precipitate is then to be redissolved

* Bull. de la Société Chimique. Nouvelle Série ii., p. 11.

† Ann. der Chemie und Pharmacie, viii., 173.

in excess of sulphuric or chlorhydric acid, heated to the boiling point, and ammonia added in excess. A white or semi-gelatinous precipitate is produced, which on boiling or standing for some time, even in the cold, gradually becomes crystalline, and finally is completely converted into beautiful talcose scales which have a pearly lustre and a pale rose colour. It is best to precipitate each time in a platinum vessel, in which the ammonia-phosphate may be boiled for ten or fifteen minutes, and to allow the salt to remain at a temperature near the boiling-point of the liquid for an hour after it has become crystalline. The ammonio-phosphate may then be filtered off and washed with hot water. The washing takes place with extraordinary facility on account of the crystalline character of the salt. The orthophosphate, after drying and ignition, yields pyrophosphate of manganese as a nearly white powder. In this manner—

	Grm.			Grm.				
1.	0·9555	$MnSO_4$	gave	0·8985	$P_2O_7Mn_2$	=46·68	p.c.	MnO
2.	1·1400	,,	,,	1·0717	,,	=46·67	,,	,,
3.	0·8145	,,	,,	0·7646	,,	=46·63	,,	,,
4.	0·9464	,,	,,	0·8886	,,	=46·66	,,	,,
5.	1·3181	,,	,,	1·2390	,,	=46·68	,,	,,
6.	1·0565	,,	,,	0·9950	,,	=46·76	,,	,,

The formula requires 46·67 per cent ($Mn=54$). The sulphate employed was pure and perfectly anhydrous. In two analyses of crystallised chloride of manganese, not quite free from mechanically mixed water, Mr. F. W. Clarke obtained 27·08 and 27·07 per cent of manganese. In the same salt the percentage of chlorine was found to be 35·68, which corresponds to 27·14 per cent of manganese.

The advantage of this method over that commonly employed for the estimation of manganese, is that the process permits us to weigh the metal in the form of a perfectly definite compound, and not as an oxide which cannot be safely assumed to consist wholly of Mn_3O_4. When manganese is associated with the alkaline earths, it is of course first to be separated as sulphide, or by Schiel's method, as a hydrate of the sesquioxide. The ammonio-phosphate is almost absolutely insoluble in boiling water, in ammonia, and in solutions of salts of ammonium. The salt is nearly white, but sometimes becomes a little more red upon the filter. If it assumes a rather deep dull red colour, the whole of the phosphate of manganese has not been converted into ammonio-phosphate. The precipitate is then to be redissolved in dilute chlorhydric acid, more phosphate of sodium added, and then ammonia in excess, after which the boiling is to be repeated. This repetition is very rarely necessary, a little practice enabling the analyst to judge when the conversion from the flocky-gelatinous to the crystalline condition is complete. The filtrate from the crystalline salt is perfectly free from manganese. Phosphoric acid cannot be determined by precipitation, as ammonio-phosphate of manganese, because the crystalline character of the salt upon which the success of the process depends is only produced by digestion with an excess of phosphate. Bette‡ has described an ammonio-phosphate of zinc which, like the corresponding manganese salt, is almost absolutely insoluble in water. Debray‖ has analysed similar salts of nickel and cobalt; and Otto§ has also described the analogous ammonio-phosphate of iron. I have myself prepared an ammonio-phosphate of cadmium which, like the other salts of this group, is extremely insoluble in water. All of these salts, however, are more or less readily soluble in ammonia and in salts of ammonium, and after repeated trials I have not succeeded in rendering any of them available for analytical purposes.—*Am. Journ. Science.*

‡ Ann. der Chemie und Pharmacie, xv., 129.

‖ Loc. cit.

§ Ann der Chemie und Pharm., xvi., 199.

ON THE

DETECTION OF METHYLATED SPIRITS BY CHEMICAL REACTIONS.*

By Dr. J. W. GUNNING, Professor of Chemistry at the Athenæum Illustre at Amsterdam, and Scientific Adviseur to the Netherlands Ministry of Finance.

(Concluded from page 187.)

Inasmuch as there might be present in an alcoholic fluid which one should desire to test in the manner described, non-volatile organic substances which would interfere with the proper action of the reagents, it is clear that the non-volatile should be removed by previous careful distillation, while, as regards the volatile organic substances, their disturbing influence may be judged from the following experiments, made by adding to 5 c.c. of pure alcohol a few drops of the undermentioned substances, and after having well mixed these with the alcohol, the reagents have been added in the usual manner, viz., mercurial solution, ammonia, and caustic potassa.

Amyl alcohol.—With 3 drops, no sensible difference, *i.e.*, the reddish-brown precipitate ensues as with pure alcohol; with 10 drops, the precipitate gets a decidedly yellow tinge.

Acetic Ether.—With 3 drops [let it be understood, 3 drops of the ether added to 5 c.c. of pure alcohol], but with addition of half the bulk of the pure alcohol of ether, a reaction ensues, as if a very small amount of wood spirit were present, viz., a faint yellowish opalescence.

Valerianate of Amyl.—(A dilute solution in pure alcohol was applied.) With 10 drops thereof added to 5 c.c. of pure alcohol a reaction ensued, as if a very small amount of wood spirit was present.

Pine Apple Essence.—Reaction as the last foregoing, but less strongly marked.

Essential Oils.—Eau de Cologne, *i.e.*, the same made with non methylated spirits, yields the same reaction as pure alcohol, but the precipitate is more yellow-coloured. If to 5 c.c. of eau de Cologne, 2 drops of wood spirit [not methylated spirit, of course] are added, there ensues no precipitate at all, and at the utmost, a faint opalescence. Larger quantities of essential oils than are met with in scented waters disturb the action of the reagents in a far higher degree. Alcohol mixed with from 4 to 6 per cent of oils of lavender, rosemary, entirely prevent the formation of any precipitate, and act therefore as wood spirit does. Oil of turpentine exerts the same action but in a somewhat less degree.

Sulphuric Ether.—The same reaction as if a small quantity of wood spirit were present.

Aldehyde, *i.e.*, in this case the crude distillate obtained on treating alcohol with bichromate of potassa and sulphuric acid, acts as wood spirit, *i.e.*, no precipitate ensues.

Spiritus Nitri dulcis.—No precipitate.

Muriatic Ether.—The same reaction as wood spirit.

Chloroform exercises no influence *i.e.*, the reddish brown precipitate is formed.

Benzol exercises a slight influence; the precipitate is somewhat yellow.

Amylen exercises a very marked influence the reaction is the same as if a small quantity of wood spirit was present.

These experiments prove that there are some substances which interfere with and more or less disturb the reaction for wood spirit; some of these substances act indeed as if wood spirit itself were present, while others again hinder the reaction, and modify the colour of the precipitate due to pure alcohol only.

As regards the first batch of these disturbing substances, there is no real difficulty to detect them, neither

* Translated from the Dutch by A. Adriani, M.D., Ph.D., &c.

is there the least difficulty to eliminate them from a fluid which it is desirable to test; for distillation with caustic potassa, followed by treating the distillate, previously diluted with water, with animal charcoal will have the desired effect, while if aldehyde is present, as for instance in the case of spiritus nitri dulcis, a separate distillation with ammonia is required to fix the aldehyde.

As regards the second batch, viz., the essential oils, if present in rather larger quantity in an alcoholic fluid which one should desire to test for wood spirit, they may be reduced to small traces by the application of the following expedient manipulation. Mix the alcoholic fluid in question with as much magnesia alba, the ordinary magnesia of the chemist's shops, as will yield a thick magma, next add twice the bulk of the alcohol of a thoroughly saturated aqueous solution of common salt, and then bring this whole mixture on a filter previously filled with magnesia; the perfectly clear filtrate is next submitted to distillation, the first small portion of the distillate, which yet will show turbidity on becoming mixed with water, is set aside, and the remainder of the distillate can then be applied to be tested for wood spirit. I ought to observe here, that if an alcoholic fluid which contains wood spirit is submitted to fractional distillation, all portions of the distillate will yield pretty fairly an equal amount of wood spirit.

The experience I have acquired by the opportunity offered to me, especially of testing on a large and ample scale divers alcoholic fluids, enables me to state that chemists will find the test described by me quite reliable to speak of, or discern with certainty the presence or absence of wood spirit in an alcoholic liquid submitted to examination. But I must expressly say that in the strictest sense the reagent only applies to aceton, and it is, therefore, only then permitted to draw the conclusion that wood spirit is really present when also the smell and other concomitant phenomena justify this conclusion. The administration of the excise in the kingdom of the Netherlands, *vulgo* Holland, does not allow wood spirit to be used for making methylated spirit, *i.e.*, mixing with alcohol, unless the wood spirit has been previously submitted to the following test: 1 part by volume of wood spirit mixed with 99 parts by volume of pure and absolute, *i.e.*, anhydrous alcohol, must be very plainly and readily recognised by the reagents above described. If there might be a doubt, or also in cases where it might be of great importance, I think the experiment by oxidation ought not to be omitted; for this reason I will briefly allude to it yet. I proceed in the following manner: 97·5 grammes of bichromate of potassa are mixed with 146·25 grammes of sulphuric acid previously diluted with 775·35 grammes of water; 35 c.c. of this fluid is mixed with 4 c.c. of the alcoholic fluid to be submitted to experiment, and placed in a small retort; the mixture while in the retort is left to itself for 24 hours, and then about 4-5ths of the contents of the retorts (several experiments of this kind are conducted at the same time, and also with mixtures of known purity) is distilled off, care being taken to keep all under the same conditions. The distillate is mixed with magnesia and evaporated. The reason why I prefer magnesia to carbonate of soda is, that the latter acts upon the aldehyde of the distillate; in consequence whereof the residue of the evaporation is rather strongly coloured, and consequently apt to reduce more of the alkaline solution of permanganate of potassa than can be accounted for by the quantity of formiate of soda which is formed. When magnesia is applied, the residue of the evaporation remains colourless even when the process of heating upon a water bath is very greatly prolonged, which is always required when essential oils are to be entirely eliminated. The residue of the evaporation is next taken up with distilled water, and then mixed with excess of an alkaline solution of permanganate of potassa of precisely known strength, *i.e.*, oxidising power, and left for at least two days quietly standing; the solution of permanganate is, after that time, tested by the well known method. I found it necessary to have two days' rest, as by experiments purposely instituted with a small quantity of pure formiate of soda, I found out that even up to 48 hours after the beginning of the experiment the titre of the permanganate had only become constant.

The following are results obtained with the described mode of proceeding:—

First Series.

	Quantity of oxygen required to oxidise the formic acid formed.
Pure alcohol	1·2 milligrammes
,, ,,	1·6 ,,
,, + 1 per cent wood spirit	2·6 ,,
,, + 3 ,,	5·2 ,,
,, + 5 ,,	8·3 ,,

Second Series.

Eau de Cologne	2·7 ,,
,, ...	2·4 ,,
,, + 2 per cent wood spirit	6·9 ,,
,, another variety ..	6·2 ,,
,, ...	6·4 ,,
,, + 1 per cent wood spirit	9·2 ,,
,, ,,	10·1 ,,

Third Series.

Pure alcohol	0·7 ,,
,, ...	1·1 ,,
Methylated spirit	17.0 ,,
,, ...	12·0 ,,
Eau de Cologne	4·4 ,,
,, ...	4·0 ,,
,, made with methylated spirit	26·0 ,,

The above-mentioned results of experiments by oxidation may be left to tell their own tale, but it will be seen that as regards the disturbing influence of essential oils especially, the checking of the experiments by simultaneously making the experiment with alcohol of known purity, is desirable.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, April 16, 1868.

DR. WARREN DE LA RUE, F.R.S., &c., President, in the Chair.

THE minutes of the previous meeting were read and confirmed. Amongst the donations to the library was the new catalogue of scientific works recently prepared by order of the Royal Society.

Dr. F. GUTHRIE was formally admitted as a Fellow, after having signed the statute book. No new candidates were proposed, but the name of Mr. Thomas Bournes, Teacher of Chemistry, 47, Rigby Street, St. Helen's, Lancashire, was read for the second time. The ballot was taken on behalf of Lieutenant Francis C. H. Clarke, Royal Artillery, Staff College, Farnborough Station, who was declared to have been duly elected as a Fellow of the Society.

PROFESSOR GUTHRIE described and exhibited an *Improved Voltastat*, by which the current of a galvanic battery may be maintained perfectly constant and regular by a self-acting arrangement, which will become intelligible by the following description:—A vertical glass cylinder of about the size of a test tube is charged with dilute sulphuric acid, with a layer of mercury below occupying about one-third of its total contents. Partly immersed in the acid liquid is a pair of platinum electrodes insulated

by glass fused upon the wires at that portion which passes through the cork stopper of the jar, and a comparatively wide glass tube open at both ends is fixed in the same cork, with its lower extremity dipping below the level of the mercury, whilst another delivery tube with bulb and capillary orifice provides for the slow escape of the mixed gases resulting from the electro-decomposition of the water. This apparatus having been placed in the battery circuit, say of three Bunsen cells, evolves the oxyhydrogen gas with a rapidity which may be easily regulated by the size of the aperture; if, then, the activity of the battery is increased, the larger volume of gas, unable to escape, exerts a greater degree of pressure upon the liquid contents of the cylinder, and the mercury is forced up the open tube, whereby the column of liquid descends and smaller surfaces of the platinum plates are left immersed, and the power of conduction is to a corresponding extent lessened. In this manner the author states that he found no difficulty in maintaining a perfectly uniform current for a period of six or seven hours, and any required adjustment could be made either by altering the size of the apparatus or of its component parts. By collecting the gases evolved this little arrangement could also be made to serve as a voltameter.

The President, in remarking upon the ingenuity displayed in the construction of the apparatus, suggested that, whilst it would be found serviceable in electro-plating and other applications where a somewhat intense current was employed, he doubted its use in the ordinary electrotype process for the deposition of copper, where weak currents only were required.

Professor Guthrie then proceeded to read his paper "*On Graphic Formulæ*," which at the outset he stated to be founded on the same general principle as that of Dr. W. Crum Brown, but would, he conceived, "serve to illustrate the molecular constitution of compound bodies from a somewhat different perspective." The author adopts a new set of pictorial symbols by which to represent the elements themselves, and arranges them in a geometrical fashion to construct the compounds formed by their union. Thus hydrogen in combination is expressed by a single dot, the gas itself by two dots; chlorine, by a pot-hook; iodine, by a small triangle; bromine, by a cross like the sign of multiplication; fluorine, by a couple of commas. Bivalent elements, thus: oxygen, a horizontal dash; sulphur, a waved line; selenium, like sulphur, but more angular. Trivalent elements: nitrogen, a large triangle; phosphorus, similar, but with lines curved inwards. Carbon is designated by a square or four-sided figure. If then, marsh gas has to be represented, the carbon atom is shown to be saturated by placing a dot, for hydrogen, outside each face of the square. In a similar manner, with ammonia, the triangle of nitrogen has a dot standing off each face. Water is a dash with dots, for hydrogen, above and below; sulphuretted hydrogen, a waved line with two dots similarly placed; hydrobromic acid, a dot and a cross; nitrous oxide, two triangles with a horizontal dash placed between them, the whole figure being placed in a symmetrical (vertical) form; nitric oxide, a single triangle with dash below; and nitric anhydride, two triangles separated by a dash, and having all disengaged faces closed in by the oxygen dash. As yet no specific symbols are proposed for the metallic elements; but the author, later in the evening, adopted for mercury the present crossed sign for that metal. By way of conclusion, Professor Guthrie drew the figure representing triethylamine, which was shown with nitrogen (a triangle) for the nucleus, with a couple of outstanding carbon squares, appropriately dotted, opposed to each face of the triangle. The author claims for his system an increased facility in representing the satisfied and unsatisfied polarities of compound bodies.

Drs. Atkinson, Russell, and Stevenson spoke briefly, and, in a general sense, adversely, as to the desirability of introducing the system to the notice of the student. The last-named gentleman considered that the new symbols would afford little or no help in elucidating the constitution of bodies beyond the methods at present in use, and they would only be to the student something more to learn.

Dr. Odling regretted the absence of Dr. Frankland, who was so warm an advocate of the policy of introducing these pictorial methods of representation. For his own part, he looked upon them much in the light of "picture alphabets," and applicable only to those who, like the juveniles, could not be brought to book without such fascinating aid. His objection, both to this and to the system of Dr. Crum Brown, was that it required the eye of an artist to show the figures to advantage, and even then they might not be arranged properly. There were two ways, for instance, of representing the constitution of *white precipitate*, HgCl, NH_2. According to one view, the mercury was made the central atom around which the affinities were severally disposed; but if nitrogen was placed as the nucleus of the system, then we arrived at the anomalous result that chlorine was directly united with it, and mercury even with a double bond; whereas the known properties of the elements would rather point to hydrogen and mercury as those for which the chlorine had the strongest affinity. Dr. Odling humourously remarked that this difficulty could be met in a manner similar to that of a "diplomatic student" at one of the Cambridge examinations, who, when asked whether the sun moved round the earth, or otherwise, answered by saying that "sometimes it went one way and sometimes another."

At the request of the President, Dr. Guthrie sketched upon the board his mode of representing the constitution of *white precipitate;* but two efforts were required before an expression was arrived at which met with general approval.

Dr. J. H. Gladstone then read a paper "*On the Tetraphosphoric Amides.*" These compounds are produced by the action of water upon the amidated oxychlorides of phosphorus, and contain, as their name implies, four atoms of phosphorus united with the other elements in what at first sight appeared extremely complex relations. Their physical condition renders them somewhat difficult of purification—most of them being "sticky flocculent precipitates,"—and it is not to be wondered at that the analytical results are not so sharp and satisfactory as with bodies which can be purified by crystallisation. Amongst the substances described by Dr. Gladstone, are the terammoniated tetraphosphodiamic acid, $P_4N_5H_{17}O_{11}$, viewed thus:—

$$P_4(NH_2)_2(NH_4)_3HO_{11}$$

and a solid acid, to which the undermentioned name and formula apply,

Tetraphospho-tetramic acid, $P_4N_4H_{10}O_9$, viewed as

$$P_4(NH_2)_4H_2O_9.$$

Two silver salts of this acid were prepared, in one of which six atoms of the hydrogen were replaced by the metal.

The President, in reference to the highly complex character of the bodies described by Dr. Gladstone, ventured to suggest that some of those now considered to be individual substances might ultimately prove to be mixtures of simpler and more definite compounds.

Mr. W. H. Perkin saw no inconsistency in the formulæ proposed by Dr. Gladstone; these bodies were constituted on the type of Professor Wurtz's polyethylic alcohols in which the ethylene was replaced by phosphorus compounds.

A paper by Mr. J. Carter Bell was next read. It was entitled, "*On the Solubility and Crystallisation of Plumbic Chloride in Water, and in Water containing various proportions of Hydrochloric Acid.*" The author finds the degree of solubility in pure water to be somewhat greater than hitherto represented, the mean experimental result gave 1 part in 121 parts of water, instead of 135. By boiling with water there is evidence of decomposition

resulting in the formation of free hydrochloric acid with oxide of lead, or a basic salt, left in solution. The solubility of the chloride in hydrochloric acid decreases until the amount of acid reaches 15 per cent, when the curve again ascends to a maximum with the pure acid. The author concludes with some observations upon the different forms of crystals obtained by the evaporation of aqueous and hydrochloric solutions.

A vote of thanks having been passed to the authors of the above communications, the meeting was adjourned until Thursday, May 7th, when Mr. Siemens will deliver a discourse "*On the Regenerating Furnace as applied to the Production of Steel.*"

PHILOSOPHICAL SOCIETY OF GLASGOW.

CHEMICAL SECTION.

THE usual fortnightly meeting of this section was held in the Philosophical Society's Hall, on Monday evening last; Alexander Whitelaw, Esq., Treasurer, in the chair. After the election and admission of a number of new associates, a paper on "*The Estimation of Potash*," by Messrs. James Chalmers, chemist to the Kames Gunpowder Company, and Robert R. Tatlock, F.C.S., analytical chemist, Glasgow, was read to the Section. The authors urged that the subject was of much greater importance than the title would seem to indicate. Glasgow and its vicinity form the chief seat of the manufacture of potash salts from kelp, and form the destination of almost all the muriate of potash now manufactured from the interesting deposit in the vicinity of Stassfürth, as well as of the potash salts largely made from French beet-root; and as the value of these salts is fixed by the amount of potash they contain as determined by chemical analysis, it is obvious that in Glasgow, at least, an accurate and uniform method of estimating that base is of the utmost importance, as an indispensible adjunct to the manufacture and sale of potash salts. That such a desideratum had long since been supplied might well be supposed from the various methods of analysis described by eminent authorities, and from the results of the experience of many able chemists who have necessarily been much engaged in the analysis of potash salts. But unpardonable discrepancies constantly occur with regard to the results obtained by chemists of standing and experience, even when operating on the same carefully mixed and uniform sample; and these point to the conclusion that either the instructions given by those who may almost be regarded as infallible authorities have been misunderstood, and but imperfectly carried out, or, that in many instances, the details of the methods as laid down are so imperfect as to be useless, if not even misleading.

The experience of the authors of the memoir, in the analysis of potash salts, has extended over a period of many years, during which they have conjointly made thousands of potash estimations, and hence they urge that they may be fairly entitled to claim some acquaintance with the subject.

By long and careful attention to the results obtained by her chemists, confirmed by their own experience, the uthors have invariably found that the general tendency s to report potash too high, and the object of the paper was not only to trace the cause of this seemingly constant error, but also to furnish from the results of a laborious and protracted course of experiments, the true means of obviating that tendency, and obtaining not only constant, but, so to speak, absolutely correct results. In the analysis of salts not particularly rich in potash compounds, a too high result is most frequently obtained, but escapes detection under cover of the extraneous salts present,—the blame of the excess of potash reported being thrown upon the soda salts, which are not directly estimated. This error is usually discovered only when approximately pure salts are under investigation, when the analysis comes out impossibly high. It is almost superfluous to remark that any process which does not give from 99·9 to 100·15 per cent with pure salt, is totally inadmissible. The authors have both a conviction and a positive assurance that the methods adopted by chemists —at least such methods as have come under their observation— do not give results so close to the truth as those just quoted; and it is from this reason that they were induced to investigate the whole subject, and bring the results of the investigation under the notice of the Section.

They admit that such remarks may seem to involve rather strong and severe strictures on experienced analysts, and that such strictures should not be hastily made when the accuracy and definite nature of modern chemical analysis are considered; but on finding differences of from 1 to 2 per cent by different analysts, and results giving a total of 100 per cent in a muriate of potash from the potassic chloride and water alone, while soda salts, insoluble matter, and sulphate of potash—a salt having a higher equivalent than potassic chloride—were ignored, they felt warranted in saying that serious errors were made. They claim no superior sagacity in chemical matters, but simply affirm that their positions and other circumstances directed their attention to the subject, and compelled them to investigate it. The errors and conflicting results are not necessarily the consequence of careless analysis or manipulation, but are chiefly due to an unsuspected source of error in the reagent employed. That these defects in "potash analyses" have so long escaped investigation, and even general observation, is perhaps owing to an unaccountable and mistaken reliance in what is generally called a "full analysis" of a muriate or other potash salt. In many cases of full analyses of compounds, when the sum of the various determinations amounts to 100 per cent, or very nearly, the analysis is general accepted as trustworthy, and deservedly so if each of the ingredients or elements is estimated separately, and not calculated from the amount of another element; yet still, in the analysis of a commercial potash salt results approaching a total of 100 per cent give no reliable check on the accuracy of a potash determination, as there is no *practical* method of determining soda, even indirectly, in presence of potash. After enlarging on the methods of estimating soda in presence of potash, the authors considered the general subject under the following heads:—

I. The chemical principles involved in the methods employed.

II. The manipulation of the process.

III. The calculation of results.

I. The authors regard the familiar method of determining potash in presence of soda—the one now almost exclusively practised, namely, that of precipitating potash as potassio-platinic chloride—as being superior to all others; but the details of it, as laid down in some analytical works, appear to the authors too meagre for guidance to correct results; and, indeed, with the exception of the writers of a few scattered notes, they know of no chemical authors, except Fresenius, who habitually subject analytical processes to a searching examination. The directions generally given are—to evaporate the potash solution to dryness with excess of platinic chloride, and to digest the residue in alcohol before filtering. As this treatment is inapplicable to salts containing an appreciable amount of soda, most practised analysts avoid this source of error, by stopping the evaporation somewhat short of dryness, and digesting the residue in strong aqueous solution of platinic chloride, which dissolves sodium compounds, but practically leaves the potassium precipitate intact. This process is capable of giving very correct results, if all the other essential points are attended to. Of the other conditions requisite to ensure accuracy, the most important is the purity of the platinic chloride solution; indeed, the authors regard this as the key-stone of the entire process—impure

platinum and false results being as inseparably associated as crime and punishment. As pure platinic chloride solution is not the rule, but the rare exception, false results must be alarmingly numerous; and as most, if not all, of the methods usually followed in recovering platinum from spent solutions and precipitates are the means of introducing impurities that are not easily removed, the authors first briefly noticed those methods, and the objections to them, as founded on numerous trials made by themselves. The usual methods are four in number:—

1st. Reduction by nascent hydrogen produced by the action of zinc on dilute hydric sulphate.

2nd. Reduction by alcohol in presence of excess of sodic hydrate.

3rd. Reduction by cane sugar, or by glucose, in a solution strongly alkaline by sodic carbonate.

4th. Reduction by ignition of the precipitated and evaporated fluid washings in a Hessian or other crucible, as recommended by Miller, Abel, and Williams.

The authors tried all these methods most extensively, testing the platinic solution in the most rigorous way by making repeated estimations with perfectly pure potassic chloride. They used the compound sold by Griffin as chemically pure, but further purified it by successive crystallisations from distilled water. It contained the normal amount of chlorine; fluorine was sought for but not found. Solution of platinic chloride was prepared from Johnson and Matthey's spongy platinum, boiled in nitric acid, and washed before dissolving. This solution gave with pure potassic chloride results bordering on 102 per cent, using the equivalents accepted by certain practical authorities; nor could these results be brought much nearer the truth. This was certainly an alarming state of things, and showed that ordinary spongy platinum is not in a fit state for preparing pure platinic chloride.

Using the first method of recovering platinum, the authors found that if commercial zinc was employed, pure potassic chloride gave with the platinic chloride results far too high. They averaged from 101·67 to 102·05 per cent. Using purified zinc, Griffin's best, sold as being free from arsenic, and for use in Marsh's test, but giving distinct indications of the presence of cobalt, the results were nearer the truth, but still too high, being from 101·37 to 101·58 per cent. The chief objection to this method is the great difficulty of procuring zinc free from lead. On one occasion one of the authors obtained a crop of chloride of lead crystals, weighing upwards of 90 grains, from the accumulated insoluble matters left on the filters during the filtration of platinic chloride solution prepared from the metal recovered by the zinc method.

It is the second method, or rather a modification of it, which the authors have used for some time in the recovery of platinum to be used in the analysis of commercial potash samples. They render the solution of platinum waste strongly alkaline by sodic hydrate, and boil with alcohol. The resulting platinum black is further purified by boiling in dilute nitric acid and soda solution, with intermediate and final washings with water. Thus prepared, the platinum invariably gave high results. The following are examples, pure potassic chloride and recognised factors being used:—101·77 to 101·95 per cent, and 101·12 to 101·24 per cent, according to the degree of purification by the acids.

The third method, as recommended by Böttger,* did not give pure platinum. The authors used both glucose and common white cane sugar. One set of analyses gave, with pure potassic chloride, from 101·3 to 101·6 per cent. In a second set—the purifying being carried to a prolonged degree—the results were from 100·76 to 100·94 per cent. To a certain extent, the authors deemed these results to be satisfactory; but if calculated by Stas's equivalents, they gave from 101·22 to 101·4 per cent. In this series of trials, carefully made—the manipulation being unchallengeable,—there were only four cases in which fair results were obtained with pure salts, by using platinic chloride prepared from platinum which had not been previously ignited to remove organic matter.

The authors deem the fourth method to be imperfect from the great difficulty of completely decomposing the potassio-platinic chloride. Simple ignition being insufficient, ignition along with nitre was tried. In some of the experiments with platinum thus obtained the following percentages were given:—100·03, 100·11, 100·16, and 100·22; but the results were always high when using less than four drachms of water to dissolve the precipitated muriate.

Many experiments—some detailed by the authors—were performed in duplicate in order to determine the best method of reduction and purification of the platinum. But very early in their inquiry they were led to suspect other sources of error: these they also investigated, and ultimately obtained results varying from 100·08 to 100·001, which are certainly satisfactory alike to science and commerce.

II. In respect of the manipulation of the process the authors object to the method of operating on such a small quantity as 10 grs., as it gives inconstant and unreliable results. (1.) Any error of the balance is greatly multiplied in calculating to per cent. (2.) The whole of the insoluble matter is necessarily included in the weight of the potassio-platinic chloride, or, worse still, the small quantity must be dissolved and filtered, and the solution evaporated.—a most tedious and unsatisfactory mode of procedure. (3.) Electrical repulsion in dry and freshly wiped watch-glasses used in weighing, frequently causes a loss of the dried powder, and such powder is always hygrometric, and thus errors may occur in two ways. (4.) The amount dried from which the sample is taken is too small. The authors prefer to follow the advice of Fresenius and other authorities, and take, say, 500 grs., which they dissolve in a small quantity of water. They filter the solution into a 5,000 gr. flask which they fill up to the proper level with the washings and water at 60° Fah., and by means of a graduated pipette they remove for precipitation an aliquot part of the whole solution. In their paper the authors detailed the mode of performing the volumetric operations, and replied to the objections raised to the use of the pipette, and gave some details regarding actual pipette measurements. One of these was 99·97 volumes of water delivering 100 volumes of muriate solution.

III. It is not enough to master the methods of obtaining pure platinic chloride, and to manipulate the analysis of a potash salt correctly, as error would still result if wrong equivalents or incorrect factors be used for calculating the results. The authors have found that some chemists of high standing and experience in practical analysis use factors which are not only not based upon exact experiments, but give results from ·5 to ·75 per cent too high when using pure potassic chloride, and when every other step in the process is rigidly correct. They then give the equivalents of platinum, potassium, and chlorine, as used by various authorities, and regard as most trustworthy those given by Stas, at the mention of whose name in connection with combining numbers, they suggest that every good chemist should cross himself and look devout. They said they would like to know where the factors ·194 and 1·585, as used by some analysts, were obtained. Those used by the authors are ·1925 and 1·584, and are based on Stas's numbers. No others, in their opinion, will give correct results, seeing that they have been determined with every refinement of which modern science is capable.

The final conclusions arrived at by the authors are:—

1. That the methods of analysis taught and practised in some laboratories are very imperfect.

2. That the use of the factor ·194, or any others than those founded on Stas's equivalents, is erroneous, and not based on reliable experiments.

* Chemical News, vol. xi., p. 168.

3. That it is necessary to check the process used, and to be satisfied of the purity of reagents and other disturbing causes, by experiments with pure potassic chloride or other potassium salt; and that in no case should results be reported unless controlled by such experiments.

In the discussion which followed the reading of the paper, several members having much experience in potash analysis, supported the views of the authors, and highly complimented them upon the extraordinary industry displayed in the elaborate series of experiments referred to in the paper, and on the rigid care taken to avoid all sources of error. Messrs. Chalmers and Tatlock were heartily thanked for their interesting and valuable communication.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, March 31st, 1868.

Edward Schunck, Ph.D., F.R.S, &c., President, in the Chair.

"*Microscopic Examination of the Solid Particles from the Air of Manchester*," by J. B. Dancer, F.R.A.S.

The air had been washed in distilled water, and the solid matter which subsided was collected in a small stoppered bottle, and on the 13th of this month Dr. Smith requested me to examine the matter contained in this water. An illness prevented me from giving it so much attention as I could have wished.

The water containing this air-washing was first examined with a power of 50 diameters only, for the purpose of getting a general knowledge of its contents; afterwards magnifying powers varying from 120 to 1,600 diameters were employed.

During the first observations, few living organisms were noticed; but, as it afterwards proved, the germs of plant and animal life (probably in a dormant condition) were present.

I will now endeavour to describe the objects found in this matter, and begin in the order in which they appeared most abundant.

1st. *Fungoid Matter.*—Spores or sporidiæ appeared in numbers, and, to ascertain as nearly as possible the numerical proportion of these minute bodies in a single drop of the fluid, the contents of the bottle were well shaken, and then one drop was taken up with a pipette; this was spread out by compression to a circle ½ an inch in diameter. A magnifying power was then employed, which gave a field of view of an area exactly 100th of an inch in diameter, and it was found that more than 100 spores were contained in this space; consequently the average number of spores in a single drop would be 250,000. These spores varied from 10,000th to 50,000th of an inch in diameter. The peculiar molecular motion in the spores was observable for a short time, until they settled on to the bottom of the glass plate; they then became motionless.

The Mycelium of these minute fungi were similar to that of rust or mildew (as it is commonly named), such as is found on straw or decaying vegetation.

When the bottle had remained for 36 hours in a room at a temperature of 60° the quantity of fungi had visibly increased, and the delicate mycelial thread-like roots had completely entangled the fibrous objects contained in the bottle and formed them into a mass.

On the third day a number of ciliated zoospores were observed moving freely amongst the sporidiæ. I could not detect any great variety of fungi in the contents of the bottle, but I cannot presume to say that all the visible spores belonged to one species, and as there are more than 2,000 different kinds of fungi it is possible that spores of other species might be present, but not under conditions favourable for their development. Some very pretty chain-like threads of conidia were visible in some of the examinations.

The next in quantity is vegetable tissue. Some of this formed a very interesting object, with a high power, and the greater portion exhibited what is called pitted structure. The larger particles of this had evidently been partially burnt and quite brown in colour, and were from coniferous plants, showing with great distinctness the broad marginal bands surrounding the pits; others had reticulations small in diameter. They reminded me of peforated particles so abundant in some kinds of coal.

The brown or charred objects were probably particles of partially burnt wood used in lighting fires.

Along with these reticulated objects were fragments of vegetation, resembling in structure hay and straw and hay seeds, and some extremely thin and transparent tissue showing no structure. These were doubtless some portions of weather-worn vegetation. A few hairs of leaves of plants and fibres, similar in appearance to flax, were seen, and as might have been expected in this city, cotton filaments, some white, others coloured, were numerous; red and blue being the predominant colours. A few granules of starch, seen by the aid of the polariscope, and several long elliptical bodies, similar to the pollen of the lily, were noticed. After this dust from the atmosphere had been kept quiet for three or four days, animalculæ made their appearance in considerable numbers, the monads being the most numerous. Amongst these were noticed some comparatively large specimens of paramecium aurelia, in company with some very active rotiferæ; but after a few days the animal life rapidly decreased, and in twelve days no animalculæ could be detected.

Hairs of Animals.—Very few of these were noticed, with the exception of wool; of this both white and coloured specimens were mixed up along with the filaments of cotton.

After each examination as much of the drop of water as could be collected by the pipette was returned to the bottle, in order to ascertain if any new development of animal or vegetable life would take place, and the stopper of the bottle was replaced as quickly as possible to prevent the admission of the particles from the air in the room; and I am tolerably certain that the objects named in this paper are those which the bottle contained when Dr. Smith brought it to me.

The particles floating in the atmosphere will differ in character according to the season of the year, the direction of the wind, and the locality in which they are collected, and, as might be expected, are much less in quantity after rain.

The small amount of fluid now remaining in the bottle emits the peculiar odour of mildew, and at present the fungoid matter appears inactive.

For the purpose of obtaining a rough approximation of the number of spores, or germs of organic matter contained in the fluid received from Dr. Smith, I measured a quantity by the pipette, and found it contained 150 drops of the size used in each examination. Now, I have previously stated that in each drop there were about 250,000 of these spores, and as there were 150 drops, the sum total reaches the startling number of 37½ millions, and these, exclusive of other substances, were collected from 2,495 litres of the air of this city*—a quantity which would be respired in about 10 hours by a man of ordinary size when actively employed. I have to add that there was a marked absence of particles of carbon amongst the collected matter.

* Behind Dr. R. Angus Smith's laboratory.

FOREIGN SCIENCE.

PARIS, APRIL 21, 1868.

Ferments present in commercial bicarbonate of soda.—Action of saline solutions on minerals.—Detection of arsenic in cases of poisoning,—ACADEMY OF SCIENCES:—A new bank paper.—Reaction of sulphuric acid upon iodide of potassium.—Mode of development of heat and cold.

M. Le Ricque de Monchy has published a note on organised ferments which occur in commercial bicarbonate of soda. He has observed in all unfiltered, concentrated solution of this substance, that he has yet examined with the microscope, very small moving corpuscles, commonly designated molecular granulations; these vegetable cells or their germs can only come from the atmosphere, where they were in suspension, since it is not conceivable that organised matter should withstand the high temperature to which soda in manufacture is submitted. The corpuscles only appear after the manufacture, and their presence explains the production of vegetable matter in media, where one is surprised to meet with it; they are ferments, the action of which varies with the surrounding matter; in certain cases they are producers of alcohol.

M. Terreil has studied for a considerable time the action of different saline solutions on minerals with a view of discovering methods of proximate analysis. At a recent meeting of the Academy, he made known the results already obtained in this direction, the note had reference chiefly to the action of ammoniacal salts upon the natural carbonates. The carbonates of baryta, strontia, and lime are easily decomposed by solutions of ammoniacal salts, with the exception of carbonate of ammonia, which leaves them in the state of carbonates. When the acid of the salt, with the base of the carbonate, gives rise to a soluble compound, the decomposition is more rapid. Carbonate of baryta is more easily attacked than carbonate of strontia, and the latter more easily than carbonate of lime. Baryta and strontia are separated in treating the two carbonates with a mixture of chlorhydrate and chromate of ammonia; the strontia is dissolved, and the baryta remains insoluble in the form of chromate. The separation of lime from baryta and strontia is effected with sulphate of ammonia, which transforms the three carbonates into sulphates; the sulphate of lime, which is more soluble in the solution of ammoniacal salt than in water, is dissolved, while the sulphates of baryta and strontia remain insoluble. Carbonate of magnesia is rapidly attacked by ammoniacal salts, as well as by carbonate of ammonia, which dissolves it, although slowly; this property enables the separation of magnesia from the preceding bases to be effected, by treating the mixture of these carbonates by chlorhydrate and carbonate of ammonia, renewing the latter salt as fast as it is volatilised. The carbonate of manganese comports itself with ammoniacal salts like the carbonate of magnesia, rendering the separation by mere solvents a difficult matter, but by adding a few drops of sulphydrate of ammonia to the boiling solution of the carbonates in chlorhydrate of ammonia, the sulphide of manganese is precipitated almost completely. M. Terreil draws attention to the fact that when sulphydrate of ammonia is added to a solution containing besides manganese, a considerable quantity of ammoniacal salts, the sulphide of manganese is only precipitated after prolonged ebullition; his experiments have shown that of all ammoniacal salts, the oxalate is the one which most impedes the precipitation of the sulphide of manganese.

The natural carbonate of iron, under the influence of ammoniacal salts, is converted into a salt of iron; the decomposition is slower than in the case of the carbonates already referred to. Under these circumstances, the iron salt produced is in the lowest state of oxidation; for example, spathic iron ore in fine powder, boiled in a solution of chlorhydrate of ammonia, yields a colourless solution, which gives, with ferrocyanide of potassium, a white precipitate. Carbonate of zinc is soluble in all ammoniacal salts, excepting the sulphydrate, which does not dissolve this carbonate even in the presence of free ammonia or carbonate of ammonia; this character enables zinc to be detected and separated from the earths. The separation of zinc from magnesia and manganese can only be effected when phosphate of ammonia and free ammonia are present. Carbonate of lead is easily decomposed by ammoniacal salts; chlorhydrate of ammonia transforms it into chloride, which crystallises out upon cooling. Lead can be separated in this way from the earths, and from magnesia by sulphydrate of ammonia; it is separated from manganese, iron, zinc, and copper by sulphate of ammonia. The green carbonate of copper, *malachite*, and the blue carbonate, *azurite*, are dissolved by solutions of ammoniacal salts, equally in the presence of free ammonia or carbonate; azurite is attacked more rapidly than malachite.

The action of ammoniacal salts on natural carbonates may be summed up as follows:—All ammoniacal salts in solution decompose the natural carbonates, by reason of the volatility of the carbonate of ammonia, which is produced by double decomposition; the acid of the ammoniacal salt unites itself to the base of the carbonate, even when this acid forms with the base an insoluble compound. From the foregoing, one sees that by treating the natural carbonates in fine powder with warm solutions of ammoniacal salts, chosen and mixed so that the acids can form, with the bases of the carbonates, soluble and insoluble compounds, these bases may be separated, and an analysis of the natural carbonates be made. M. Terreil promises in a future communication to treat of the analysis of oxides, sulphides, arsenides, and silicates by neutral saline solutions.

M. Buchner has published some facts in connection with the detection of arsenic in cases of poisoning. M. Buchner has several times recognised the presence of sulphide of arsenic in the bodies of persons poisoned by arsenious acid. Certainly this fact has never been observed except where the corpse has been in a more or less advanced state of putrefaction; the sulphurisation would appear to be due to sulphuretted hydrogen, a constant product of putrefactive decomposition. The last observation upon this point M. Buchner has made, was upon the remains of a woman who had been poisoned eleven months previously; the large intestine was in full decomposition, and there were yellow marks upon the mucous membrane, caused by a fine powder which could be removed by washing. This powder resembled the yellow deposit which is produced in arsenical solutions by sulphuretted hydrogen; further, it gave the characteristic reactions of sulphide of arsenic. Examining now whether the arsenic had been administered as sulphide, he concluded in the negative, for the following reasons:—The contents of the stomach and small intestine being boiled with hydrochloric, and the vapours from the distillation of the acid collected in water, in a few minutes a quantity of chloride of arsenic was obtained; such would not have been the case with sulphide of arsenic, notwithstanding that this sulphide is not absolutely unacted upon by boiling concentrated hydrochloric acid. The sulphide of arsenic being insoluble in pure water and in acidulated water, it would not be carried into the circulation, also it would not be found in the liver and spleen, both of which in this particular case were saturated with arsenic. A part of the stomach and small intestine cut up and placed in the dialyser with water acidulated with hydrochloric acid, gave at the end of twenty-four hours a solution containing arsenious acid in sensible proportion, a fact proving that all the arsenic had not passed into the state of sulphide.

The following memoirs were communicated to the Academy on the 6th inst. "Researches on the combinations of molybdic acid and phosphoric acid," by M.

Debray; "Note on the manner in which sulphuric acid and iodide of potassium act when in contact," by M. Houzeau.

M. Durand read a memoir having for its title, "On the mode of development of heat and cold from a physical point of view;" this memoir was sent to the physical section.

M. Armand submitted to the Academy a new bank paper which he considered inimitable; it was sent to the chemical section. M. Debray's memoir was one of great interest, the translation has already been published in your columns.

All chemists know that iodide of potassium is immediately decomposed with liberation of iodine by ordinary sulphuric acid, but M. Houzeau has shown that an extreme degree of dilution paralyses the chemical affinities to such an extent that the dilute solutions may be boiled together without any change occurring either in the iodide or acid. It was not therefore without surprise, M. Houzeau says, that he read in a recent number of the *Comptes Rendus* a note on the pretended reaction which sulphuric acid always exerts, even in the cold, upon iodide of potassium. Even supposing that the author of this note had used extremely dilute solutions, and that he had operated on neutral iodide and on sulphuric acid deprived of nitrous compounds, his result is easily explained. The ether which served to characterise the reaction of the iodide on the acid, is precisely the reagent which should not have been employed, for this it is that provokes the reaction. M. Houzeau remarked that the confusion here between cause and effect was the less inexplicable when M. Schönbein had, several years ago, pointed out ether to be both a producer and a vehicle of oxygenated water. Thus the peroxide of hydrogen which determined the oxidation of the alkali metal of the iodide and set the iodine at liberty, in the disputed experiment was carried by the ether. Far from contradicting the exactitude of his method of determining oxygenated water, this experiment, M. Houzeau says, confirms it, and shows the great degree of sensibility of the iodide for traces of oxygenated water. He was perfectly acquainted with this perturbing cause, and to avoid it he proposed the employment of pure chloroform, which besides being more sensitive to colouration by iodine, never provokes the mutual reaction of iodide of potassium and sulphuric acid. In conclusion, M. Houzeau maintains the fact to be incontestable, that a mixture of neutral iodide of potassium and pure sulphuric acid remains unaltered in sufficiently dilute solution, and in the conditions indicated in his works on ozone and oxygenated water.

M. Brouzet addressed a note referring to a process for separating good silk-worms' eggs from bad ones. The process consists in treating first with nitrate of silver, and then submitting the eggs to a kind of sorting, by means of their very different densities in water.

CORRESPONDENCE.

PUTRESCIBLE MATTER IN WATER; SANITARY WATER TESTS.

To the Editor of the Chemical News.

Sir,—I have on my premises two supplies of water, namely, one from the Southwark Waterworks, the other from a surface well. The pipe water, when shaken in a wide-mouthed bottle, partially filled, has no appreciably unpleasant smell. When the bottle containing it is set aside in a warm place for three days and then shaken, the contents give out a very faint offensive odour, which is a proof that organic matter held in solution by it has become putrescent. When the freshly drawn pipe water is slightly tinged with a permanganate solution, it gradually loses the colour imparted by that substance, which generally is an indication that it contains organic matter in a non-putrescent condition. Being treated with a further quantity of permanganate and allowed to stand for twenty-four hours, this water, when then set aside in a warm place for three days, is found to be no longer capable of undergoing putrescence. On being shaken up in the bottle it now possesses not the slightest trace of offensive odour.

The well water, when shaken in a partially filled, wide-mouthed bottle, is also free from any appreciably offensive smell. When the bottle containing it is set aside in a warm place for three days, the contents give out a strong odour of putrescence. In this condition it decolourises permanganate rapidly, and in considerable quantities.

The freshly drawn well-water, on being treated with permanganate, decomposes that substance with rapidity, showing that it may be considered to be largely polluted with organic matter of a putrescible nature. When permanganate is added until permanence of colour is obtained, and the water at the end of four-and-twenty hours is allowed to stand sufficiently long to cause the disappearance of the colour produced by the testing solution, it is found to be no longer susceptible of undergoing putrescence. On being now shaken up in the bottle the well-water possesses quite as little trace of offensive odour as the pipe-water after treatment by permanganate. After treatment with the testing solution, neither the pipe-water nor the well-water is capable of decolourising permanganate except by prolonged contact: the less stable organic matters contained in them having been burnt up. These experiments show that both of the above methods of testing exhibit the presence in water of putrescible organic matter—the one, after a considerable lapse of time, to the nose, the other, immediately, to the eye. If it were necessary to choose between the two, several considerations, such as those alluded to by Mr. Muter, would seem to incline the choice in favour of the latter as a popular water test. But so far from this being necessary, these two methods will be found in practice to supplement each other most usefully, and when employed together to furnish results which are sufficiently exact for most sanitary purposes, and as regards the detection of putrescible matter perhaps more to be relied on than some of the refined analytical processes of modern chemistry.—I am, &c.,

H. B. Condy.

Battersea, April 21, 1868.

SCIENCE TEACHING.

To the Editor of the Chemical News.

Sir,—Some months ago you published the conclusion to lectures on chemistry delivered at Eton College, and again in No. 435 you publish the conclusion to lectures on "Heat" delivered in the same place.

If we may judge of the lectures themselves from these extracts, I would say that such lectures are not adapted to attract boys to science. I think that most of your readers will agree with me here.

At the present time, when science is so much talked about as an item of school education, I feel constrained to enter my humble protest against a system of teaching it in high places, which I consider to be calculated to bring it into disfavour.

The current of modern thought tends to the popularisation of science, and the teacher who envelopes science in too learned language is looking back towards the Sodom of the middle ages,—I am, &c.,

D.

DR. GUTHRIE'S GRAPHIC FORMULÆ.

To the Editor of the Chemical News.

SIR,—I conceive the following objections may be urged against the adoption of Professor Guthrie's system of Graphic Formulæ :—

The dot (for hydrogen) is already engaged in Berzelius' scheme to represent oxygen. Guthrie's symbol for oxygen, a horizontal dash, might be easily mistaken for the algebraical sign of minus; the cross, bromine, for the sign of multiplication; the dash and two dots, representing water, are exactly like the sign of division; and the commas for fluorine are likely to be confounded with the double dash now used to indicate the diatomicity of an element. The distinction made between nitrogen and iodine is only a variation in the size of the triangle; and the modified form of the larger triangle (with lines curved inwards instead of straight), intended to represent phosphorus, would often be confounded with nitrogen in roughly executed manuscript; and the same remark applies to the proposed distinction between sulphur and selenium.

Again, the scheme is incomplete, for we ought at least to have been supplied with symbols to represent arsenic, antimony, boron, and silicon, even if the author had not yet decided upon the proper course to be taken with the remaining metallic elements. Much might also be said on the score of want of originality, for the early chemical and alchemic works teem with graphic illustrations and codes of symbols, based upon the same general principle; thus in "Nicholson's Dictionary of Chemistry," dated 1795, there are at the end of the second volume two large folio plates showing "the characters to be made use of in chemistry," as proposed by Haffenfratz and Adet; and "Table vii.—The chemical signs as they occur in the writings of Bergman." Curiously enough, the first of these authorities adopted the horizontal dash as the symbol for oxygen; they used triangles, circles, and squares in every conceivable manner, and actually provided a series of "characters to express such new and simple substances as may hereafter be discovered."—I am, &c.,

F. C. S.

April 18th, 1868.

OZONE.

To the Editor of the Chemical News.

SIR,—The following are the more salient points in the development of ozone during the first three months of the present year :—

January 1st to morn. of 12th. Small amounts, except on the nights of the 3rd and 6th, when there was a tendency to an increasing development. Aft. of 12th—22nd, period with large amounts during the nights (9.30 p.m.—9.30 a.m.) and small amounts during the days (9.30 a.m.—9.30 p.m.) The maximum was found on the night of 18th, and large amounts on the nights of 13th, 14th, 17th, and 19th; 23rd—29th small amounts, except on aft. of 24th when a large amount was present, and on aft. of 27th when there was a tendency to an increasing development. No ozone on 23rd, aft. of 26th, and morn. of 27th. 30th—Feb. 3rd, large amounts. 3rd—16th, a variable period: the periods of increased and decreased development were short and unsettled. Large amounts on 5th and 7th, and on the nights of the 8th and 15th. No ozone aft. of 8th, morn. of 9th, throughout the 12th, morn. of 13th, aft. of 15th, and morn. of 16th. 17th—23rd, large amounts, especially during the nights.

23rd—28th, small amounts. No ozone on aft. of 23rd to 28th—March 17th, large amounts, except on aft. of 2nd, when no ozone was found, and throughout the 3rd when about the average quantity was present. From the 4th—14th there was a very well marked and settled period of ozone. 18th—morn. of 22nd, small amounts; aft. of 22nd—morn of 23rd, large amounts; aft. of 23rd—25th, small amounts. No ozone aft. of 24th and morn. of 25th. Night of 26th; large amounts, aft. of 26th—31st, a variable period with no ozone on aft. of 28th, throughout the 29th and on the morn. of 30th, and considerable amounts on afts. of 30th and 31st. Speaking generally, from the 18th—31st the development of ozone was variable.

The ozone period from the 4th—14th of March was very well marked: it occurred towards the close of a long period of equatorial wind, which shortly afterwards passed into the polar current.

During the whole of the three months the amount of ozone developed during the night (9.30. p.m.—9.30., a.m.) very considerably exceeded that developed during the day (9.30. a.m.—9.30. p.m) This excess in the amount developed by night over that developed by day was greatest in January and least in March.—I am, &c.,

R. C. C. LIPPINCOTT.

Eastbourne, April 4, 1868.

MISCELLANEOUS.

Poisoning with Oxalic Acid.—The case of poisoning at Bristol, here recorded, is remarkable in many respects:—

1st. The patient took ¾ of an ounce avoirdupois.

2nd. She died ten minutes afterwards, or very shortly after.

3rd. She vomited almost all the poisoning material, as the coats of the stomach retained by absorption only 2 gr. of the oxalic acid.

4th. There was nothing to be found in the contents of the stomach, which were merely effused blood. The stomach was intensely red, inflamed in that short period.

5th. She tested the contents of her own stomach by having vomited into a bucket of water holding a great quantity of lime in solution. "The vomited matter was like milk," when seen on the floor; and when she vomited into the bucket "it appeared to turn the water into milk."

This did not come out in the evidence, as the girl vomited into the pail in which they were in the habit of washing the glasses and cups used in the bar, and of course the landlord did not want to damage his business by giving such evidence.

The floor was wooden, not of stone, and the oxalic acid was dissolved in hot water, highly charged with lime; and it acted as an instantaneous emetic, and came up almost as it was swallowed, a milky-looking fluid, capable of precipitating a large quantity of lime. At the inquest, the following evidence was taken :—

William James Pester deposed: The deceased, Sarah Salmon, was in my employ as barmaid and housekeeper for nearly twelve months. On Saturday evening she died. I saw her at 20 minutes to seven o'clock, and she then appeared to be quite well. Shortly afterwards she became very sick, and continued so until her death. I went out shortly after half-past six o'clock, and came back about seven o'clock, and I then found her very sick, but still sensible. I asked what was the matter with her, but she did not reply. She was assisted upstairs by the servant, and lay down on the bed. She died shortly afterwards, within ten minutes of her going upstairs.

I sent her up a glass of brandy and water. I sent for Mr. Fardon, and afterwards for Dr. Herapath. She seemed rather peculiar and excited during the whole o Saturday, especially in the afternoon. Her conduct was very different to her ordinary demeanour, and attracted my attention, she being generally very reserved in her manner. She scarcely spoke half a dozen words to me

during the day, and then only when I asked her a question. She stood in the bar leaning against the counter during the afternoon with her arms folded. She would not wait upon the parlour customers, and she would not move out of the way when I wished to enter the bar until I asked her to do so. No one saw her take anything.

Emma Thomas stated: I am in the service of Mr. Pester as general servant. On Saturday, about 7 o'clock in the evening, I noticed that she was very sick in the bar. I helped her upstairs, and she lay on the bed, the sickness still continuing. She died in about 10 or 15 minutes after I got her upstairs. When I first saw her there was a half-pint cup turned upside down in a tray in the bar. It had been recently washed in cold water. No one else could have washed it but the deceased.

Dr. Herapath asked if there was anything peculiar in the vomited matter. Mr. Pester stated that it presented a white, milky appearance—just like lime water.

William Brass: About half-past six o'clock I met Mr. Pester's servant, Emma Thomas, in Castle Street. She asked me if I would get three-pennyworth of oxalic acid for her at the druggist's shop. I complied with her request. I gave it to the girl Thomas, and she delivered it, in my presence, to Miss Salmon.

Dr. Herapath, F.R.S.: I was called in to the deceased a little after seven o'clock. I arrived about half-past seven o'clock, and she was then dead, and had been dead some fifteen minutes. I was told that she had been vomiting, and vomited matter was shown me. The vomit was a very remarkable one—mucus, with curdled, dark stuff—which led me to suspect altered blood. There did not appear to be any particles of food in the vomit. I have since examined the vomit, and I have found oxalic acid in small quantities in it. I have obtained crystals from the vomit, so small, however, that it required the microscope to discover them. I have examined the stomach. It presented an intensely blood-red appearance, and the fluid contained in it was blackened, curdled, altered blood. There were some very small white patches existing on the stomach, and the vessels were braced out and darkened as if by hardened blood. The blood was so altered in character that it was in fact insoluble. If the oxalic acid had been taken in water strongly impregnated with lime salts the white appearance presented by the vomit would be accounted for, as the lime would be precipitated as oxalate of lime. I have never seen an instance of poisoning by oxalic acid before, but I have experimented upon animals. It is a very rapid case of poisoning—one of the most rapid on record. I have no doubt that she was poisoned by oxalic acid. The deep colour of the stomach was caused by the intense irritation, and next by the exudation of blood. The precipitation, darkening, and curdling of the blood are the first symptoms produced by this poison. One-sixth of the quantity taken in this case is recorded as having killed a person, and the shortest time on record is eight minutes. The time in this case I should think was about 15 or 20 minutes.

Mr. Pester: No, I don't think it was so long as that; not more than ten minutes. Mr. Pester also stated that the water drawn from his well and used in his house contained lime in large quantities.

Dr. Herapath: I have not analysed the contents of the stomach. She died unquestionably from collapse, which would be produced by oxalic acid.

Dr. Herapath remarked that the deceased, in his opinion, suffered from impulsive insanity.

The jury returned a verdict of "Temporary insanity."

Discovery of a Silver Lode.—A lode has been discovered on the banks of the Don, in Tasmania, yielding cobalt, silver, copper, and antimony; an analysis, giving the result was of cobalt, 4 oz. to the ton silver, 100 oz. to the ton; and copper, 14 per cent.—*Express.*

CONTEMPORARY SCIENTIFIC PRESS.

Under this heading it is intended to give the titles of all the chemical papers which are published in the principal scientific periodicals of the Continent. Articles which are merely reprints or abstracts of papers already noticed will be omitted. Abstracts of the more important papers here announced will appear in future numbers of the "Chemical News.")

Comptes Rendus.
December 30, 1867.

F. Pisani, "On Woodwardite from Cornwall." E. Bourgoin, "On the Electrolysis of Tartaric Acid."

Poggendorff's *Annalen der Physik.*
December, 1867.

R. Weber, "On some Compounds of Bichloride of Titanium."

Journal für Praktische Chemie.
December, 1867.

E. Salkoski, "On the Estimation of Hippuric Acid as Hippurate of Iron." H. Laspeyres, "On the Composition of Prehnite." T. S. Hunt, "On J. D. Whelpley and J. J. Storer's New Method of Treating Ores." J. H. Gladstone, "On Pyrophosphoric Acid." H. Rittthausen, "On the Use of a Solution of Sulphate of Copper and Potash as a Test for Protein Compounds." C. Bigott and R. Fittig, "On the Synthesis of some New Hydrocarbons."

Annales de Chimie et de Physique.
December, 1867.

Boussingault, "On the Decomposition of certain Sulphates at a High Temperature."

Le Technologiste.
October, 1867.

E. Beanes, "On the Use of Ozone for Whitening Sugar, Syrup and Molasses."

Comptes Rendus.
January 6, 1868.

A. Houzeau, "On the Estimation of Minute Quantities of Peroxide of Hydrogen." H. Gal, "On the Action of Chloride of Cyanogen on Zinc-Ethyl."

January 13, 1868.

A. Rommier, "On Xylindeine, a New Colouring Matter Extracted from Decayed Wood."

January 20, 1868.

A. W. Hofmann, "On the Compounds Isomeric with the Sulphocyanic Ethers; 1. Oil of Mustard of the Ethylic Series."

Monatsbericht der Koniglich Preussischen Akademie der Wissenschaften zu Berlin.
September and October, 1867.

Hofmann, "On a new Series of Isomers of the Nitriles." "Contributions to the Knowledge of Methyl-aldehyde."

Poggendorff's *Annalen der Physik.*
December, 1867.

C. Rammelsberg, "On the Phosphites."

Annalen der Chemie und Pharmacie.
December, 1867.

F. Beilstein, "On Xylol and its Derivatives." H. Y. de Schepper, "On Sulphydrate of Xylil (Sulphoxenol)." A. Vollrath, "On Chloride of Xylol, Chloride of Tolyl, and their Derivatives." W. Holleman, "On Dichloride of Xylol and Trichloride of Xylol." F. Beilstein, "On Nitroxylol and its Derivatives." G. Deumelandt, "On Xylidine and its Derivatives." E. Luhmann, "On Dinitroxylol and Trinitroxylol." R. Fittig and J. Konig, "On Ethylbenzol and Diethylbenzol." G. Stadeler, "On the Constitution of Phenyl-sulphuric Acid." H. Limpricht, "On the Amines of Benzyl Alcohol." H. Wichelhaus, "On the Constitution of Organic Acids containing Three Atoms of Carbon."

January, 1868.

A. Grabowski, "On the Tannic Acid of Oak Bark." R. Otto and O. von Gruber, "On Toluol-Sulphurous Acid." "On the Estimation of Sulphur in Organic Substances by means of Chromate of Copper." R. Otto, "On Bichlorsulphobenzide." E. Linnemann, "On the Preparation of the Fatty Alcohols from their Primary Members." "On Artificial Methyl Alcohol." A. Siersch, "On the Preparation of the Fatty Alcohols from their Primary Members." "On the Transformation of Methyl Alcohol into Ethyl Alcohol." W. Heintz, "On the most Simple Process for Preparing Glycolamidic Acids from Monochloracetic Acid." L. Scheffer, "On Obtaining a Double Phosphate of Zinc and Soda by Fusion." A. von Flemming, "Note on Sulphochloride of Phosphorus." H. Schweikert, "Note on Phosphate of Soda and Ammonia, and on the Separation of Phosphoric Acid from Oxide of Zinc." K. Birnbaum, "On the Combinations of Ethylene and its Homologues with Bichloride of Platinum." A. Geibel and H. L. Buff, "On a Hydrocarbon analogous to Ethylene, obtained from Chloride of Hexylidene."

Annales des Mines.
1867.

W. Eggertz, "Note on the Estimation of Sulphur in Iron and Iron Ores."

Comptes Rendus.
January 27, 1868.

J. Reiset, "Chemical Researches on the Respiration of Cattle." (Continuation.) "On the Influence of Food on the Respiration of Cattle." "A Comparative Study of the Respiration of Calves fed on Milk, and Calves at Grass." "On the Nature of the Gases of the Hoove in Cattle, and on a Remedy for the same." "Note on the Production of Nitric Oxide during the Fermentation of Beet Juice." "On the Estimation of Ammonia in Beet Juice." L. Marignac, "On the Reduction of Niobium and Tantalum Compounds." H. Deville, "On the Extraction of Niobium." H. Debray, "Researches on Dissociation."

February 3, 1868.

T. SCHLŒSING, "On the Decomposition of Nitrates during Fermentation."

February 10, 1868.

E. BECQUEREL, "Fourth Memoir on some newly discovered Electro-Chemical Effects of Capillary Action." DUBRUNFAUT, "Memoir on a Nitrogenous Substance possessing greater Activity than Diastase contained in Malt, and on the Preparation and Application of the same in Manufactures." "On the Distillation of Beetroot, and on the Formation of Nitric Oxide during the Fermentation of the same." A. CHAUVEAU, "On the Nature of Vaccine Virus.—Experimental Determination of the Elements which constitute the Active Principle of the Virus of Small Pox."

Annalen der Chemie und Pharmacie.

Supplement.

J. VON LIEBIG, "On some Methods of Silvering Glass." W. VON SCHNEIDER, "A Method of Obtaining Pure Platinum and Iridium." H. KOPP, "On the Boiling Points of the Hydrocarbons." H. SCHIFF, "On Aldehyde Bases." E. ERLENMEYER, "On the relative Constitution of the Butylic and Amylic Alcohol of Fermentation."

Bulletin de la Société Chimique de Paris.

December, 1867.

BERTHELOT, "On a new Thermometer for Measuring High Temperatures." A. SCHEURER-KESTNER, "On a Crystallised Stannate of Sodium." BERTHELOT, "On Alcoholate of Baryta." "On the Oxidation of Organic Acids." J. B. GRANGE, "On the Quantity of Urea contained in the Urine of Persons Suffering from Chlorosis." GONDOLO, "A Modification of Boussingault's Process for Manufacturing Oxygen and Nitrogen by passing a Current of Atmospheric Air over Caustic Baryta." BOTHE, "On Tessié du Mothay and O. Maréchal's Process for the Manufacture of Oxygen." JACQUEMART, "A new Process for the Manufacture of Sulphite of Alumina." A. GIRARD, "A Process for the Manufacture of White Lead." JUETTE and DE PONTEVES, "A Process for the Manufacture of Tartaric Acid from Refuse Grape Skins." DUSART, "A Method of Preparing Essence of Bitter Almonds for Use in Perfumery." P. SCHUTZENBERGER, "A Method of Remaking Waste Paper." PERIER, POSSOZ, CAIL, and Co., "An Improved Method of Using Lime for the Preservation of Saccharine

NOTES AND QUERIES.

Extract of Madder.—Can any of your readers inform me how the extract of madder which is now being used, is made.—W. B.

Estimation of the Sulphur Acids.—Can any reader suggest a method by which NaS, NaO,SO_2 and NaO,S_2O_2, can be detected and estimated separately in ordinary commercial "soda-ash."—W. BLACKRIL.

Unit of Momentum.—I will be much obliged if any one will inform me what the value of the unit of "momentum" is in avoirdupois weight. Our books on Mechanics tell us that momentum, or "quantity of motion," is equal to the product of the quantity of matter into the velocity, thus, M = QV. Now, suppose 1 lb. of matter moves 1 foot per sec., what is its moving force or "momentum" in avoirdupois ounces?—J. R. B., Davidson College, N.C.

Information Wanted.—A correspondent at Halifax, Nova Scotia, wishes to be put in communication with parties who would give reliable information—1. On the best ways of utilising the waste in iron-works, arising from the operations of planing, turning, boring, &c. 2. On the best way of manufacturing chloride of lime. 3. On the best way of manufacturing paper pulp from wood.

Palm Oil for Softening Dyed Yarns.—Palm oil and other oils may be made to mix with water without the use of alkalies, by subjugating, or incorporating with the oil albuminous substances, best yolk of eggs, along with a small quantity of glycerine, and sometimes a solution of gum may be useful; in this way a magma is obtained which may be gradually mixed with water, and will form what is called an emulsion, in fact a mixture of water and oil, or better and more correctly, oil minutely divided through water by the aid of albumen. It is a well known fact that the yolk of eggs contains a large quantity of oil in its natural state, to which its colour is due, and which oil undoubtedly also assists the forming of an emulsion.—Dr. A. A.

Estimation of Chlorine.—Mr. Dunn will undoubtedly find in the manifold published works on Analytical Chemistry, Guy Lussac's method, as also those of Mohr and Wagner, and perhaps he may try if he likes Runge's method, which is not so generally known, and is executed in the following manner:—Two grammes of the bleaching powder to be tested are well mixed with water, and the fluid so obtained mixed with a solution of protochloride of iron freshly made by dissolving 0·6 grm. of pure iron wire in pure hydrochloric acid, next pure hydrochloric acid in excess is added, and the fluid boiled in a flask, after previous addition of a piece of rather thick, perfectly clean, and brightly polished sheet copper, of a weight of about 4 grammes: the boiling is continued until the at first darkish colour of the fluid has become bright green; the copper is then removed from the flask, washed with distilled water, dried, and weighed. A loss in the weight of copper of 63·4 = 2Cu, is equal to 35·5 chlorine in the bleaching powder. This method is based on the fact, that under the conditions described, the chlorine of the bleaching powder first changes the protochloride of iron into perchloride, which in its turn is again reduced to protochloride by the metallic copper, whereby some of the latter becomes dissolved; for every 2 equivalents of copper dissolved in this way there is 1 equivalent of chlorine in the bleaching powder. —Dr. A. A.

MEETINGS FOR THE WEEK.

MONDAY.—Royal Geographical, 8½.
—— Medical, 8.
—— Philosophical Club. Anniversary Meeting, 6.
TUESDAY.—Royal Institution, Dr. M. Foster, "On the Development of Animals," 3.
WEDNESDAY.—Society of Arts, 8.
—— London Institution. Anniversary Meeting, 12.
THURSDAY.—Royal, 8½.
—— Royal Institution. Professor Odling, "On Chemical Combination," 3.
—— Zoological. Anniversary Meeting, 1.
—— Royal Society Club, 6.
FRIDAY.—Royal Institution, 2. Annual Meeting. F. T. Palgrave, Esq., "How to form Good Taste in Art," 8.
SATURDAY.—Royal Institution. Professor Odling, "On Chemical Combination," 3.

Patent Gas Producing Mixture.—It having come to the Subscribers' knowledge that some individuals in Britain have exported to Paris, and other Continental towns, quantities of a GAS-PRODUCING MIXTURE, known in Britain as "M'KENZIE'S OR HAMILTON'S PATENT," the Subscriber, Mr. B. Hugo Dullens, who holds Patents for this invention for France, Belgium, Holland, Italy, Austria, Denmark, and other Continental Countries, as well as the United States, hereby warns all parties against exporting any of said Patented Material, certifying those who shall do so, that not only will the Material be confiscated, but he will proceed against them for such penalties and damages as the Law may award to him.

B. HUGO DULLENS, Bieberich, Nassau.
JAMES Y. PULLAR, I.S.C,
17, Broughton Place, Edinburgh,
Agent for Mr. Dullens.

Pharmaceutical Society of Great Britain.—SCHOOL of PHARMACY.—SESSION from OCTOBER to JULY.

LECTURES.—On Chemistry and Pharmacy, by Professor Redwood. On Botany and Materia Medica, by Professor Bentley.

LABORATORY for Practical Instruction in General and Pharmaceutical Chemistry.—Director, Dr. Attfield; Assistant, Mr. Tilden.

A syllabus of Instruction and particulars of the Examinations may be obtained from the Secretary, 17, Bloomsbury Square, W.C.

The Scientific Wonder.—This Instrument has a clear magnifying power of 32,000 times, shows all kinds of Animalculæ in water, Circulation of the Blood, &c., &c., Adulteration of Food, Milk, &c., and is just the microscope that every Surgeon, Dentist, Schoolmaster, Student, and Working Man should have. It is pronounced by the Press (and all scientific men who have seen it) to be the best, cheapest, and most simple Microscope ever invented. It has twenty times the power of the Coddington or Stanhope Microscope, and is twice as good as the celebrated Rae Microscope (which has been awarded so many prize medals), as may be inferred from the following letter, received from Mr. Rae himself:—

Carlisle, December 12th, 1867.

To Mr. McCulloch, Philosophical Instrument Maker.

SIR,—Having seen some of your Diamond-Plate Lenses, I write to ask your terms for supplying me with the same per 20 gross, as I consider them superior to mine.—Yours, &c.,

RAE & Co., Opticians, Carlisle.

I beg to inform the Public that I have no Agents anywhere, and all pretended Agents are Impostors. The above Instrument can only be had from me, in Birmingham. Those at a distance who care for instruction and amusement, can have it safe and free by sample post, with book of full instructions, on receipt of 32 Postage Stamps.

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MEDAL PARIS EXHIBITION, 1867.

Methylated Spirits.—David Smith Kidd, Licensed Maker, Commercial Street, Shoreditch, N.E
Also, FINISH, FUSEL OIL, and RECT. NAPHTHA.

THE CHEMICAL NEWS.

Vol. XVII. No. 439.

ON THE ESTIMATION OF SULPHUR IN IRON AND ITS MINERALS.*

By M. W. EGGERTZ,
Professor at the Fahlun Mining School.

As small quantities of sulphur, even a few ten-thousandths, are sufficient to render iron, even of excellent quality, red-short and unfit for certain uses, it would be very useful to be able to ascertain the quantity of sulphur which exists in the iron, not only with the necessary scientific exactness but also with a facility and accuracy suitable for practical wants. But as the methods ordinarily employed for this purpose, especially on the score of facility, leave much to be desired, I have been engaged for several years in improving the known processes, and I wish to communicate in these pages the results of my researches. I have found in this difficult work, especially in the estimation of sulphur in iron analytically, useful aid on the part of M. J. E. Sieurin, and some other students of the Mining School, but I must especially mention the valuable services of M. J. F. Lundberg.

A. *Detection of sulphur by means of chloride of barium.*

1. Iron.—5 grammes of iron passed through a sieve with apertures of 0·6 m.m., are added to a solution of 10 grammes of pure chlorate of potash, free from sulphate, in 200 cubic centimetres of distilled water, and contained in a flask of a capacity of 500 c.c. Cover this flask with a small funnel and heat the contents on a sand-bath to complete ebullition; then add gradually 60 c.c. of hydrochloric acid of the specific gravity 1·12.

It is best to add the hydrochloric acid at first drop by drop from a burette, as otherwise a strong disengagement of gas would take place; but in proportion as this evolution diminishes larger quantities of acid may be added; ordinarily half an hour is required for this operation, and during this time the solution ought to be kept in complete ebullition, so that no sulphuretted hydrogen gas escapes. Then allow the liquid to boil gently for five or six minutes more. In this way the carbon and part of the silicic acid remain insoluble, and often also a brown flocculent or pulverulent substance resembling hydrated oxide of iron, which is composed of a geic product analogous to humus, which is formed from the carbide of iron. During the solution a little sulphur is also sometimes separated; it swims on the surface. To oxidise this sulphur and drive off the chlorine and hydrochloric acid which are present in the solution, it must be evaporated to dryness in a water-bath; the sides of the flask and the small funnel which covers it should always be first well washed with a jet of distilled water.

If there is much sulphur on the surface the moment it has disappeared the funnel should be replaced by a paper cover. When the solution has become syrupy the desiccation should be hastened by stirring it well with a glass rod. Add to the dry mass 10 c.c. of hydrochloric acid and 30 c.c. of water, and then leave the mixture on the water-bath until all the crystals of chloride of iron are dissolved, then add about 20 c.c. more water, and collect the insoluble portion on a filter, washing it perfectly with warm water. It sometimes happens that during the washing the organic matters remaining on the filter dissolve and precipitate again in contact with the acid solution, but they are easily redissolved on boiling the liquid. The last washing water ought to have no acid reaction; evaporated and heated to redness on platinum foil it should leave no trace of residue.

* *Moniteur Scientifique.*

The filtered liquid, whose volume is about 50 c.c., should be rapidly boiled and mixed with 2 c.c. of a saturated solution of chloride of barium. (This amount is sufficient to precipitate the sulphuric acid formed from 0·1 grm. of sulphur). After cooling add to the mixture 5 c.c. of ammonia sp. gr. 0·95, then stir well with a glass rod, and leave the whole to rest at the ordinary temperature for twenty-four hours. The clear solution should be decanted as completely as possible on to a strong filter, and the precipitate stirred up with about 20 c.c. of cold water, and then left to itself until it has quite settled. If warm water is used without having added a few drops of hydrochloric acid, a little oxide of iron will precipitate. The clear liquid is likewise thrown on to the filter, and the operation is repeated several times with cold water, and then two or three times with boiling water, without which precaution the sulphate of baryta will pass through the filter. Finally collect the precipitate, and wash it carefully with warm water. The last drops of this water on being evaporated on a watch-glass ought not to leave more than a scarcely visible white ring. The precipitate is then dried, heated to redness, and weighed. If it is coloured with oxide of iron it must be washed with a little hydrochloric acid, dried in the water-bath, and taken up with a few drops of acid and water, and then the preceding operations repeated (washing, drying, heating, and weighing). If the precipitate has only a feeble red colour, which is often the case, this latter operation will be unnecessary.

100 parts of sulphate of baryta contain 34·3 parts of sulphuric acid, or 13·72 parts of sulphur. If in this experiment 5 grms. of iron have been used, each 0·001 grm. of sulphate of baryta will correspond to 0·00274 per cent of sulphur in the iron.

ON THE MANUFACTURE OF GLASS.

By HENRY CHANCE, M.A.

Abstract of a Lecture delivered at the Chemical Society, March 19th, 1868.

Referring to the raw materials of which glass is made, the various qualities of sand were in the first place described. The American sand is considered the finest of all; then that from Fontainebleau; Belgian next; and, of English samples tolerably free from iron, the deposits occurring at Leighton Buzzard were in great demand. This quality is, indeed, almost exclusively employed by the Messrs. Chance, and, though of a yellowish tint, contains less iron than many whiter samples. Sand of a strong reddish hue may be made nearly white by calcining it with a small quantity of common salt. The Welsh sand is not held of much account, for it seems to produce glass in which striæ abound.

With respect to the alkaline ingredients for glass making the use of kelp, soda of alicant, and Egyptian natron were briefly noticed, and the influence of Le Blanc's discovery traced. The substitution of the sulphate for the carbonate of soda—due to Gehlen—tended to cheapen the glass, but at the sacrifice of quality; for the best kinds of plate glass the carbonate (soda ash) is always employed. For the common blown window glass the sulphate answers well: it will permit the use of larger charges of lime, and the glass produced from it is harder, takes a better polish, is less liable to devitrification, and to the alteration of its surface by what is termed "sweating." The addition of carbon in the form of charcoal, or powdered anthracite, facilitates the reduction of the sulphate, and tends to promote vitrification. Only one atom of carbon is, in practice, found sufficient for two atoms of sulphate of soda. The object of its employment would, then, seem to consist in furnishing the means of reduction of a part only of the

alkaline sulphate to the state of sulphite, and not to sulphide; the decomposition of the sulphate is further facilitated by a liberal use of carbonate of lime, the action of which is not clearly intelligible. Glass made from sulphate of soda is of a bluish, whilst that made from the carbonate is of a yellowish tint. When an extra pale colour is desired, the carbon is kept at a minimum, and the lecturer once saw, at glass works in the north of France, an expedient practised in this direction, which consisted in employing the sulphate in large excess, and ladling off the upper layer of this fused salt whilst the pots stood in the furnace. Should any of this ingredient be accidentally left in the crucible, "salt-blisters" are formed.

The lime required for the crucible charges is introduced in the form of quick-lime when carbonate of soda glass is made; but for the other kind either chalk or limestone, preference being given to the latter, probably on account of its containing some carbonate of magnesia. This addition of lime is in flint glass replaced by oxide of lead, giving great lustre and refracting power, but if made into sheet would be full of striæ. Pipe clay and alumina are sometimes used in glass making, but are not supposed to improve the quality; all glass fused in clay pots must necessarily contain more or less alumina, and it has been noticed that the outer portions next the crucible often show irregularities, and occasionally some coarse gritty particles become detatched from the inner surface by corrosion of the finer clay under the action of the fused alkali. Chloride of sodium excepted, no appreciable amount of alkali is lost by volatilisation in the furnace; at any rate, the deficiency from all sources of waste, including the mixing, decrepitation on first heating, &c., never exceeds 1 per cent.

The average composition of different qualities of glass was stated to be as follows:—

Crown and Sheet Glass.

	English.	Foreign.
Silica	73	74
Lime	13	14
Soda	13	11
Alumina, oxides of iron, and manganese	1	1
	100	100

Ancient Glass.

	12th century.	16th century.
Silica	51·60	54·60
Alumina	2·16	8·96
Protoxide of iron	1·58	0·75
Lime	8·04	19·31
Magnesia	2·22	3·43
Alkalies	34·40	12·95
	100·00	100·00

The use of peroxide of manganese and arsenious acid in glass making is mainly for the purpose of effecting the peroxidation of the iron which in this state has far less colouring property. The author here pointed out as an anomaly the circumstance of carbon and metallic oxides being employed in the same mixture, but an attempt to defer the addition of the latter until the carbon had done its work was only partially successful, since it is difficult in the last stages to ensure a proper incorporation of the materials. The Belgian manufacturers are said to have discontinued the use of the above metallic oxides. The purest coloured flint glass is composed of sand, potash, and oxide of lead. For some kinds of optical glass a portion of the red lead is replaced by lime, and if the lead is used in excess the heavy flint glass produced has a strong yellowish tint. When much manganese is employed to correct the colour arising from impurities in the glass mixture, there is a tendency for the glass to undergo changes of colour upon exposure to sunlight; and a greenhouse roofed with glass in which manganese has been used will often display, after a lapse of time, a great variety of tints. Reference was then made to Mr. Gaffield's experiments on the action of sunlight upon glass, and to the practical conclusion arrived at, to the effect that the alteration in colour was solely due to the different states of oxidation of the manganese. The lecturer further asserted that he had noticed changes of colour in glass which did not contain a trace of this metallic oxide, and specimens were exhibited in which the glass, originally white, had become strongly tinged with yellow. In connection with the employment of Siemen's regenerating furnaces at his works, Mr. Chance stated the following particulars. The cubical capacity of the glass furnace was about 1800 feet (or 20 feet long, 10 broad, and 9 feet high). This chamber accommodated eight large clay melting-pots, each of which was capable of producing two tons of refined glass in five and twenty hours. The saving on Siemen's plan was, firstly, the difference in cost between large coal and slack; and, secondly, a more cleanly manipulation, whereby a superior product was obtained in open pots. By way of conclusion the lecturer described Gossage's process for the manufacture of soluble glass, and the results of his own experiments on the fusion and devitrification of the basaltic rock known as "Rowley Rag." A large slab of the fused material, which is similar to black marble, and other specimens of the same devitrified, were exhibited.

TEST FOR BROMIDES.

By Surgeon J. H. BILL, U.S. Army.

In conducting some investigations during the past summer, on the physiological relations of the halogens, it became absolutely necessary in the course of the experiments, to devise a ready and sensitive test for bromine in the presence more particularly of chlorine.

Any analyst will bear out the assertion—although the books make light of the matter—that it is a difficult thing to recognise traces of bromine in the presence of excess of the other halogens. Thus in the case stated above, it was found impossible to obtain by the ordinary methods of the books, a certain and easy recognition of bromine when chlorine was present.

The Fresenius test solution of auric chloride produces in faintly acid solutions of alkaline bromides, a colouration ranging from dark orange red to light straw colour, according to the strength of the solution. Iodides must be out of the way. Chlorides, however, do not interfere in the least. The following is the best way of applying the test:—Separate iodides by palladium, and after getting rid of excess of palladium by sulphuretted hydrogen, concentrate the solution to about twenty-five cubic centimetres. Select two test tubes, of the same size and shape, and colour of glass. Into one pour the solution suspected to contain bromide. Into the other pour pure water, adding perhaps a trace of chloride potassium; add now to each test tube a drop of chlorhydric acid, and then to each *one* drop of auric chloride solution. On now comparing the two tubes particularly in the direction of their long axes, a yellow colour will be observed in the tube containing the bromide, and made very manifest by comparison with the other tube.

The following experiment shows the delicacy of the test applied as above:—One centigramme of potassic bromide was dissolved in one thousand cubic centimetres of water. Thirty centimetres of this solution, compared with thirty centimetres of a very weak solution of potassic chloride, gave a decided yellow colour. This experiment was varied by dissolving a gramme of potassic chloride in two thousand cubic centimetres of water, halving, and adding one centigramme of potassic bromide to the one half. Thirty centimetres of each of the two solutions now tested, gave ample evidence of the presence of bromide.

The mixed chloride and bromide should be brought to the state of salts of the alkalies if necessary, by precipitating with argentic nitrate, thoroughly washing, and fusing with potassic carbonate. If sodic carbonate is used, the subsequent reaction with the gold test is not so decided.

A test for chloride in the presence of bromide, as simple and delicate as the above, is much needed. The writer has sought long for it but in vain.—*Am. Journ. Sci., March*, 1868.

ON THE CHEMISTRY OF THE BESSEMER PROCESS.*

Analyses representing each successive step in the Bessemer process do not appear, as yet, to have been published. The following analyses of the raw material and the various products at different stages of this process were sent with the manufactured objects to the late Paris Exhibition from the Bessemer Works of Neuberg, in Austria. In the absence of more complete analyses, they may be of some value.

The charge was a good dark-grey iron, weighing 62 cwts. 80 lbs. (Austrian); it was transferred directly from the blast furnace into the converter. Blast was applied for 28 minutes under a pressure of 20 lbs. to the inch; at the close of this first period a sample of the iron was taken. During the second period, of 7 minutes, the pressure of the blast was 18 to 19 lbs. per square inch; the third period lasted only 3 minutes, under about the same blast. The amount of slag was a little larger than usual, probably because of the taking of samples from the mass. The analyses of the raw-iron and of the samples taken at the close of the above period, gave the following results:

1. The iron taken was dark-grey, graphitic, containing considerable silicon, very little phosphorus and sulphur, and much manganese; in every respect an excellent material for the Bessemer process. A small amount of copper was present, but not enough to either hinder the process or deteriorate the product.

2. At the close of the first period spoken of, all graphite had disappeared, partly by combustion, partly by combination with the iron; almost four-fifths of the silicon had been separated; all but a trace of sulphur had disappeared; the amount of phosphorus remained nearly the same; also the total amount of the copper, while its percentage was a little higher; much of the manganese was lost. The product at the close of this period was a pure white raw-iron, containing not overmuch of carbon.

3. During the second period the removal of the carbon progresses rapidly, so also the still remaining silicon and manganese are rapidly disappearing, while again the copper and phosphorus remain almost the same. The product at the close of this period of only about seven minutes was a good steel; according to the common scale, steel No. 3.

4. At the close of the third period, a steel No. 7 was obtained. The addition of 6 cwts. raw iron gave a Bessemer steel No. 6.

The slags obtained at the various stages were also analysed; they always contained a great relative amount of silica, but, both before and after the second (or "boiling") period, remarkably little of ferrous oxide. During the last stages of the process, the percentage of manganese in the slag decreases, because most of the manganese is removed in the first period, so that the increase of slag during the last stages of the process can only add iron to it, *i.e.*, reduce the percentage of the manganese. A little alumina and lime found in the slag is ascribed to the walls of the furnace.

* "*Aus der Natur*," 1867, p. 714, &c.

From the composition of the raw material and the final product, the amount of the various elements removed during the process is obtained by the difference of the first two quantities. The amount of oxygen necessary for this removal may easily be calculated on the supposition that the silicon is converted into silica, carbon into carbonic acid (CO), phosphorus to phosphoric acid (anhydride), sulphur to SO_2 or SO_3; iron to magnetic oxide (Fe_3O_4), of which but a small amount is found in the slag, the greatest portion being blown out in the shape of a red smoke (Fe_2O_3). The following table contains the results thus obtained:

	Raw iron taken. lbs.	Bessemer steel obtained. lbs.	Removed. lbs.	Requiring Oxygen. lbs.
Carbon ..	258·59	12·79	245·80	327·73
Silicon ..	128·97	1·80	127·17	139·07
Phosphorus	2·63	2·40	0·23	0·29
Sulphur ..	1·13	—	1·13	1·69
Manganese	227·67	7·59	220·08	63·56
Copper ..	5·59	5·59	—	—
Iron (by difference) ..	5956·42	5429·82	525·59	200·22
Total	6580·00	5460·00	1120·00	732·56

	Producing lbs. of	
Carbon	573·53	CO
Silicon	266·24	SiO_2
Phosphorus	0·52	P_2O_5
Sulphur	2·82	SO_3
Manganese	283·64	MnO
Copper	—	
Iron (by difference)	725·81	Fe_3O_4

This amount of oxygen can be had in 43,330 cubic feet of atmospheric air, or 1,140 cubic feet per minute, or 660 cubic feet per cwt. of the charge.

The supposition that the carbon burns to carbonic oxide (CO) and not to carbonic acid (CO_2) seems to be proved by the spectroscopic investigations of Lielegg.

GRAPHITE IN CALIFORNIA.

The principal plumbago deposit of California, known as the Eureka Black Lead Mine, is situated on the west side of Tennessee Gulch, a tributary of Wood's Creek, about 1½ miles from Senora, the county seat of Tuolmune county, and about 68 miles from Stockton, the head of navigation on the San Joaquin River. The mineral exists as a well-defined lode, from 20 to 30 ft. wide, having a dioritic foot-wall on the west, and a soft clay-slate hanging wall on the east. The trend of this lode is nearly north-east by south-west, and it dips very irregularly to the east, at some places being nearly vertical, and at others nearly horizontal. It has been traced and explored for 3,900 ft., beyond which there are but faint indications of its existence. The whole deposit, including the walls and vein, is enclosed in the limestones peculiar to Tuolmune and the adjoining counties.

The graphite near the surface is much contaminated with the materials of the clay-slate, which rapidly decomposes on exposure to the atmosphere. It was this circumstance that delayed the development of the mine, as it was found impossible to separate the earthy matter from the more valuable plumbago. About two years since it was discovered that the graphite floated on water, when a simple apparatus was employed, which separates nearly 100 tons per day, giving employment to some 25 labourers to wheel the stuff from the mine to the arrastre, about 50 ft. distant. This separating process is not now required for in a large portion of the vein below the surface, at a depth of 40 ft., the graphite is cut out in solid blocks, and sacked without any further preparation; but it is divided

by lenticular bodies of clay-slate, a few inches thick, and sometimes extended for several feet. This material is heavily charged with graphite, and is washed in order to separate it. At the depth of 60 ft. the graphite is exceedingly solid and hard, with a very fine lustre. The whole material is taken out between the walls by an open cutting; all found not absolutely pure is washed, the other is bagged at once.

The product of the mine at present is about 1,000 tons per month, but is capable of an almost indefinite extension. The process of separating the graphite from the impurities is very simple. An inexpensive sort of large vat, with a stone bottom, 20 ft. in diameter, and 3 ft. deep, holds the materials, which are stirred up by iron rakes, fastened to four cross-bars projecting from an upright shaft, moved by a water-wheel. When in motion a small stream of water passes into this vat, an outlet to which exists a few inches from the surface; through this outlet the graphite passes with the water, and is led by spouts into broad tanks, where it temporarily subsides, when the dirty water is run off, and a large stream of clean water forces the graphite into a series of shallow tanks, in which it is dried by the sun, the whole process requiring about five days to complete. The tanks and reservoirs at present used cover several acres. The cost of production of the solid plumbago at the mine does not exceed 4s. per ton, two men in the lower workings being able to extract 10 tons per day of solid graphite, in blocks of any desired size. The water used on the mine costs but £10 per month, which sum is more than realised by gold found in the vat when cleaning up, there being a thin seam of auriferous quartz between the hanging wall and the graphite occasionally passing into it. Lumps of gold, worth from 8s. to £4 each, are sometimes found imbedded in the graphite.—*Dicker's Mining Record.*

ON THE

EMPLOYMENT OF SAND AND GLASS FILTERS IN QUANTITATIVE ANALYSIS.

By WOLCOTT GIBBS, M.D.,
Rumford Professor in Harvard University.

Sufficient attention has not been paid to the advantages of filters of sand and glass over those of paper, when precipitates are to be dried upon the filter at a definite temperature. By choking the throat of a funnel with coarse fragments of glass and then placing upon these successive layers of powdered glass or sand, the upper layer being of the finest powder, it is easy to make a filter upon which almost any precipitate may be filtered off and washed out completely without the slightest loss. The funnel with its contents may then be dried at any temperature below that at which the glass softens or at which the precipitate undergoes chemical or physical change. Mr. E. R. Taylor, who has carried out this suggestion with the greatest care and thoroughness, obtained the following results in three analyses of tartar-emetic:—

	Grm. tartar-emetic.		Grm.	
I.	0·0917	gave	0·0463	Sb_2S_3 = 36·09 per cent Sb.
II.	0·6236	,,	0·3149	,, = 36·08 ,,
III.	0·1766	,,	0·0891	,, = 36·07 ,,

The formula requires 35·92 per cent (Sb = 120).

In the first and second analyses the precipitated sulphide was dried at 375°C. In the third, the funnel had the shape of a tube tapering at one end and dilated in the middle to a sort of bulb, and the precipitated sulphide of antimony, after drying, was ignited in the filter tube itself in a current of carbonic acid gas. This form of funnel, which is due to Mr. Taylor, will be found very advantageous. A small common funnel may be inserted into the top, and after drying, the tube funnel closed with a cork in weighing.—*American Journ. Science.*

FOREIGN SCIENCE.

Paris, April 28, 1868.

Tar-water.—Extraction of the colouring matter from madder.—Petroleum in Galicia.—Academy of Sciences.—Formula of molybdic acid and equivalent of molybdenum.—Production of paracyanogen and transformation into cyanogen.

The medicinal properties of tar-water administered externally and internally in the treatment of various diseases of man and other animals are well known. Up to the present time this solution has not been manufactured in such a way as to contain a fixed proportion of tar. M. Guyot de Grandmaison, an intelligent chemist, has succeeded in obtaining a tar liquor perfectly capable of mixing with water, and containing a certain percentage of resinous matter. The application of his concentrated definite tar liquor has been attended with the greatest success, and this new preparation has, according to a report of one of our best practitioners, "*comblé une véritable lacune.*" Several trials were made with this article, in fifteen public hospitals in Paris, with the best results. This is a most natural consequence of the solution of tar, containing in a small volume all the active and medicinal portions of the tar, to the exclusion of the bitter empyreumatic principles always disagreeable to a sick palate, and often injurious to certain affections. No foreign substance is allowed to enter that can alter, suspend, or complicate the expected and precise effects of the medicament. The effects are produced by the tar alone, and by no other ingredient. Starting from the fact that two tablespoonfuls of the liquor, added to a litre of water, constitute a tar-water of average strength, and always constant, it is easy to increase or diminish the intensity at will. Pure, or mixed with water, the concentrated tar essence can be put to a variety of new usages which were not available for want of a convenient method of administration; in fact, ordinary tar-water was so slow in action that it had almost been set aside. A number of uses will at once suggest themselves to medical men; among the uses to which this new liquid is applicable, the following may be mentioned—the treatment of infectious wounds, skin diseases, small-pox, neuralgia, and rheumatism. In the shape of vapour, the tar-liquor has been frequently used with success in cases of pulmonary consumption and bronchitis. M. E. Guyot's address is Rue des Francs Bourgeois, 17; the success of his preparation is well deserved.

A process for the extraction of the colouring matter of madder has been patented by M. Guignot. This process only differs from that patented by M. Leitenberger, in garancine being operated upon instead of madder. In an exhausting apparatus, the garancine is submitted to treatment with boiling water. As soon as the water has passed through the filter, it is replaced by more, until the garancine is completely exhausted. Each quantity of hot water that has passed through is charged with a large proportion of colouring matter, which is nearly pure; by mixing all the coloured filtered liquors, and allowing the solution to cool, and repose a sufficiently long time, the colouring matter, previously kept dissolved by the heat, gradually becomes less ond less soluble, and at last precipitates to the bottom of the vessel. The supernatant liquid, which is almost colourless, is decanted, and the deposit thrown on to calico filters to drain, afterwards being dried sufficiently to enable the colouring matter to be transported in commerce, in the shape of powder. Garancine is obtained by submitting ground madder, exhausted by water of all soluble principles, and again dried, to treatment with concentrated sulphuric acid at a temperature of 100°. The ligneous, glucous, and pectous matters under this treatment become carbonised, while the colouring matter is left intact; washing with a little cold water removes the adhering acid, and matters which have been rendered soluble. M. Leitenberger's process was made to

operate upon the madder-root itself. He first exhausted with water at 45°, afterwards with boiling wood spirit, and these operations he carried out in apparatus perfected and patented by himself.

Mineral oils are now found and worked in other countries besides the United States; just now, Galicia is attracting attention as a valuable source for these oils. In West Galicia, mineral oil abounds; the principal petroleum reservoirs in this tract are found in the mountains enclosed by Limanowa, Neusandec, Grybow, Cieskowice, Gorlice, and Sbyszyce, covering an area of about 20,000 hectares. The ground is so charged with mineral oil, that it frequently issues in the form of springs, and it always suffices to bore 10 or 15 feet below the surface, to encounter the valuable substance. Despite the primitive modes of extraction employed, Western Galicia was able to extract in 1866 about 268 tons weight of petroleum. Several American engineers have visited the country; according to their accounts, the little river Dunajec has a fair chance of becoming the European oil-creek. Geologists and engineers who have carefully examined the country on all sides, consider the petroleum of Eastern Galicia to resemble that of Canada, while that from the western part of the province is almost identical with Pennsylvanian oil. In this latter portion, namely, the western, there are sources of oil which the American wells do not yield in purity and abundance; worked with intelligence, they would doubtless yield such fortunes as those in America have.

At the meeting of the Academy, on the 13th of April, the following memoirs were contributed:—"On the formula of molybdic acid, and on the equivalent of molybdenum," by M. Debray; "On the production of paracyanogen and its transformation into cyanogen," by MM. Troost and Hautefeuille. "The employment of fluoride of calcium for the smelting of phosphide of iron minerals," by M. Caron; "On the crystallisation of sulphur," by M. Schützenberger; from whom there was another memoir, entitled "On some reactions giving place to the formation of oxychloride of carbon;" "On the amides of sulphoxyphosphoric acid," by M. Chevrier; "An addendum, by M. Lecoq de Boisbaudran, to his previous communications upon the supersaturation of saline solutions," completes the list of papers of chemical interest.

It is almost superfluous to say that M. Debray's memoir is one of interest. M. Debray prepared chloride of molybdenum by the direct action of chlorine on the metal gently heated; the product, distilled in a current of dry carbonic acid (to eliminate excess of chlorine), has a deep green colour; it melts at 194°, and boils at 268°. The mode of preparation is similar, and the majority of the characters of this substance agree with a chloride of molybdenum to which Berzelius and others have attributed the formula MCl_2. In all probability this formula was given to the chloride in consequence of the solution, precipitated by ammonia, yielding an abundant precipitate of hydrated binoxide of molybdenum; but the presence still of a large quantity of molybdic acid in the filtered liquor is easily demonstrated. Further, three analyses of this substance have given 35 to 35·2 per cent of molybdenum: the formula M_2Cl_5 requires exactly 35 per cent of metal, while the formula MCl_2 requires rather more than 40 per cent. M. Dumas has accorded to molybdenum, in his work on the equivalents, the number 48. More recently, M. Delafontaine and M. Ullik have, notwithstanding, used the number 46 throughout their researches on the molybdates; M. Rammelsberg also found 46, in reducing molybdic acid by hydrogen, and this number is generally admitted in Germany. M. Debray's results confirm those of M. Dumas. He prepared pure molybdic acid by subliming it in a platinum tube; the sublimation cannot be effected in a porcelain tube, since molybdic acid easily attacks porcelain at the temperation of sublimation. The sublimed acid is extremely bulky, and to enable operations to be made upon a considerable scale it was necessary to transform the acid into molybdate of ammonia: this salt gives by careful calcination an acid both dense and free from lower oxides. The transformation of the acid, which is volatile, into fixed red oxide is effected by heating in a current of hydrogen at the lowest possible temperature; this reduction is made in a glass tube. Under these circumstances a little matter is carried out from the boat, and forms a red ring upon the sides of the tube. This amount of substance is appreciable; it must be carefully withdrawn, and treated successively with nitric acid and ammonia, and the weight determined. The final reduction is effected in a tube of unglazed porcelain, at a very high temperature. As molybdenum attacks the porcelain wherever there is contact, a platinum boat is used, and this prevented from touching the tube by a small piece of platinum foil. Finally, it is necessary to avoid the use of corks and every thing capable of furnishing gaseous carbides. The hydrogen is purified by passing over a long column of copper maintained at a red-heat during the whole time the experiment lasts; it is afterwards dried with fused potash. The porcelain tube M. Debray makes use of is long and furnished with a narrow tubulure, serving to convey the hydrogen. The equivalent deduced from the amount of metal obtained from a given weight of molybdic acid was in one experiment 48·03, in another 48·04, and in a third 47·84. The smallness of the third number is explained by experimental errors. The synthesis of crystallised molybdate of silver enabled these results to be verified. In evaporating slowly in a dark oven, a strongly ammoniacal solution of molybdic acid and nitrate of silver, small regular octahedral crystals of molybdate of silver were obtained. This solution was found to contain a little more silver than that required by the hypothesis $M = 46$, $MO_3 = 70$. By gently evaporating to dryness, and dissolving out the excess of nitrate of silver, the weight of silver not precipitated by the molybdic acid was easily arrived at by precipitating as chloride. The results thus obtained showed $M = 48$, and $M = 47·98$. The molybdate of silver was of course free from ammonia.

PROCEEDINGS OF SOCIETIES.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

February 24th, 1868.

J. B. Dancer, F.R.A.S., President of the Section, in the Chair.

Mr. Sidebotham sent two beautifully finished water-colour drawings, accompanied by the following note:—

"I send you a couple of drawings of the dry-rot fungus *Merulius lachrymans*, remarkably fine. Mr. Lynde, our treasurer, brought me the specimens, which he had met with during the demolition of some old buildings. The drawings are of the natural size. It is very rarely that such perfect specimens are found, showing, as this one does, the curious structure of the species, and its connection with both the pore and the gill-bearing divisions of fungi."

Mr. Dancer exhibited some sand from the sea-shore at Santos, South America. This sand was remarkably silvery in appearance, a large portion of it consisting of minute plates of mica, and very transparent fragments of quartz, which made it an interesting object for the polariscope. It was rich also in Foraminifera, spines of Spatangus, and fragments of Coralline.

"*Remarks on Molecular Activity as shown under the Microscope*," by J. B. DANCER, F.R.A.S.

The author stated that, during the last 30 years, he had met with many microscopical observers who were not acquainted with the phenomenon, to which the name of molecular action has been given. This class of microscopists had confined their attention to objects requiring a very moderate amount of magnifying power, and generally to dry objects; but when their investigations extended to minute objects immersed in fluid which required powers of 800 to 1500 diameters, they were startled by the appearance of particles in active motion, not moving in a direct line, but vibrating as if attracted and then repelled by each other, some single, and others in clusters.

Many instances have come under the author's notice, in which these objects have been regarded by microscropists as animalculæ. They have given rise to many very ingenious speculations, some of which are connected with spontaneous generation, these observers would have been saved much labour if they had been acquainted with the experiments of the late Dr. Robert Brown on active molecules.

The author does not imagine that the members of this section are wholly unacquainted with the experiments of the early microscopists on this subject, but in the absence of more important matter, he thinks a brief account of the early observations on these so-called active molecules may interest them.

The moving particles had been noticed by Leewenhock, Stephen Gray, Buffon, and others who supposed them to be animated matter.

In the year 1827, the late Dr. Robert Brown, whilst engaged in the microscopical investigation of unimpregnated ovulum, noticed that the pollen of the clarckia pulchella was filled with particles, which appeared in active motion when immersed in water: These observations were followed by the examination of the pollen of other plants, the particles of which he found to exhibit similar activity. For some time he was exceedingly perplexed with these phenomena, and was disposed to believe that he had really seen in these minute bodies the supposed constituents or elementary molecules of organic bodies, first so considered by Buffon, Wrisberg, Müller, and Milne Edwards, and on examining various animal and vegetable tissues, whether living or dead, he found, as he had expected, that active molecules were visible by merely bruising the substances in water.

Continuing his observations, he found that particles from a bruised specimen of fossil wood appeared to consist entirely of these moving bodies. From this he inferred that these molecules were not limited to organic bodies, or even to their products.

After this he proceeded to examine minerals, simple earths, metals, and many other substances too numerous to mention, and with similar results.

Some writers who commented on these experiments, but who had not carefully followed his communications, asserted that Dr. Brown imagined these particles to be animated,—and this statement was generally believed.

In 1829 the author's late father repeated many of Dr. Brown's experiments, and to prove that these moving particles could not be animalculæ, he placed some crystals and minerals in a crucible which he subjected to a red-heat, ground portions of them to powder, then put it into distilled water, and showed the particles in motion to his scientific friends. It is now well known that all kinds of matter, if reduced to sufficiently small particles, and placed in a medium in which they will not readily sink, will exhibit these movements.

Now, as this phenomenon occurs in the cells of plants, the yolk of the egg, and in decomposing animal and vegetable matter, it is not surprising that the early microscopists, and, indeed, modern ones, should have mistaken them for animalculæ. The particles which exhibit the greatest activity are exceedingly minute, ranging from 10,000th to 30,000th of an inch in diameter; they remain active a considerable time if they are nearly of the same specific gravity as the solution in which they are immersed. One simple mode of producing them is to rub a little gamboge in water, on a glass slide, and place a thin glass cover on it, using a power of from 800 to 1,200 diameters. They can easily be distinguished with less magnifying power, but are not so effectually shown. If they are required for prolonged examination, Dr. Brown recommends that the solution of gamboge be mixed with a little almond oil. The minute globules of water are thus surrounded by oil, and rapid evaporation is prevented.

The cause of the phenomenon is not yet satisfactorily accounted for. Some have imagined that it is the physical repulsion of the particles when uninfluenced by gravitation. The author has tried many experiments with electricity and magnetism without success. He thinks that the movement may possibly be connected with the absorption and radiation of heat.

Those interested in Dr. Brown's experiments and observations on active molecules, can refer to his republished papers in Vol. I. of the Ray Society's publications for 1866.

After the meeting, slides containing active molecules were exhibited to the members.

A conversation took place on the preservation of dried plants, in which Mr. Bailey stated that he uses with success glue with carbolic acid for attaching them to the paper, as a preservation against mites, placing also a few crystals of the dry acid in his cabinet.

Silvering Glass Mirrors.—The process we propose to describe has for its author Prof. Henry Draper, of this city, and may be divided into five operations, viz., the cleaning of the glass, the preparation of the silvering solution, the warming of the glass, the process of silvering, and the polishing. The description is for a 15½ inch mirror. 1. Rub the glass plate thoroughly with aquafortis, and then wash it with plenty of water and set it on edge on filtering paper to dry; then cover it with a mixture of alcohol and prepared chalk, and rub it in succession with cotton flannel. 2. Dissolve 560 grains of Rochelle salt (tartrate of soda and potassa) in 2 or 3 ounces of water, and filter; dissolve 800 grains of nitrate of silver in 4 ounces of water. Take an ounce of strong ammonia of commerce and add nitrate solution to it until a brown precipitate remains undissolved. Then add more ammonia and again nitrate of silver solution. This alternate addition is to be carefully continued until the silver solution is exhausted, when some of the brown precipitate should remain in suspension. Filter. Just before using, mix the Rochelle salt and add water enough to make 22 ounces. The vessel in which the silvering is to be performed should be a circular dish of ordinary tin plate, and coated with a mixture of equal parts of beeswax and rosin. At opposite ends of one diameter two narrow pieces of wood are cemented to keep the face of the mirror from the bottom of the vessel. 3. The glass is slightly warmed by putting it in a tub or other suitable vessel and pouring in tepid water to cover the glass; then hot water is gradually stirred in. 4. Carry the glass to the silvering vessel, into which the silvering solution has been poured, place the whole apparatus before the window and keep up a slow rocking motion. Leave the mirror in the liquid 20 minutes or half an hour, and wash with plenty of water. 5. When the mirror is perfectly dry, take a piece of the softest buckskin, stuff it with cotton, and go gently over the whole silver surface to condense the silver. You may use some of the finest rouge. The best stroke is a motion in small circles; rub an hour. The thickness of the silver thus obtained is about 1-200,000th of an inch.—*Scientific American.*

CHEMICAL NOTICES FROM FOREIGN SOURCES.

New Synthesis of Alcohols, and Chemical Structure of Ethylene.—Butlerow and Ossokin have acted upon ethylglycoliodhydrin with zincic methide and ethide with a view of obtaining alcohols, from the nature of which conclusions might be drawn as to the chemical structure of ethylene present in the iodhydrin. The reaction proceeds in the following two stages:—

$$\left.\begin{matrix}C_2H_4I\\H\end{matrix}\right\}O + R_2Zn = \left.\begin{matrix}C_2H_4I\\(RZn)\end{matrix}\right\}O + RH$$

and,

$$2\left(\left.\begin{matrix}C_2H_4I\\(RZn)\end{matrix}\right\}O\right) + R_2Zn = \left.\begin{matrix}C_2H_4R\\(RZn)\end{matrix}\right\}O + ZnI_2.$$

By decomposing the compound last formed with water, the alcohols are obtained, and they were found to be secondary (iso-)propylic—when zincic methide was used—and secondary butylic alcohol—in the case of zincic ethide. It would follow from these experiments that the structure of ethylene is

$$\left.\begin{matrix}CH_3\\CH\end{matrix}\right\};$$

as, however, all other reactions of ethylene lead to the formula

$$\begin{matrix}CH_2\\CH_2,\end{matrix}$$

it is supposed that in the present instance its structure changes from the latter to

$$\left.\begin{matrix}CH_3\\CH\end{matrix}\right\}.$$

—(*Zeitschr. Chem.*, N.F. iii., 680).

Silvering of Glass.—Liebig. After a long series of experiments, the author finally adopted the following process for silvering glass. The solutions employed are:—I. One part of fused argentic nitrate dissolved in 10 of water; II. (a), commercial nitric acid, free from chlorine, neutralised with ammonic sesquicarbonate, and diluted to sp. gr. 1·115; or (b) 242 gr. ammonic sulphate dissolved in 1200 c.c. water (sp. gr. 1·105 to 1·106); III. Solution of sodic hydrate sp. gr. 1·050 prepared from sodic carbonate, free from chlorine; IV. 50 grm. white sugar candy dissolved in little water, 3·1 gr. tartaric acid added, the mixture kept boiling for one hour, and diluted to 500 c.c.; V. 2·857 gr. dry cupric tartrate, covered over with water, and solution of sodic hydrate gradually added till solution has taken place, and solution made up to 500 c.c. These solutions are mixed in the following proportions:—1st, 14 vol. of I., 10 vol. of II., and 75 vol. of III. = 99 vol. of (A) *silvering solution*; 2nd, 1 vol of IV., 1 vol. of V., and 8 vol. of water = 10 vol. of (B) *reducing solution*. The *silvering mixture* is then made by diluting 50 vol. of the silvering solution (A) with from 250 to 300 vol. of water, and adding 10 vol. of the reducing solution (B). If ammonic sulphate has been employed for solution (A) the liquid, after mixing the three ingredients, must be allowed to stand three days before being used; the clear liquor may then be drawn off.—(*Ann. Chem. Pharm. Suppl.*, v., 257.)

On some Compounds of the Toluol-group.—H. Limpricht and H. Schwanert. Toluylene $C_{14}H_{12}$ is converted into dibenzyl $C_{14}H_{14}$ by the action of iodhydric acid. It combines with bromine to $C_{14}H_{12}Br_2$, which on distillation or treatment with alcoholic potassic hydrate changes to bromtoluylene $C_{14}H_{11}Br$. This again combines with bromine forming a crystalline body $C_{14}H_{11}Br_3$. Continued action of alcoholic potassic hydrate removes the whole of the bromine with formation of tolan $C_{14}H_{10}$, a well crystallising hydrocarbon isomeric with anthracene. Tolan forms the bromide $C_{14}H_{10}Br_2$, which is reconverted to $C_{14}H_{10}$ by alcoholic potassic hydrate. Bromtoluylene shows the reaction of the bromide of a diatomic alcohol; with argentic acetate toluylenic acetate

$$C_{14}H_{12}(C_2H_3O)_2O_2$$

is produced, and this is converted by means of alcoholic potassic hydrate into toluylenic alcohol $C_{14}H_{14}O_2$, which is identical with Zinin's hydrobenzoine.—(*Zeitschr. Chem.*, N.F. iii., 684).

NOTICES OF BOOKS.

Tea. By E. F. Bamber. London: Longmans. 1868.

The author, who has lived for some years in the tea-producing districts of Asia, has produced a very readable little book. He first goes into the historical part of the subject; then he discusses fully the cultivation of the plant, and the manipulation the leaf undergoes before it becomes an article of commerce, and explains what constitutes the difference between the three classes into which tea may be divided:—*Flowery Pekoe*, *Green Tea*, *and Black Tea.*

It is a great pity that a writer who is so fully conversant with his subject should not confine himself to that with which he is familiar, but should diminish the trustworthy character of his book by speaking authoritatively on matters concerning which he appears to be profoundly ignorant. The section in which Mr. Bamber explains the chemistry of tea is remarkable as showing the unhesitating manner in which a writer ignorant of the veriest elements of chemistry will criticise the results obtained by one of the leading chemists of the day. Speaking of Theine, Mr. Bamber writes:—

"Theine cannot be produced artificially. 'Water decomposes this compound easily, and Hydrochloric Acid evaporates from it in warm air, so weak a base is Theine.' In referring to Mulder's analysis, it will be remarked that Muriatic Acid extracts form nearly one-fifth of the whole weight of Tea; and Muriatic Acid and Hydrochloric Acid are synonymous terms."

The following passage is, in our opinion, inimitable:—

"Mulder's analysis is considered the best that has been made of Tea. It appears not to be a very good one; for Muriatic Acid has a strong sour taste, and if Tea had, one-fifth of its total constituents, Muriatic Acid Extract, it should have a strong sour taste, which it has not; probably the Theine in the Tea which he analysed had turned into Muriatic Acid. As a proportional analysis, Mulder's can carry with it but little authority, for the totals of the parts of each of the Teas disagree. Still it has its use, for where Mulder states that there was no trace of a constituent, probably there was none."

The Inductorium, or Induction Coil. By Henry M. Noad, Ph.D., F.R.S. London: Churchill & Sons.

We noticed the first edition of this useful little volume on its appearance in 1866. It gives us pleasure to have the opportunity of here recording the issue of the third edition, containing several additions and improvements. Amongst these we notice a description of Wheatstone's and Siemen's automatic or self-charging electro-magnet, and the ingenious method of utilising the current energy evolved by these machines in the modification constructed by Mr. Ladd. Dr. Noad's description of the somewhat

complicated principle involved in these machines is so clear that we are induced to quote it for the benefit of our readers.

On page 32 he writes: "In this curious contrivance the terminals of a revolving armature are (with the intervention of a commutator, to change the direction of the alternate induced currents) placed in connection with the terminals of the electro-magnet coil. Now if the iron body of the electro-magnet were absolutely free from magnetism, no action would issue from the rotation of the armature; but practically that is never the case, for the mass of iron is never absolutely pure and soft; it consequently always retains a small portion of the magnetic polarity which was last induced in it, by the transmission of an electric current through its coil, consequently on the first semi-rotation of the armature, a minute induced current is evolved; but this, in consequence of the connection described, is returned into the electro-magnet coil, and slightly exciting the electro-magnet, a slightly stronger induced current is delivered by the armature, during its second semi-revolution; and so on successively, until the currents induced in the rotating armature acquire a very high degree of energy. If the primary coil of an inductorium be now interposed in the circuit as in Professor Wheatstone's apparatus, sparks of considerable length may be obtained between the terminals of the secondary coil; or a certain length of platinum wire will become incandescent. It may here be remarked that as the energy of the induced currents is augmented, the resistance of the armature to rotation, and consequently the amount of dyamic energy necessary to rotate it is similarly augmented. This fact presents a strong confirmation of the theory that an electric current is merely an undetermined form or mode of energy, and that, by the mediation of the machine here described, dynamic is converted into electric energy."

Mr. Ladd's machine is similar to the above: he employs a second armature, placed between the contrary ends of the plates which constitute the electro-magnet, in which an equal current is induced as in the armature already described; this latter current may be utilised in any required manner.

Mr. Ladd has allowed us to use some of the very beautiful wood-cuts which adorn this book, to illustrate his dynamo-magneto machine which gained the silver medal at the Paris Exhibition, 1867. In this machine (Fig. 1), the ends A of the electro-magnets, B, are firmly screwed to two pairs of iron prolongations, C, forming two cylindrical cavities in which the two armatures, *m*, *n*, are rotated in the same direction by two bands passing over a common drum. The armature, *n*, returns its currents into the electro-magnet, while the currents from *m* are utilised, as in producing the electric light between the carbon terminals, D.

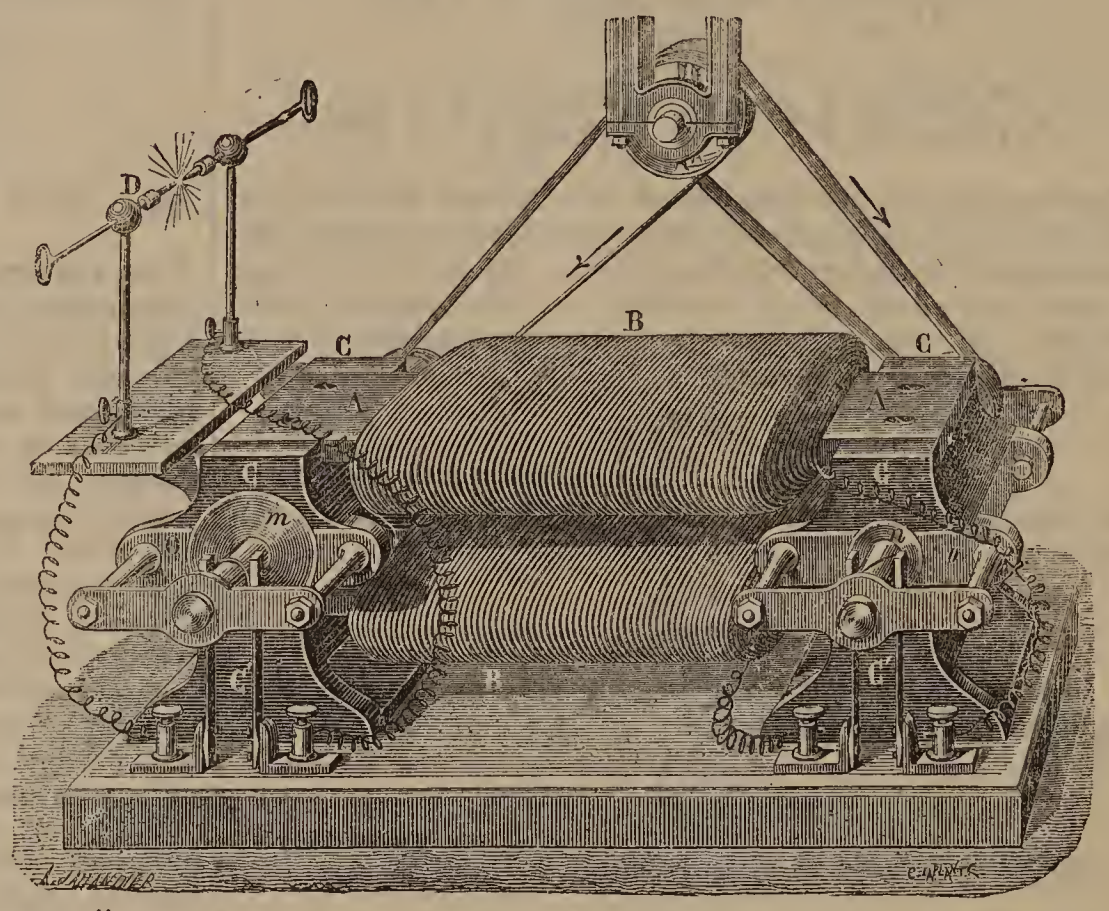
In another form of the apparatus constructed by Mr. Ladd, the two armatures of the former are combined in one, and the coils are wound at right angles to each other as shown at A and B in Fig. 2.

Of these coils the shorter, B, feeds the electro-magnet, and the longer, A, furnishes the utilised current.

The results to be derived from these dynamo-magneto machines are always in proportion to the power expended, and the only limit appears to be the rapidity with which the armatures can be rotated.

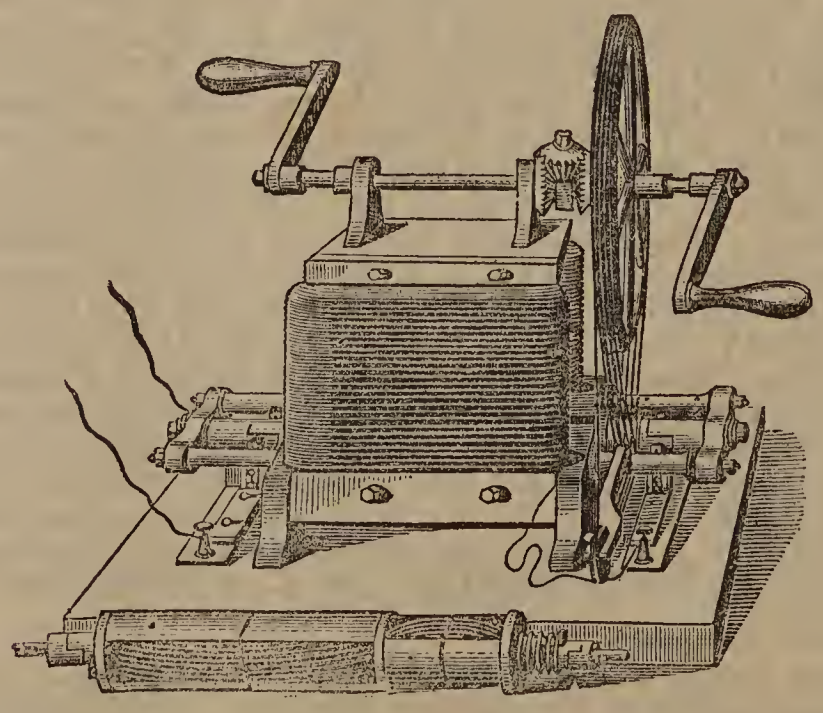
Mr. Beanes's ozone generator, of which so much has been heard and so much more is expected, is here described. The object for which it was made was to produce ozone in very large quantities, so as to apply it to commercial purposes, such as decolourising sugar, oils, rags, &c. For this purpose an apparatus was made as follows:—"Thirty plates of thin glass 18 in. by 9 in., each plate being coated with tinfoil on one side and separated from those next it by slips of glass placed along two edges so as to leave a series of thin broad channels, for the passage of the air to be ozonised, each coating being alternately connected together in a similar way to a condenser of an inductorium; the whole is then placed in a well-made box thoroughly protected with shellac to prevent the ozone acting upon the wood; the box is left sufficiently long to allow of two openings, one at either end beyond the glass plates, for the passage of a current of dry air; the ends of the box have glass plates inserted for observation. The area between the plates in the above

Fig. 1.



apparatus was about 14 square inches; the two terminals of the coatings are insulated through the box, and when connected with a good inductorium, a glowing discharge will be seen between the plates of glass, and as one discharge is sufficient to ozonise the air at that instant between the plates it is evident that a very rapid blast can be sent through, and an enormous quantity of ozone

Fig. 2.



obtained; it is essential that the air should be cold to properly ozonise, but a moderate amount of heat afterwards does not affect it. The pipes conveying the ozone should be of lead, tin or, glass; there are but few other substances that are not affected by it."

Braithwaite's Retrospect of Medicine. Vol. lvi.

For more than a quarter of a century this most useful compilation has rendered good service to the medical profession. The present volume contains no fewer than six papers, amongst others from the authoritative pens of Lister, Syme, and Sir J. Y. Simpson, "On the Application of Carbolic Acid to Practical Surgery." Whether according to Pasteur, carbolic acid acts by destroying the septic organisms which probably in myriads infest the air of hospitals, or whether, according to his antagonist Pouchet, it operates in some undetermined manner, by preventing oxidisable matter passing into a putrid condition, certain it is that this agent is effecting a beneficent revolution in surgery. A large proportion of the disasters and deaths which arise from compound fractures, wounds, and amputations, is caused by the decomposition of blood and sloughs. The evidence contained in this retrospect shows that carbolic acid, properly employed, prevents this corrupting change, and warrants the assertion that the boon which chemistry has conferred on medicine will avert a measureless amount of suffering and death.

CORRESPONDENCE.

ORGANIC MATTER IN WATER.

To the Editor of the Chemical News.

Sir,—A little consideration of the action of permanganate of potash on organic matter will, I think, show the fallacy of the supposition that the quantity of a standard solution used is in direct ratio to the weight of organic matter present in water; unless we come to the unwarranted conclusion that the organic matter in water is homogeneous. Suppose two waters contain exactly the same weight of organic matter, W say, and that this weight is distributed among the elements carbon, hydrogen, oxygen, and nitrogen, in such a manner that, in the one case—

$$W = pC + qH + rN + sO,$$

and in the second case—

$$W = p'C + q'H + r'N + s'O.$$

Suppose that the permanganate exerts its full oxidising action, so that all the carbon is changed into carbonic acid, and all the hydrogen into water. Let O denote the quantity of oxygen required in the first case, and O′ the quantity in the second. Then, if O″ denotes the quantity of oxygen contained in each measure of the standard solution, we shall have $PO'' = O$ and $P'O'' = O'$, where P denotes the number of measures used in the first case, and P′ the number in the second case. We shall then have the following equations:—

$$PO'' = (pC \times 2\tfrac{2}{3} + qH \times 8) - sO,$$

and in the second case—

$$P'O'' = (p'C \times 2\tfrac{2}{3} + q'H \times 8) - s'O.$$

According to the permanganate method of estimation, we should have the equation $P = P'$, and consequently, $PO'' = P'O''$. A result which is very remote, and which evidently requires the extraordinary supposition that, whilst the total weight of organic matter remains the same, the four quantities vary so among themselves, that the quantity of oxygen consumed remains constant. For the sake of the simplicity of the example, suppose that the organic matter consist of carbon and hydrogen only, and that two waters contain each, in an equal bulk, 5 gr. of organic matter, distributed as follows:—In the first case, $3\frac{3}{4}$ gr. of carbon and $1\frac{1}{4}$ gr. of hydrogen; and in the second case, $4\frac{2}{7}$ gr. of carbon and $\frac{5}{7}$ gr. of hydrogen. Our equations would be—

$$PO'' = 3\tfrac{3}{4} \times 2\tfrac{2}{3} + 1\tfrac{1}{4} \times 8 = 20.$$

$$P'O'' = 4\tfrac{2}{7} \times 2\tfrac{2}{3} + \tfrac{5}{7} \times 8 = 17{\cdot}14.$$

Thus the number of degrees of permanganate used in the first case would be to the number used in the second, as 20 : 17; and it would be inferred that the quantities of organic matter were in this ratio, whereas the weights were equal. If, on the other hand, the permanganate of potash does not exercise its full oxidising power, but reduces the organic bodies present to forms of less complexity, it is evident no reliance can be placed upon an agent of such uncertain action.—I am, &c.,

James Bottomley, B.A.

Queenwood College, near Stockbridge, Hants.
April 22nd, 1868.

PERMANGANATE AS A SANITARY WATER-TEST.

To the Editor of the Chemical News.

Sir,—Owing to the Easter holidays, I have been unable sooner to make answer to the remarks which have been addressed to me in the Chemical News, by Mr. Spencer and Professor Attfield.

To the former, I would reply that I was well aware of the fallacious indications which the presence of iron, in solution, in water may occasionally cause permanganate to give; but, as I was not recommending that substance for analytical purposes, but merely as a sanitary water-test, well suited for popular use, and did not pretend to give directions for its employment, and the correction of possible errors, I did not consider it necessary to allude to them. To prove, however, that I had not overlooked this source of fallacy, I will quote a passage from a communication of mine on this subject, which appeared in the *British Medical Journal* of the 15th of February last. "The presence of deleterious organic impurities in water," I said, "will rarely or never fail to be detected by the addition of permanganate solution. Other less dangerous or innoxious oxidisable matters, as iron, for instance, may no doubt occasionally cause a comparatively inoffensive water to be suspected of being hurtful; but it is barely possible that when really dangerous matter of organic origin is present, the permanganate will allow it to escape detection. It may leave comparatively untouched other organic matters, but these will assuredly be the more sound and stable substances which are incapable of producing the 'horrible results' that are sometimes occasioned by unsound and decomposing matters."

When iron is present in such quantity as to cause erroneous indications, it will usually be detected by the palate. The taste communicated to water by even very minute quantities of iron, is one which most people would at once recognise. This source of error being known, nothing is easier, for a person possessed of only a smattering of chemical knowledge, than to determine whether the decolouration of the permanganate arrises from iron. But the grand object of a popular water-test, so far as organic matter is concerned, is to afford the public a ready and simple means of detecting and rejecting water that may prove prejudicial to health. This the permanganate test satisfactorily accomplishes. If its use may sometimes lead to the temporary rejection, as unwholesome, of water charged with nothing worse than iron, the mistake will be on the safe side, and can never give rise to injurious results.

The popularisation of the permanganate test would assuredly tend to put an end to the occurrence of those outbreaks of fatal disease which are caused by the use of polluted water; and if this result were obtained at the expense of the occasional rejection of an innoxious water, it would be very cheaply purchased. But, what is more, the popular use of this substance would not only prove an effectual safeguard against those accidents to the public health which arise from the use of befouled water, but would afford the means of extemporarily purifying such water, and rendering it fit for consumption. Water that has been treated by permanganate will be found to be deprived of putrescible matter. Even repulsively foul

water, after purification by this means, is not liable to become offensive. Permanganate solution, therefore, is not only of great value as a popular test for polluted water, but a safe and reliable means of freeing potable water from deleterious putrescible matter.

To Professor Attfield, I would reply that I am quite ready to admit that, instead of having proposed a new method of detecting organic pollution of water by the olfactory sense, as I, it seems, erroneously put it, he has merely suggested an improved means of rendering the nose a more efficient detective. As stated in my former communication, I consider Professor Attfield's suggestions on this subject well worth knowing, and I give that able chemist every credit for freely offering them to the general public. When he found fault with my letter in the CHEMICAL NEWS of the 3rd of April, for being similar to one which had appeared previously in the *Medical Times and Gazette*, he was not, perhaps, aware that the communication which called it forth had already appeared almost verbatim in another periodical.—I am, &c.,

J. MUTER.

SCIENCE TEACHING IN SCHOOLS.

To the Editor of the Chemical News.

SIR,—In the last number of this journal, your correspondent "D." criticises some lectures of mine recently given at Eton College, and I feel constrained to offer a word in reply, although it is to be wished that the writer had given his name in full.

It would appear to be unwise to judge of the nature of a course of lectures from the concluding sentences of the last lecture, because a generalisation founded upon the assumption that the whole is similar to an infinitely small part cannot be very trustworthy. In the case in point, the small part differs materially from the whole, because the concluding portions of a course of lectures, to a greater or less extent, summarise the matter of the entire course, and usually give the more plausible and dominant deductions. A work on arithmetic may commence with simple addition, and end with adfected quadratic equations; but it would be obviously an erroneous practice to deduce the nature of the whole book from its last chapter. As in the book, so in the course of lectures; we ascend from simple matters to those which are more difficult:—"*Neque enim in plano via sita est*," says Francis Bacon, "*sed ascendendo et descendendo; ascendendo primo ad axiomata, descendendo ad opera.*"

"If we may judge of the lectures themselves," writes "D.," "from these extracts, I would say that such lectures are not adapted to attract boys to science." Allowing for a moment, for the sake of argument, that the lectures may be judged of from the extracts (which I trust has been disproved above), I may say that I fully concur with "D." "that such lectures are not adapted to *attract* boys to science." They would not be fulfilling their object if they sought to do this; for if science, in its plain unvarnished form, does not present sufficient attractions to the student, it ill becomes the teacher to make it externally showy and factitiously attractive, while the reality must ever remain the same. It would be like binding a book, on say, Hebraic punctuation, in the same kind of binding as a popular novel. Those who require to be attracted to science by forced means can never study it with advantage, nor should we ask such to commence the study.

I do not mean to say that lectures on natural science should be sparsely illustrated by experiments: there should always be a sufficiency, and there is a limit in both directions. A lecture which is insufficiently illustrated is quite as bad as—some would say worse than—one which is over-illustrated; in the latter, there are so many experiments, that it is impossible to have them properly explained; in the former there is too much dry explanation, which could as well be acquired from a book. It is the happy mean that we all desire to attain. A lecture on natural science can rarely be sufficiently illustrated by less than fifteen experiments, and it is usually over-illustrated when the number of experiments exceeds twenty-five.

Because science happens to be popularised in certain places and in certain well known magazines, there is a general impression that it is to be viewed as an amusement, and that it does not require serious study and application to master it. Nothing can be more erroneous. Until this idea is disproved, natural science can never be taught in schools with advantage. If science is to be taught at all, it must receive from the pupil as much attention as he gives to Latin and Greek; it must not be regarded as an amusement or light study; notes must be taken, and themes must be written after each lecture, and a two-hour examination must be held at the conclusion of the course. Popular science is as much out of place in a school as popular Greek would be. To popularise a thing, is usually to degrade it. It is indeed right for us to rend the dark veil of the temple of nature, so as to expose the shrine to the worshippers without; but I think it is unnecessary to make the shrine garish with a false lustre.—I am, &c.,

G. F. RODWELL.

ESTIMATION OF POTASH.

To the Editor of the Chemical News.

SIR,—With reference to the abstract of our paper on the "Estimation of Potash," which appeared in last week's impression, we beg to state that on account of its requiring to be posted here on the evening following that on which it was read, we were unable to revise it.

Will you therefore kindly allow space for the following corrections in this week's issue, in order to prevent the necessity of any remarks or correspondence on the part of your numerous readers?—We are, &c.

THE AUTHORS.

Corrections.—Col. 2nd, line 10th from bottom, read *sulphate* of soda. Col. 3, line 12 from bottom, read *hydric sulphate or hydric chloride*. Col. 3, line 17, read *precipitates and evaporated washings*. Col. 4, line 12, read to dissolve the *pure potassic chloride*. Col. 4, line 13, introduce the following paragraph:—

"The method which we ultimately adopted for purifying recovered platinum, is that by reduction with alcohol and excess of sodic hydrate, washing, boiling with hydrochloric acid, washing, drying, and simply igniting the resulting platinum black, and finally boiling with nitric acid and washing. This method invariably gives excellent results when the platinic solution is tried on pure potassic chloride."

Col. 4, line 23, read, the authors object to the method of *weighing* small quantities (as 10 grs.) of dried salts. Col. 4, line 11 from bottom, read, ·19313 and 1·582493.

DEATH BY ELECTRICITY.

To the Editor of the Chemical News.

SIR—I perceive you have a paragraph on the above in your last number of the CHEMICAL NEWS, being an alleged new form of capital punishment, by a writer in *Harper's Weekly*.

I beg to inform you that I am the original proposer of the above; in June, 1863, I suggested to the Conservateur des Abattoirs à Paris this method for slaughtering cattle; through insufficient address, this letter was returned to me by the "dead letter office." February 17th, 1865, you published my suggestion in the CHEMICAL NEWS, page 84.

In May, 1866, the Royal Society for the Prevention of Cruelty to Animals wrote to me that "they will be glad to appoint a deputation to attend any experiments I may desire to make, and that they will procure the permission of a butcher for the trial." This fell through because I had neither the apparatus nor the time to carry it out.

In 1866 I proposed this scheme to His Grace the Duke of Richmond, to be used instead of hanging, in capital punishment, conjointly with the suggestion that executions should take place in private, for which I gave my reasons why I considered it more expedient than the present mode. I also used the following words—"Simultaneously with the shock, the wretched being would pass into eternity without a pang."

I have condensed my letter as much as possible, and in conclusion beg a place in your journal, in common fairness to myself.—I am, &c.,

Chas. N. Ellis.

Wolverhampton, April 29th, 1868.

MISCELLANEOUS.

TECHNICAL EDUCATION.

The Chemical News, we think, may be justly proud of the fact that it was the first journal to introduce this subject to the English reading public. The question has been warmly taken up both by the medical, weekly, and ordinary daily papers, numerous articles have been written upon it, and a number of scientific gentlemen have formed themselves into a committee for advocating its adoption in England, For this really very considerable amount of progress we have to thank mainly the British Association for the Advancement of Science; and if a scheme is promptly drawn up, and as promptly carried out, for the fulfilment of the objects in view, the credit resulting to the Association will be one of the greatest in the series of its already great triumphs.

The public must do scientific men the justice of acknowledging that they have not been slow on their part in urging this matter, the result of its neglect every one now admits to be most disastrous; let us not, therefore, wait for something worse by letting these questions now die out by want of discussion. The recognition of a defect is the first step towards its remedy, and that the recognition has been given any one may judge from the daily press; we quote the following from a publication of great promise, *Beeton's Journal*, which is intended to obtain, and we believe has already done so, a wide circulation among the boys of our large colleges and schools:—

"However soon we may set technical schools going, we cannot bring the knowledge so acquired to bear for many years. Our hope must be in the rising generation of Englishmen. They should think, *think* over these mechanical arts; they should view them as a noble study. Every fresh labour-saving machine means another class of men raised from degrading toil to healthy independence; from a position where the mind is robbed, it may be, of all chance of improvement, to one where the mind is employed and the body saved. And it means, moreover, a new source of wealth to the country, and a new vantage-ground over foreign states.

"If the spirit of the great inventors is to slumber through the rising generation, then, when our great men do arrive, we shall probably be too far behind in the race to make good our lost ground. But this ought not to be. The new generation sees the works of the giants of other days; it reads of their doings. It has not the same fear of leaving their beaten paths, of departing from their practice, which may be supposed to have deadened the inventiveness of the pupils of the great men themselves. There are great things waiting to be invented. There are fame and wealth to be won. But we want something altogether new and wonderful, something to set our hearts beating as our fathers' beat at the electric telegraph, not to set our brains calculating the dividends to be made by sending messages. Something grand, and with an interest apart from money."

It should not be forgotten that the question of technical education for workmen and their employers must be regarded as inseparable from that of scientific education; in the higher classes of society, the one without the other is quite valueless. It may be freely granted that technical education is encouraged abroad and neglected at home; the evil result is admitted to be of magnitude; but few results of this kind are due to one cause only, and a further cause, and in the main, not a less effective one, is the want of recognition that is awarded socially and politically to men of science generally. Science requires support by State grants, Parliamentary representation, and introduction into schemes of general education, for its proper development in Engiand. It is sufficient merely to indicate the ground that should be taken up by all scientific men, and we have endeavoured to supply them in this journal with the materials necessary for a clear expression of facts as they stand. When such materials are afforded as data, we can safely omit all comparisons, and there need be no fear that the verdict of all thinking men will be otherwise than a unanimous one.

We begin, then, with the report that has been drawn up by Dr. Bahin upon the technical schools of France; we only wish that there existed in England an official blue book drawn up by competent commissioners.

"Of the many institutions in France devoted to technical education the *Ecole Centrale des Arts et Manufactures* stands highest. It enjoys a world-wide reputation, as pupils flock to it from all parts of the civilised globe. It was founded in 1829 by a private Society of eminent scientific men for the purpose of training civil engineers and managers of industrial work and manufactories. It has well sustained its reputation; from a private it has gradually risen to the importance of a national institution, by raising industry to the rank of the liberal arts, and by creating, as it were, a new faculty. The instruction provided here is not only theoretical, but eminently practical, as most of the professors are former pupils of the school, who, after devoting many years to practical work, return with this additional experience, to carry out those sound methods of instruction under which they themselves were formed. The pupils of this school are not all drawn from the richer classes, but are to a certain extent recruited from the workshops. The principle of assisting poor but deserving students forms a striking feature in its organisation. I quote the following words from the prospectus:—"To give to a young man, poor but talented, the means of developed his faculties, to endow the country with an industrious man who will know how to draw fresh wealth from it, is not only a good action, but a good investment." The government, the local authorities of the departments, and the *Societé d'Encouragement* have each contributed towards this noble work. The course of study extends over three years. Candidates for admission are required to pass satisfactory examination in elementary mathematics and composition. The studies of the first year are general and obligatory upon all pupils. They form a suitable preparation for the succeeding years, and enable each pupil to select the special branch of industry for which his taste and capacity best adapt him. The selection must be made at the beginning of the second year. The studies are then divided into four sections—1. Machinery; 2. Physical science; 3. Chemistry; 4. Mining and metallurgy. The pupils undergo special examinations during the year, and a general one at the end of each year, which serves to determine their fitness for passing from a lower to a higher grade. Those who at the end of the course satisfy all the examination tests receive diplomas; and those who satisfy only a certain number receive

certificates of capacity. These diplomas and certificates are the sure introduction to honourable and lucrative employment, affording thus a valuable criterion of the estimation in which the school is held, and of the importance of the work which is done in it.

Next in importance are the Ecoles impériales des Arts et Métiers of Aix, Angers, and Châlons. These are essentially Government schools, established for the purpose of training foremen and managers of workshops and skilled artisans. Each school draws its pupils from a certain number of departments, of which it is the centre. Applications for admission must be addressed to the prefect of the department in which the candidate resides, accompanied by certificates of birth, health, good conduct, and general aptitude for some mechanical labour. Admission is obtained only after passing two examinations. The first is both oral and written, and comprises reading, writing, arithmetic, elementary geography, geometrical drawing, and the drawing of ornament; and, in addition, each candidate is required to execute some piece of work in wood or iron, in connection with the trade which he has been learning. The second examination is entirely oral, and limited to the subjects of the first. The following details are worthy of note :—Pupils are received only as boarders, at an anuual payment of twenty-four pounds. The parents of poor but successful candidates, on addressing a formal application to the minister, are exempted from paying either the whole or a certain proportion of this sum, according to their means. The course of instruction extends over three years, and is both theoretical and practical. The former comprises arithmetic, algebra, trigonometry, descriptive geometry, mechanics, drawing, and grammar. The latter is carried on in four workshops, and includes carpentry, smithery, casting, and fitting. Prizes are awarded at the end of each year to the most deserving pupils, and certificates at the end of the course to those who acquit themselves satisfactorily; and, if any specially distinguish themselves, they receive in addition a silver medal. In the eastern departments of France, which abound in manufacturing industry, the benefits of technical education have been widely extended. At Mulhouse, one of the principal centres, there are several important schoolls, founded and maintained by the munificence of the leading manufacturers.

The Ecole Professionnelle renders valuable services to the cause of industrial progress by the variety and extent of its educational programme. It comprises five divisions —1. An elementary division, in which languages, mathematics, and the principles of science are taught. 2. A special division, where, in addition to the higher branches of the above subjects, practical science is taught in the workshops, under the direction of an engineer and two foremen. 3. A commercial division. 4. An industrial division, where the instruction is chiefly practical, and carried out in the workshops, the chemical laboratory, and the schools for spinning and weaving. 5. An upper division, serving as a complement to the second and fourth divisions, and preparatory to the Ecole Centrale. In the practical department of dyeing, printing, spinning, and weaving, the pupils are initiated into all the most recent improvements, and, under the guidance of an experienced professor, they pay periodical visits to all the great industrial establishments in the neighbourhood. There is also a special school at Mulhouse for teaching the theory and practice of power-loom weaving established under the patronage of the Industrial Society. The school was called into existence for the express purpose of stimulating improvement in this branch of industry. Connected with it is a complete working establishment, to which the pupils are admitted, only after the satisfactory completion of their studies, either as working weavers or overseers. This establishment also becomes a valuable medium between inventors and manufacturers, by undertaking to examine and report upon all new inventions and improvements brought before it, and by helping to secure their immediate adoption in all cases where their merits are duly recognised.

No sketch of technical education in France would be complete without some notice of the Conservatoire des Arts et Métiers. This is a vast museum of the accumulated industry of the country, and in connection with the Exhibition, merits a very careful inspection. The principal objects in the collection are, models of the tools and operations employed in almost every trade; models of building construction, machinery and engineering; also, models illustrating all sorts of manufacturing processes, such as the working of iron and the reduction of metallic ores. The collections of physical and chemical apparatus are very valuable, and serve to illustrate in a remarkable degree the progress of scientific discovery.

NOTES AND QUERIES.

Extraction of Grease by Bisulphide of Carbon.—If "C. L. W." will apply to "E. S.," care of Messrs. Parry and Garnham, 12a, Wellington Street, London Bridge, he may obtain all necessary information regarding this subject, also concerning the apparatus employed for the purpose.

Extraction of Grease by Bisulphide of Carbon.—If your correspondent, "C. L. W.," will apply to Mr. Jesse Fisher, Ironbridge, Salop, I have no doubt he may obtain the information that he requires.

Extraction of Grease by Bisulphide of Carbon.—"C. L. W." will find whatever information he desires on this subject by inspecting, at the library of the Commissioners of Patents, Southampton Buildings, Chancery Lane, the numerous patents taken in reference to this subject, amounting to several dozens; it does not appear that, either in this country or abroad, there is any process in use which is *not* subject to Patent right.

Extraction of Grease by Bisulphide of Carbon.—Having had considerable experience in the employment of bisulphide of carbon as a solvent in manufacturing operations, I think I might be of use to your correspondent C. L. W., in respect to the apparatus which he requires.—H. B. CONDY.

Lithia.—I have access to an almost unlimited supply of lepidolite, and wish to know the best means of utilising it for the lithia it contains. Will any of your courteous friends on the other side of the Atlantic, kindly inform me how best to set about this? I see from an announcement in our reprint of your invaluable journal that you will extend the same favours to American Correspondents that you have long granted to English ones.—O. K. Y., Tenessee, U. S. America.

MEETINGS FOR THE WEEK.

MONDAY.—Royal Institution. General Monthly Meeting, 2.
TUESDAY.—Royal Institution. Dr. M. Foster, "On the Development of Animals," 3.
WEDNESDAY.—Society of Arts, 8.
—— Geological. Alfred Taylor, Esq., F.L.S., F.G.S., "On the Quaternary Gravels of England." S. V. Wood, Jun., Esq., F.G.S. "On the Pebble-beds of Middlesex, Essex, and Herts." Dr. J. Schmidt. Communicated by Sir R. I. Murchison, Bart., K.C.B., F.R.S., &c., "On the Eruption of the Kaimeni of Santorin." 8.
THURSDAY.—Royal Institution. Professor Bain, "On Popular Errors," 3.
—— Royal, 8½.
—— Chemical. Mr. C. W. Siemens, "On the Regenerative Gas Furnace as applied to the Production of Cast Steel," 8.
—— Royal Society Club, 6.
—— Astronomical, 8.
FRIDAY.—Royal Institution. Mr. C. Greville Williams, "On the Artificial Formation of Organic Bodies," 8.
SATURDAY.—Royal Institution. Professor Bain, "On Popular Errors," 3.

TO CORRESPONDENTS.

Communications have been received from J. Sillett; W. F. Barrett; W. Bywater; H. Grubb; J. Samuelson; J. Horsley; A. Liversidge; W. Bartlett (with enclosure); Dr. B. H. Paul (with enclosure); J. B. Bird (with enclosure); P. Holland (with enclosure); G. F. Rodwell (with enclosure); H. B. Sykes; W. Kinder; O. Philips; J. Solomon; G. Spencer; G. Benson; The Master and Wardens of the Society of Apothecaries (with enclosures); H. B. Condy Thos. Spencer; Dr. Adriani; H. Gregory; G. Stephens; W. Ladd (with enclosure); E. J, Lloyd; J. A. Wykes; S. A. Sadler; J. Alexander; W. Walker; Dr. M. Reimann; W. Butler; Dr. G. E. Day, F.R.S.; A. Hensman; H. H. Watson; W. Thompson; J. C. Braithwaite; Murray & Heath; F. C. Calvert & Co.; A. McCulloch; G. W. Eccles; Muspratt, Brothers, & Huntley; Sydney Gibbons, Melbourne; D. Marples; E. Yendale; C. Horner; J. C. Brough; G. Fletcher & Co.; A. C. Bowdler; H. Baillière; G. Dickens; H. Streeter: J. Kemp; Dr. Odling, F.R.S.; Dr. Fraser, Nova Scotia.

THE CHEMICAL NEWS.

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ON THE ESTIMATION OF NITRIC ACID.

(PELOUZE'S METHOD.)

By PHILIP HOLLAND.

Of the various processes from time to time proposed for the estimation of nitric acid in combination, there are three which may be said to be in some measure related, viz., those of Pelouze, Schlœsing, and Walter Crum. In the two first the weighed nitrate is boiled out of contact with air with a solution of protochloride of iron and an excess of hydrochloric acid. Under these conditions the N_2O_5 splits up on the one hand into oxygen gas which indirectly by depriving the hydrochloric acid of its hydrogen admits of the conversion of the protosalt of iron into a persalt, and on the other into a lower oxide of nitrogen which escapes. The reaction being a definite one obviously affords two methods for estimating nitric acid. We may either determine the iron oxidised, which forms the basis of Pelouze's process, or we may after Schlœsing reconvert the nitric oxide into nitric acid by the addition of oxygen, and titrate with an alkali in the usual manner. In Crum's method the nitrate together with a sufficiency of sulphuric acid to decompose it is agitated with mercury, whereby a reduction is effected with formation of nitric oxide as in the preceding instance. Of the three processes I have enumerated Schlœsing's is valuable to the analyst, whose object it is to determine our acid when in the presence of organic matter; it does not appear to have come into general use, this is perhaps to be accounted for by the fact that the operations it involves are rather tedious, and somewhat difficult of execution. Since the method of Pelouze is extensively practised, a description of a simple apparatus for its successful prosecution may be interesting. Before I describe my apparatus I will notice the process as it stands at present.

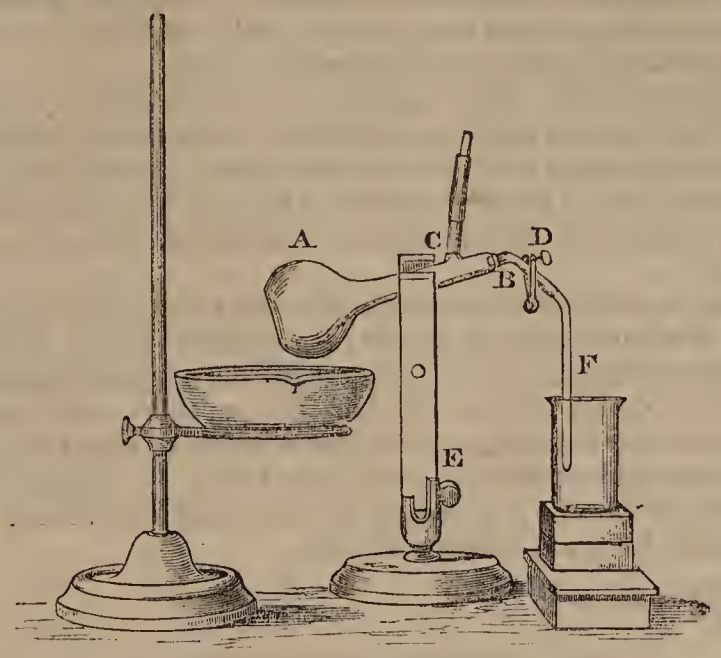
What are its absolute requirements? Two; firstly, that the decomposition of the nitrate shall take place in an atmosphere of which oxygen does not form a part, and secondly that such decomposition shall be complete.

I need not describe the method as set forth in Fresenius; it is familiar to all. The author conducts the operation in a retort, through which he passes a current of hydrogen. Attached to the stem of the retort is a U-tube, containing a little water, which serves as a lute to prevent access of air.

In the modified arrangement I am about to describe, the operation is conducted in vacuo, and at its termination the solution is boiled to expel the nitric oxide, thus avoiding the necessity of employing either hydrogen or carbonic acid.

In the accompanying sketch, A, is a long-necked assay flask drawn off at B, so as to form a shoulder, over which is passed a piece of French india-rubber tube, D, about 6 centimetres long, the other end terminating in a glass tube, F, drawn off so as to leave only a small orifice. On the elastic connector D is placed a screw compression clamp. At C, a distance of 3 centimetres from the shoulder, is cemented with the blow-pipe a piece of glass tube about 2 centimetres long, surmounted by one of French tube rather more than twice that length. The elastic tubes must be securely attached to the glass by binding with wire. After binding, it is as well to turn the end of the connector back and smear the surface with fused caoutchouc, and then replace it.

This device is recommended by Dr. Sprengel for securing an air-tight oint. The wooden clamp E gives support to the flask; the rest of the arrangement requires no explanation.



The analysis of a nitrate is conducted as follows:—A small funnel is inserted into the elastic tube at C, the clamp at D being for the time open; after the introduction of the solution followed by a little water, which washes all into the flask, the funnel is removed, and the former placed in the inclined position it occupies in the figure. The contents are now made to boil so as to expel all air and reduce the volume of the fluid to about 4 or 5c.c. When this point is reached a piece of glass rod is inserted into the elastic tube at C, which causes the water vapour to find egress through F.

Into the small beaker is put 50 c.c., more or less, of a previously boiled solution of protosulphate of iron in hydrochloric acid. (The amount of iron already existent therein as a persalt must be known).

The boiling is still continued for a moment to ensure perfect expulsion of air from F, the lamp is then removed, and the caoutchouc connector slightly compressed with the first finger and thumb of the left hand. As the flask cools the solution of iron is drawn into it, when the whole has nearly receded the elastic tube is tightly compressed with the fingers, whilst the sides of the beakers are washed with a jet of boiled water, which is also allowed to pass into the flask. The washing may be repeated, taking care not to dilute more than necessary or admit air. Whilst F is still full of water, the elastic connector previously compressed with the fingers is now securely closed with the clamp, the screw of which is worked with the right hand. Provided the clamp is a good one, F will remain full of water during the subsequent digestion of the flask.

After heating at 100° for half an hour the flask is removed from the water-bath and cautiously heated with a small flame, the fingers at the time resting on the elastic connector at the point nearest the shoulder; as soon as the tube is felt to expand, owing to the pressure from within, the lamp is removed and the screw clamp released, the fingers maintaining a secure hold of the tube, the gas flame is again replaced, and when the pressure on the tube is again felt, this latter is released altogether, thus admitting of the escape of the nitric oxide through F, which should be below the surface of water in the beaker whilst these manipulations are performed. The contents of the flask are now boiled until the nitric oxide is entirely expelled, and the solution of iron shows only the brown colour of the perchloride. At the completion of the operation the beaker is first removed, and then the lamp.

It now only remains to transfer the solution of iron to a suitable vessel, and determine the perchloride with chloride of tin in the usual way.

A mean of six closely approximating experiments for the percentage determination of N_2O_5 in pure nitre gave 53·53 per cent instead of 53·41. The process is easy of execution, and gives satisfactory results. The points requiring attention are that the apparatus should be

capable of retaining a sufficiently perfect vacuum during the operation; this condition is fulfilled by the use of suitable elastic tube and clamps. What is known as French tubing serves the purpose well; its sides are thick, and its material free from metallic oxides.

It is advisable when making the digestion at 100° to place the flask in cold water which is afterwards raised to the boiling point; if this be not done, but on the other hand the flask is heated at once, a violent bumping ensues, and portions of the liquid are projected into the tube at c.

I find it necessary to standardise the chloride of tin at the time the analysis is made; it appears to be liable to constant change. 10 c.c. of my solution originally equalled ·04162 of N_2O_5; the second day the numbers were ·03947; whilst on the third day ·0364 was a mean of two titrations. Particular precautions were not taken to preserve the solution, otherwise perhaps the numbers would not have presented so great a dissimilarity.

Chorley, April 23rd.

ON MUSICAL AND SENSITIVE FLAMES.

Abstract of a Lecture delivered, at the invitation of the Council, before the Dublin Royal Society, on January 3, 1868, by W. F. BARRETT, *Natural Science Master at the International College, London, &c.*

ONE of the earliest natural facts which arrests the attention of a thoughtful mind is the stability of the wonderful universe in which we live. This permanency is, nevertheless, the product of incessant change; for nothing is absolutely at rest. The secret of the stability of nature, its unresting repose, is found in the fact that the motion is regular—the change is periodic. Atoms, as well as planets, have their period of revolution. Hence, sooner or later, in the physical world at any rate, phenomena repeat themselves. Like a vast living body the throbbings of the universe announce the accord of its varied parts. This rhythmic flow of nature constitutes most literally the "Music of the Spheres." Not this, but a less ethereal music, I have had the honour of being invited to bring before you this afternoon.

The so-called musical or singing flames were discovered nearly a century ago by a native of this city, Dr. Higgins, who found, that when a flame of hydrogen was burning within a glass tube the flame emitted a musical note. The experiment was repeated; and it was moreover shown that glass tubes were not necessary, for similar sounds, though of different quality, were produced when metal or pasteboard tubes were employed. Neither was it necessary to use hydrogen, for a small flame of common coal gas gave a musical note when burning within a tube.

The cause of this phenomenon had been investigated by many, but most successfully by an illustrious man who had lately passed from among us—a man who had left behind him a name as good as it was great, and who possessed a mind as simple and child-like as it was sagacious and profound—the late Professor Faraday. This subject had been one of Faraday's early *flames*. The cause was shown to be due to the fact that the gas, in issuing from the burner, did not burn silently. It rustled in passing through the orifice of the burner, and in burning it made a continuous series of inaudible explosions. This was proved by several experiments, for, by suitable means both these causes could be exalted so as to become sensible. The resonance of the tube placed over the flame renders audible all the sounds of a certain pitch made by the gas. By a series of experiments it was then proved that any noise, if made regularly and with sufficient rapidity, was converted into a musical note. Thus rough and rude taps and hard and harsh explosions could be chased into perfect melody by mere rapidity of succession.

The condition of the flame when burning within the tube was shown by a moving mirror. It was seen that when the flame was silent, and the mirror moving, a band of light was produced, but when the flame was sounding, this luminous ribbon was broken up into a series of disjointed images of flame. The effect of lengthening the tube in which the flame was burning was next shown, and a series of gas jets burning within glass tubes of varying length gave a corresponding series of musical notes of varying pitch. By placing the finger upon the top of these tubes the sound could be quenched, and thus a novel musical instrument could be constructed. From glass tubes the lecturer passed on to show the effects of flames burning within extremely long tin tubes. Within a tube 6 ft. long, and about 1½ in. in diameter, the flame of a large gas burner gave a loud unmusical roar. By adding to the end of this tube a glass chimney, it was seen that when the flame was sounding it was broken up into wild confusion. By enclosing a still larger gas flame from a huge Bunsen's burner within a tube 18 ft. long and 3 in. diameter, a deep roar was obtained, intermingled with loud reports similar to the discharge of musketry.

Returning once more to the gentler music of the small glass tubes, two flames, enclosed in their respective tubes, were taken and made to emit notes of the same pitch. This point was gained by shifting to and fro a paper slider, which moved stiffly at the upper extremity of one of the tubes. When the notes were nearly in unison a series of intermittent sounds or *beats* were obtained, due, as is well known, to the mutual extinction at certain intervals of the two sounds. Corresponding beats were obtained from two organ-pipes and two tuning-forks nearly in unison. One of these tuning-forks, mounted on its resonance case, being silent, the other, unmounted, was now struck, and its prongs brought near to, but not touching those of the first fork; at first no sound could be heard, but by degrees the unmounted fork transferred its motion to the mounted one, and the sound of the latter slowly welled forth. The sound of the voice can thus be transferred to the strings of a pianoforte, and in the same way a flame can be made to accept and resound to a note of the proper pitch. This was illustrated as follows:—A singing flame, by adjusting the paper slider, was tuned to the note of a certain fork; the tube was then raised slightly, so that the sound could be quenched by momentarily placing the finger on the top of the tube. On now striking the fork, and holding it over a resonant jar, the flame instantly started into song. The same effect was shown by the syren, and also by the human voice. Retreating to some distance from the flame, the latter could be made to respond at pleasure, by pitching the voice to the proper note, whilst it remained utterly unaffected by any note not in unison with itself. Musicians would find such a flame a faithful monitor in training the voices of their pupils.

In the last experiment we have really a *sensitive* flame; but this name is now applied to another discovery, which was made in the following manner:—Two years ago (December, 1865), whilst engaged in some acoustic experiments, the lecturer had observed that every time a shrill note was produced, a tall tapering gas flame in his vicinity was singularly affected; the flame shrinking every time the note was sounded. That observation led to further experiment and enquiry, the result of which has been the discovery of the conditions of success for obtaining flames sensitive to the slightest sound. Some months after the above observation, Professor Tyndall took up the subject, and having largely added to its interest and importance, offered an explanation of the phenomenon in a lecture delivered at the Royal Institution, in January, 1867. At this lecture the discovery was first published, and the name given to "Sensitive Flames." Subsequently the lecturer

had proposed a fuller explanation, and had discovered that not only flames, but all gases could be rendered extremely sensitive to sound—the track of the gas being marked by mixing it with smoke.† This historical notice would be unjust without referring to an observation made ten years ago in America by Professor Leconte. That physicist had noticed that certain sustained sounds in an instrumental concert caused a very perceptible movement of the ordinary gas flames in the room. This observation is really the germ of the more wonderful effects afterwards independently discovered by the lecturer. Though Professor Leconte was the first to publish the fact, in 1858, it appears that, previous to this date, artisans had frequently noticed the phenomenon as resulting from the shrill sounds of their work; and several musicians have informed the lecturer that the same effect has been one they have commonly observed.

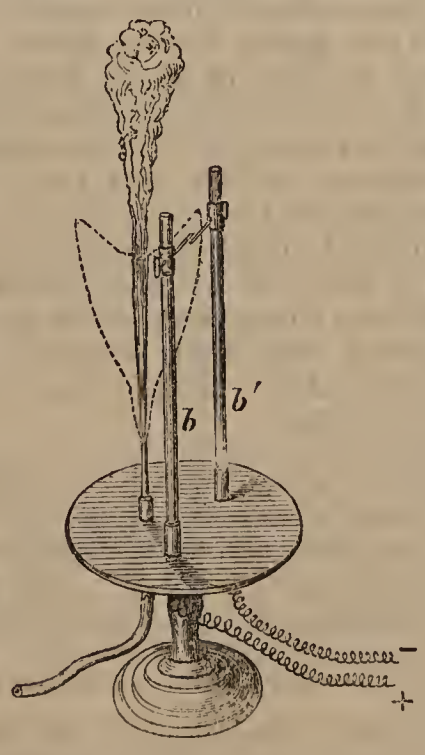
Turning now from scientific history to experiment, the lecturer showed various kinds and degrees of sensitive flames. First, a "batswing" flame, which, under the ordinary gas pressure, moved slightly at the sound of a whistle, but thrust out long tongues of fire when the pressure was increased by urging the gas from a holder. This increased pressure was always necessary to obtain the more sensitive flames, for a reason that will be understood directly. A jet of gas, issuing from a V-shaped orifice, was shown to be quite insensible to sound until the flame reached a height of 10 or 12 inches, and then, at the sound of certain high notes, the flame shortened and spread out into a fan-shape. Whistling to this flame in one key had no effect, while in another the effect was very marked. Playing an air upon a so-called bird-organ, the flame selected the high notes, and promptly shortened at their recurrence.

The probable cause of the sensitiveness of these flames was then alluded to. The impact of air evidently had nothing to do with the phenomenon. This was strikingly shown in the following experiment:—By tapping a membrane stretched over the mouth of a large tin funnel, a puff of air could be driven with such force from the narrow extremity that a candle was easily extinguished some 12 ft. away. Directing this puff of air against the sensitive flame, it was seen that the flame moved violently, but was utterly unaffected when the puff was driven either to the right or left. This should also be the case if in former experiments it were the impact of the air, and not the sound that produced the effect. But it was at once seen that when the lecturer whistled, at the same time slowly turning round, the flame still continued to shrink, and was almost as powerfully moved when the back was turned to the flame. The effect, then, is solely produced by the wave-like to and fro motion of the sonorous pulses. As first indicated by Professor Leconte, a gas flame, to be sensitive, has to be brought near its point of roaring; it then stands, according to Dr. Tyndall, as it were on the brink of a precipice, over which the sound pushes it. Agreeing with this explanation, that a sensitive flame is a body in a state of unstable equilibrium, the lecturer supplemented it by comparing the flame to a resonant jar; the flame, as was proved by a moving mirror, being in a state of rapid isochronous vibration when under the influence of external sound. The actual shrinking of the flame was due to an increase in the velocity of the current of gas, which was possibly brought about by an external sound throwing the pipe that conveys the gas into a state of vibration, which would thus narrow the channel of the gas passage: the change in the aspect of the flame being largely modified by the shape of the burner.

Whatever may be the complete explanation of the phenomenon, there can be no doubt that in a somewhat similar manner other objects besides flames are also sensitive to slight external impulses. Thus, many chemical compounds, as, for example, fulminating powders, are in a state of unstable equilibrium. The so-called "Rupert's Drop," which, when scratched, flew into a thousand fragments, is another instance of this kind; and some of the most eminent physicists are inclined to believe that the surface of our sun is in a somewhat analogous sensitive condition. From inorganic things we may travel on to organic, for we have evidence that there also exists in organised structures a more or less sensitive state at certain times. Thus, our wonderfully complex bodies, by disease or nervous derangement, are often thrown into an abnormal state, and when in that condition are sensitive to the slightest stimuli, if of the proper kind. This may possibly be the foundation for whatever truth there is in the science of homœopathy, the body being sensitive to a feeble influence, similar in kind to the disease under which it is suffering.

Here some may ask—"Of what good are these speculations, and to what practical end can these experiments be turned?" This observation, permit me to remark, is wholly improper. There is something nobler in life than the accumulation of wealth, and a higher end to experiment than its mere monetary value; for all accession to knowledge must finally benefit the world. This ever intrusive exclamation *cui bono* is a serious check to the advancement of knowledge, for it disheartens those who are making nature yield up her secrets, and it damps the ardour of every searcher after truth. Allow me to illustrate my meaning. Imagine that when enchanted by the performance of some well-executed opera or oratorio, a companion by our side were to say—"Well, after all, of what good are these fine sounds: to what practical end can you turn this music?" Should we not instantly condemn a speech so characteristic of a sordid and sensuous mind? And when the student of nature is listening with admiration and even awe to the sweet, though silent, music sung to him by every object of his diligent study—by air and water, by flowers and flames—he is conscious that he bows before an oratorio as far above that of Handel as the works of the Creator are superior to the composition of the creature.

Still, however, the lecturer was enabled to show a practical application of these sensitive flames. Attention was drawn to the fact, that the flame shortened and spread out laterally under the influence of a whistle. Advantage was taken of this peculiarity to construct an instrument which may be turned to some practical use. The instrument consists of two sliding brass rods, *b b'* (see diagram); attached at right angles, to the summit of one is a compound metallic ribbon, consisting of thin layers of silver, gold, and platinum, welded together. This arrangement



expands unequally by heat, by so doing it swerves aside, and is thus brought into contact with a platinum point projecting from the top of the second brass rod, which is fixed about half an inch from the free extremity of the compound metallic ribbon. Connected with the two brass rods is an electric battery, associated with which is an electric bell, placed in a far distant part of the room. The bell will immediately ring if the electric circle be complete, but at present there is a gap in the circuit between the metallic ribbon and the platinum point. "I now ignite," said the lecturer, "a sensitive flame, which, in its ordinary state, burns at about 2 inches from the compound metal ribbon. I retreat some thirty feet from the flame, and whistle; the flame at once responds; it shrinks and spreads out sideways. By so doing it comes in contact with the metal ribbon; the latter instantly springs aside at the warm touch of the flame, strikes against the

† *Philosophical Magazine*, March and April, 1867.

platinum point, completes the electric circuit, and there you hear that distant bell answering me every time I whistle." In the same way, at any hour of the night, the crying of a child in its cot would automatically announce itself in its parent's room. By a somewhat similar arrangement, using, however, a different burner, a burglar, filing the iron-cased doors of a jeweller's shop could be made to sound an alarm bell; and it is even possible, by making use of the propagation of sound through water, the reflection of that sound through a trumpet immersed in the water, and its conduction to a sensitive flame, shut out by non-conductors of sound from the noises on board ship, that an arrangement might be constructed by which the approach of a vessel in a fog might be detected by ringing a bell in the captain's cabin. It is not, however, my province to develope such inventions. With diffidence I throw out these suggestions, which may, I trust, by the practical mind be in some way turned to the public good.*

The lecturer had reserved for the conclusion a flame wonderfully sensitive to the slightest noise. The burner which gave this flame was formed of steatite, and consisted of a single circular orifice, through which the gas was forced from a large holder in the lecture-room, with greater pressure than could be obtained from the main. The flame was now fully 2 ft. in length, and observe, said the lecturer, how delicate and fragile a thing it appears to be; for at the slightest noise it drops down a foot.‡ The jingling of this bunch of keys, the crumpling of this paper, the dropping of a small coin, are more than sufficient utterly to break up its height and symmetry. This flame makes no response to the vowels, O, U, nor to the labials, but it energetically responds to the sibilants. Repeating the stanza—

"Roll on, O rill, for ever!
Rest not, lest thy wavelets
Sheen as shining silver—
Shrink and sink to darkness."

The flame is unmoved by the first line, but emphatically bobs at the sound "rest" and "lest," and admirably suits its action to the words of the last line, for, when shrinking, the light of the flame almost disappears. So sensitive is this flame, that even a chirp made at the far end of the room brings it down more than a foot. Like a living being, the flame trembles and cowers down at a hiss—it crouches and shivers as if in agony at the crisping of this metal foil, though the sound is so faint as scarcely to be heard; it dances in tune to the waltz played by this musical box—and, finally, it beats time to the ticking of my watch. How wonderful are all these facts! And the more we know of them the more wonderful do they appear; for this astonishing change in the aspect of the flame is produced by an infinitesimal portion of those almost inaudible sound-waves, already enfeebled by their distance from the flame. Looking back on these, and innumerable other wonders revealed by physical science, and looking forward on that vast region which remains to be explored, do we not feel ourselves sinking to utter significance by contemplating the mysteries by which we are surrounded; whilst at the same time are we not conscious there is that within us still more wonderful than that without—a consciousness which lifts itself above all phenomena, grand and mysterious though they be?

* Several of the laws of acoustics may be illustrated to a large audience by means of the sensitive flame next to be described. Placing, for example, a watch in the focus of one concave mirror, and a sensitive flame in the focus of a distant second one, the reflection and convergence of sound is seen by the regular beating of the flame to every tick of the watch. The decay of sound, and the prevention of that decay by tubes, can also be shown in a similar way. Many other illustrations of acoustical phenomena at once suggest themselves. I hope shortly to publish some further applications of this novel *phonoscope*.—W. F. B.

‡ It is easy to see how a modification of the instrument just described can be, and has been, applied to this flame. The diminution of heat arising from the falling of the flame can cause the compound ribbon now placed above the flame, to *recoil* upon the other battery connection: or, another arrangement may be employed, an air-thermometer having a bent stem, in which are sealed asunder platinum terminals; the circuit being closed by the backward movement of mercury in the tube, owing to the contraction of the air in the bulb.

EGG-ALBUMEN, FROM A CHEMICAL POINT OF VIEW.

By JOHN SPILLER, F.C.S.

On a recent occasion it has been proposed to employ the white of egg in a naturally moist condition as the standard of comparison in estimating the quality, or degree of impurity, of potable waters, as regards their nitrogenous organic constituents. Variations in the amount of nitrogen contained in the white of egg have, however, been admitted, and the proportion of water, or, in other words, the weight of dry albumen, is suspected to be subject to variation. Whilst engaged in some experiments upon this new method of water analysis, in which, as I have said, albumen is taken as the starting point, and its nitrogen (or a known fraction of it) is evolved and estimated in the form of ammonia, it appeared to me desirable at once to ascertain whether or not the moist contents of the egg lost water by evaporation through the calcareous shell; for if this be the case to any appreciable extent, the constitution of the albumen within cannot possibly remain for any length of time fixed and definite, although the egg itself may, during this period, be perfectly preserved from organic decomposition.

My affirmative anticipations on this point were based upon the circumstance that a new-laid egg exhibits no cavity on breaking the shell, whilst a stale one always contains a considerable air-space; however, to set the question at rest, I made the following experiments:—The weight of a hen's egg was exactly taken, and it was then supported upon a wire tripod-stand, so that the air might have free access to the whole external surface of the shell. Upon weighing the egg after an interval of twenty-four hours, it had lost exactly one grain, and this ratio of loss by evaporation remained tolerably constant during several days. As, however, the experiment was made in the winter time, and during a season of wet weather, I repeated it under somewhat modified conditions. Two new-laid eggs of large size were taken, and, after their weights had been accurately determined, they were supported in a similar manner within a bell-jar, resting on a flat glass plate, with a dish of concentrated sulphuric acid to absorb the water given out by the eggs. The whole arrangement (that is to say, the desiccator and its contents) was placed in a room the temperature of which was maintained pretty uniformly between 55° and 60° Fahr., and there left for six weeks, the diminution of weight being frequently observed during this interval. In all, ten weighings were taken, and the results proved that 100 gr., or more, of water can be abstracted under these circumstances, whilst the loss in the intermediate periods followed a diminishing scale, but nearly coincided with the several intervals of time. The average loss of water by evaporation through the shell may be stated, for the two eggs operated upon, to have been at the rate of 2 and 2½ gr. respectively per diem. Upon breaking the eggs at the end of six weeks, one was found perfectly sweet and good, with the envelope of the yolk unbroken, but the second and smaller one was discoloured next the shell, and the albumen had become slightly decomposed. The last results with this egg were consequently disregarded.

The details of the experiments are quoted, for they serve to show the proportion of yolk to white at the final stage; ratio of water evaporated to total liquid contents; and the exact weight, in each instance, of the shell with its lining membrane, after careful washing with dilute ammonia and subsequent drying in the air:—

Egg—No. 1.

Original weight (entire)	975·0 gr.
Loss of weight in six weeks (water) ..	100·0 ,,
Shell and membrane	99·2 ,,
Yolk	317·2 ,,
White (by difference)	458·8 ,,

Egg—No. 2.

Original weight (entire)	930·6 gr.
Loss of weight in three weeks (water)	41·6 ,,
Shell and membrane	85·2 ,,
Yolk and white	803·8 ,,

It will here be noticed as an anomaly that the heavier shell of No. 1 egg permitted a faster rate of evaporation through its substance than No. 2. The latter appeared, however, upon inspection, to have a smoother external surface, and to be stronger and more compact in structure throughout.

The following table shows the actual amount of water lost by evaporation in the two instances for the intervals of time specified in the first column :—

Loss of Weight in Desiccator (Water evaporated).

Intervals of time. Days.	No. 1 Egg. Gr.	No. 2 Egg. Gr.
1	3·4	2·6
2	6·9	4·7
7	17·3	14·0
14	34·3	27·6
21	51·1	41·6
42	100·0	—

Ratio of Water Evaporated to Total Liquid Contents of the Egg (per cent).

Time.	No. 1.	No. 2.
In two weeks	3·9	3·3
three ,, ...	5·8	4·9
six ,, ...	11·4	—

It would be difficult to prove whether or not the water lost by evaporation from the white is partly compensated by an accession of water, by diffusion, from the yolk. No colouring matter travels outwards unless an organic decomposition sets in, when all the natural barriers are destroyed, and the several parts of the egg become merged. I am inclined to think that this re-distribution of water actually occurs, since the *sac* of the yolk appears wrinkled in a stale egg, as though by loss of a portion of its liquid contents.

The composition of fresh egg-albumen may be said to vary between the following limits :—

Water	88 to 85 per cent.
Dry albumen (containing nitrogen 1·55 to 1·75 per cent)	12 to 15 ,,

Other analyses have been recently published, in which the nitrogen amounts to 1·81 per cent and upwards, with the minimum proportion of water. It is probable that these latter results were obtained with eggs which had been longer kept in stock.—*The Photographic News*.

RELATIVE VALUES OF FRENCH AND ENGLISH WEIGHTS AND MEASURES.

By A. A. FESQUET.

I see in the column of Notes and Queries (American reprint, March, 1868) that one of your readers wishes some calculations showing the relative value of French and English weights and measures. Tables for this purpose are to be found in many technical works, but I think they are not so complete as those I join to this letter. You will see by the number of different values of the gramme, and of the carat weight, that "the doctors or the standards disagree."

The value I have adopted for the gramme is 15·438395 troy grains, calculated from 1 pound avoirdupois = 7000 grains = 453·4148 grammes.

It may be that these data, or my calculation, are incorrect; if so, I wish to be corrected. It would be a satisfaction to many of your readers to know the exact value in grammes of the English standard pound avoirdupois, and how many troy grains it contains. Most authors say 7000 grains; however, I have seen 7004 grains printed somewhere.

The use of the metric system is extending more and more; but the abbreviations in writing are not always short and corresponding to the simplicity of the system, therefore I have added to the tables a series of symbols for abbreviations, which will be understood by looking at them. This system is based on the same principles followed in chemical symbols. Each unit and its prefixes are indicated by their first letter, with this difference, that a capital letter is used when the prefixes are those increasing the unit.

We use already: kgm. = kilogrammetre; we might have as well, kg°. = kilogramme degree, g = gramme, instead of gm. or grm.

Philadelphia, March 23rd, 1868.

TABLES

SHOWING THE RELATIVE VALUES OF FRENCH AND ENGLISH WEIGHTS AND MEASURES, &c.

Measures of Length.

Millimetre	=	0·03937	inch
Centimetre	,,	0·393708	,,
Decimetre	,,	3·937079	inches
Metre	,,	39·37079	,,
,,	,,	3·2808992	feet
,,	,,	1·093633	yard
Decametre	,,	32·808992	feet
Hectometre	,,	328·08992	,,
Kilometre	,,	3280·8992	,,
,,	,,	1093·633	yards
Myriametre	,,	10936·33	,,
,,	,,	6·2138	miles

Inch ($\frac{1}{36}$th yard)	=	2·539954	centimetres
Foot ($\frac{1}{3}$rd yard)	,,	3·0479449	decimetres
Yard	,,	0·91438348	metre
Fathom (2 yards)	,,	1·82876696	,,
Pole, or Perch ($5\frac{1}{2}$ yards)	,,	5·02911	,,
Furlong (220 yards) ..	,,	201·16437	,,
Mile (1760 yards)	,,	1609.3149	,,
Nautical mile	,,	1852	,,

Superficial Measures.

Square millimetre	=	$\frac{1}{645}$th of a square inch	
,, ,, ...	,,	0·00155	,, ,,
,, centimetre	,,	0·155086	,, ,,
,, decimetre	,,	15·5086	,, inches
,, ,, ...	,,	0·10769	,. foot
,, metre or centiare	,,	1550·86	,, inches
,, ,,	,,	10·7698	,, feet
,, ,,	,,	1·196033	,, yard
Are	,,	1076·98	,, feet
,, ...	,,	119·6033	,, yards
,, ...	,,	0·098845	rood
Hectare	,,	11960·33	,, yards
,, ...	,,	2·471143	acres
Square inch	,,	645·109201	sq. millimetres
,, ,, ...	,,	6·45109	,, centimetres
,, foot	,,	9·2903	,, decimetres
,, yard	,,	0·836097	,, metre
,, rod or perch ..	,,	25·291939	,, metres
Rood (1210 sq. yards) ..	,,	10·116775	ares
Acre (4840 ,, ,,) ..	,,	0·404671	hectare

Measures of Capacity.

Cubic millimetre	=	0·00006102	cubic inch
,, centimetre, or millilitre	,,	0·061029	,, ,,
10 cubic centimetres, or centliitre ..	,,	0·61029	,, ,,
100 ,, centimetres, or decilitre ..	,,	6·10295	,, inches
1000 ,, centimetres, or litre	,,	61·0295688	,, ,,
,, ,, ,, ..	,,	1·760773	imperial pint
,, ,, ,, ..	,,	0·2200967	,, gallon
Decalitre	,,	610·295688	cubic inches
,, ...	,,	2·2009668	imp. gallons
Hectolitre	,,	3·5317	cubic feet
,, ...	,,	22·009668	imp. gallons
Cubic metre, or stere, or kilolitre	,,	1·308	cubic yard
,, ,, ,, ..	,,	35·3171	,, feet
Myrialitre	,,	353·171	,, ,,

Cubic inch	=	16·3855	cubic centimetres
,, foot	,,	28·3159	,, decimetres
,, yard	,,	0·764520696	,, metre

American Measures.

Winchester, or U.S. gallon (231 cub. in.)	=	3·785085	litres
,, ,, bushel (2150·42 ,,)	,,	35·23603	,,
Chaldron .. .. (57·25 cub. feet)	,,	1621·085	,,

British Imperial Measures.

Pint ($\frac{1}{8}$ gallon) ..	=	0·567932	litre
Quart ($\frac{1}{4}$,,) ..	,,	1·135864	,,
Imperial gallon ..	,,	4·54345797	litres
Peck (2 gallons) ..	,,	9·0869159	,,
Bushel (8 gallons)	,,	36·347664	,,
Sack (3 bushels) ..	,,	1·09043	hectolitre
Quarter (8 bushels)	,,	2·907813	hectolitres
Chaldron (12 sacks)	,,	13·08516	,,

PETROLEUM FUEL FOR STEAM SHIPS.

The considerations which should govern the discussion of the question of the substitution of petroleum in place of coal for the generation of steam may be classed under the following heads:—First, the comparative heating or steam-producing capacity of the two substances, weight for weight; second, the comparative cost, weight for weight; third, the comparative cost of of attendance in the fire or boiler-room; fourth, the cost and durability of the apparatus necessary for burning the petroleum (under this head, with the retort system, the air-pumps for forcing air into the retort, and, if it is not by gravity, the pumps for forcing in the petroleum should be included); fifth, the comparative difficulties encountered and space required in stowing the two fuels.

With respect to the comparative heating power of anthracite coal and petroleum, let us first mention their absolute theoretical steam-generating capacities; that is, if the combustible elements in both cases could be completely consumed, and all the heat thus generated be utilised in making steam. According to the accurate and elaborate experiments of the eminent chemists, MM. Favre and Silberman, a pound of carbon completely burned, so as to make carbonic acid (that is, by combining with $2\frac{2}{3}$ lbs. of oxygen), will evaporate 15 lbs. of water from a temperature of 212° Fahr.; and similarly, 1 lb. of hydrogen, by combining with 8 lbs. of oxygen, will evaporate 64·2 lbs. of water from the same temperature. The chemical composition of petroleum is said to be $C_{12}H_{12}$; or, in other words, it is composed of 12 atoms of carbon and 12 atoms of hydrogen. As an atom of carbon exceeds an atom of hydrogen six times in weight, it is seen that in a pound of petroleum 6 parts are carbon and 1 part hydrogen. The absolute theoretical evaporative power of a pound of petroleum, in pounds of water, from 212°, is therefore $\frac{6}{7} \times 15 + \frac{1}{7} \times 64{\cdot}2 = 22{\cdot}02$ lbs. Allowing that there is 20 per cent of non-combustible matter in anthracite coal, there remains in a pound $\frac{4}{5}$ of a pound of carbon; hence $\frac{4}{5} \times 15 = 12$ lbs., the weight of water that theoretically may be evaporated from 212° by a pound of anthracite, under these conditions. Consequently, the absolute heating power of petroleum is 1·835 times greater than that of a pound of anthracite.

Now let us look at the comparative cost: the average price of crude petroleum is about 21 cents per gallon, and coal about 5 dollars per ton; in other words, the petroleum costs 3 cents per lb., while the cost of the coal is ·23 of 1 cent. Hence, a pound of petroleum, while it has but 1·835 times more absolute heating power than a pound of anthracite, costs 13 times more than the coal; that is, the heat produced by petroleum costs over seven times more than the heat produced by coal. The greater cost of the petroleum is therefore decisive against its economical use as a steam fuel, and this, too, without considering the complication of the apparatus necessary for burning it. With regard to the comparative cost of the attendance on the fire, economy in this particular is decidedly in favour of the petroleum. It is probable that a steam vessel which requires, say 24 men in the fire-room, could dispense with at least 14 of them by the use of petroleum.

So far, we have assumed that not only was all the heat generated that is attainable from the perfect combustion of the two fuels, but that the whole of this heat was utilised in the evaporation of water. It need scarcely be remarked that such results are quite out of the question in practice. The experiments carried on with the machinery of the U. S. gun-boat, *Palos*, in Boston harbour, throw much light on the evaporative power practically attainable with the two fuels. The petroleum in these experiments was burned by the contrivance known as Foote's retort apparatus, and the result of these trials was, that if the evaporative power of the anthracite is represented by 1, that of the petroleum is 1·38—a result much inferior to that which we have shown is due to the combustible elements of the two substances. This, perhaps, is due to the fact that it is more difficult to completely burn a hydrocarbon fuel than it is to burn one composed almost wholly of carbon. If we modify the calculations we have before entered into, according to the results attained with the experiments on the *Palos*, conducted under the immediate supervision of the petroleum retort inventor, it will be seen that the result is still further against the economical use of the oil as a steam fuel.

We have now to consider the cost and durability of the apparatus for burning the oil. As to the cost, let us look at the expense of applying the retort system to vessels of the *Wampanoag* class, which have 58 furnaces in their boilers. It is estimated that such an apparatus would cost at least 250 dollars for each furnace, or 250 × 58 = 14,500 dollars, and when to this is added the cost of the air-pumps for pumping air into the retort, the engines for driving them, together with the pumps for forcing the petroleum into the retorts, and the paraphernalia of cocks, valves, and pipes connected with this machinery, we think it is safe to assume that the application of the retort system to such a ship would cost at least 60,000 dollars. When we come to look at the durability of this apparatus, the case is still more strongly against the petroleum; and when the high temperature to which the apparatus is exposed is considered, it becomes evident that portions of it will require constant renewal. But the most fatal practical objection to the retort system remains to be mentioned: it is the deposition of carbon, coke, and incombustible matter within the retorts and pipes. In experiments with a retort apparatus, the pipes and passages became so choked in less

than 48 hours that the fire went out, and could only be renewed by taking the fixture apart and cleaning it.

With regard to storing petroleum on board ship, another serious difficulty is encountered. A great portion of the crude oil evolves, at ordinary temperatures, an inflammable gas; and this gas, when mixed with atmospheric air, forms an explosive compound, which instantly takes fire when brought in contact with flame. Other samples do not evolve this gas until the temperature is raised from 80° to 100° Fahr.

These facts point out at once the extraordinary care that must be taken in storing this substance on board ship, in order to guard against accidents of the most frightful character. It therefore seems clear that a proper regard for safety demands that the tanks containing the petroleum should be immersed in water; when the weight of these tanks and cisterns is borne in mind, together with the bulk of petroleum and coal composition (a cubic foot of the former weighs 50 lbs., and a cubic foot of anthracite, as stored in bunkers, weighs 52·5 lbs.), and also their relative heating values, which may be set down as 1·4 for the former and 1 for the latter, it may be with safety assumed that there would be a saving in weight and space of not over 30 per cent in stowage capacity by substituting the oil for the coal. It is therefore evident, from what we have said, that even assuming that the oil and the coal can be burned with equal facility, and with an equal degree of liability to derangement, in steam-boiler furnaces, the excess of the heating power of the petroleum over that of the oil is so very much less than its excess of cost, that there is not the slightest probability, as long as these ratios exist, of petroleum ever taking the place of coal as a steam fuel.

When we look at the great complication and danger that must be added to steam machinery in order to burn the oil, and the liability to derangement and the want of durability, it is not likely that any prudent steam navigation company would allow it to be employed in their vessels, even if they could find an engineer to recommend it.—*American Artisan.*

PROCEEDINGS OF SOCIETIES.

PHILOSOPHICAL SOCIETY OF GLASGOW.

Ordinary Meeting, April 29th, 1868.

Dr. F. H. Thompson, President, in the Chair.

Amongst the papers read at this meeting, there was one "*On the Sources of Sulphur used in the Manufacture of Oil of Vitriol or Sulphuric Acid*," by Mr. James Mactear, F.C.S., of the alkali department, St. Rollox Chemical Works. After making some few preliminary remarks,

Mr. Mactear proceeded to say that the quantity of oil of vitriol manufactured in this country exceeds 500,000 tons per annum, and of this quantity about 320,000 tons are used in the conversion of common salt into sulphate of soda, for the production of alkali by Leblanc's process, the remainder being taken up chiefly in the manufacture of artificial manures. The sulphur consumed last year, in making oil of vitriol, amounted to 160,000 tons, but of this quantity only from 10,000 to 20,000 tons consisted of brimstone; the remainder is obtained from pyrites, of which upwards of 375,000 tons were burned. The brimstone is used chiefly in the manufacture of "sale acid," and this again is used mostly for bleaching purposes which require a very pure acid, that made from pyrites being always more or less contaminated with arsenic and other impurities of a detrimental character.

Since the year 1851 the price of brimstone has been very high, owing to the great quantity of sulphur required in the treatment of the vine disease.

Up to the year 1856 all the pyrites used was obtained from Cornwall and Ireland, except a small quantity of "coal brasses" found in the coal-fields. In that year some cargoes of Spanish pyrites, containing a small percentage of copper, were imported and used by the Tyne manufacturers, who liked the ore very much, on account of the high percentage of sulphur which it contained; while it is burned at as low a cost per ton as the ore of low strength. At least one half of the ore now used is Spanish cupriferous pyrites. The author illustrated the economy of working pyrites by stating that Irish ore contains about 35 per cent of sulphur, while Spanish ore contains as much as from 45 to 50 per cent; and, assuming that the cost of burning in both cases is 2s. per ton, in the one case 35 parts, and in the other from 45 to 50, are burned off for the same amount. But it is found that the refuse contains, in both cases, about 5 per cent of sulphur on the average, so that there is actually burned off 86 per cent of the sulphur of the Irish ore, and 89 per cent of that contained in Spanish ore of 45 per cent strength, thus giving a clear gain of 3 per cent.

Mr. Mactear then referred to certain disadvantages attending the use of pyrites instead of Sicilian sulphur:—

First. A greater amount of chamber space is required for burning pyrites than for sulphur. This is usually taken to be in the ratio of 45 to 30, and is due to the fact that the iron of the ore is peroxidised during combustion, and therefore uses extra oxygen and nitrogen (which passes on unchanged); and for the extra amount of both of these gases chamber space must be provided.

Second. The arrangements for burning pyrites are much more costly than for burning sulphur; the heat is greater, and therefore there is more wear and tear of the chambers and connecting-pipes.

Third. There is always a larger percentage of nitre required with pyrites than with brimstone.

Fourth. Owing to various causes, the acid produced is of inferior quality.

Fifth. It involves more labour to work pyrites than to work brimstone; and there is a large amount of refuse—approximately three-fourths of the weight of the raw ore—for which room has to be provided.

Sixth. In burning pyrites, and during transit, much *small* is made, and this cannot be burned properly unless in a separate furnace. The most common form is a muffle furnace; in it the "smalls" are roasted, with free admission of air, but the quantity passed into the chamber is so great, in proportion to the sulphur, that this method is objectionable.

Taking all these disadvantages into account, it is found that, in making ordinary sulphuric acid, 1 ton of sulphur, in the form of pyrites, costs only £4 10s., while Sicilian sulphur costs from £6 to £7. The ore is usually sold at so much per cent of sulphur per ton; for some years the rate was 1s. per unit, at present it is 8d.

Theoretically, 100 parts of pure sulphur should yield 306·25 parts of oil of vitriol, and in practice it is found that 100 parts of brimstone, containing 1 per cent of impurity, can be made to yield 302 parts of acid, while with pyrites the acid produced only amounts to 285 parts of the sulphur actually burned, or 275 parts on the sulphur actually present in the ore.

The author then referred in detail to the several sources from which the required sulphur is obtained. He spoke of them in the following order, and illustrated his remarks by a very extensive collection of specimens:—

Sicilian Sulphur.—This is found chiefly in the volcanic districts on the south coast of Sicily, more or less pure, and imbedded in lime and clay marl. It is extracted from the crude mineral in various ways, according to the richness of the material. If the mineral is very rich, it is simply melted in a cast-iron pot, and then the liquid sulphur is ladled out into moulds; but if the mineral is poor in sulphur—some kinds containing only 15 to 20 per cent—it is distilled in earthen retorts, and usually condensed in water. As imported into this country it consists

of three qualities—"firsts," "seconds," and "thirds." The latter only is used in making oil of vitriol, the others are refined for making gunpowder.

Spanish Pyrites.—In the author's opinion, this mineral claims the first attention, owing to its forming at least one half of all the pyrites burnt in the sulphuric acid manufacture. It is found chiefly in the province of Huelva; the deposits pass, however, from Spain into the adjoining part of Portugal. Some of the deposits are nearly a mile long, of great depth, and of varying width. Till lately, the great bulk of pyrites imported was brought chiefly from Mr. Mason's mines, near the river Gaudiana. The owner was the first person to construct a railway as a means of transport from the mines to the coast, instead of the antiquated and expensive method of mule carriage. Mules are still employed at all the other mines, and the mineral is carried in sacks or baskets on the mules' backs. A few years since, the Tharsis Mining Company, consisting chiefly of Glasgow capitalists, was formed to purchase and work a number of mines. The pyrites from the Tharsis mines are now used largely, and the imports of Mr. Mason's ore have been much reduced. The latter usually contains about 50 per cent of sulphur, besides 3 or 4 per cent of copper. The Tharsis mineral contains 48 to 50 per cent of sulphur, and 4 to 5 per cent of copper. Both kinds are much the same in their working qualities; they burn well, and make comparatively little dust in breaking.

Norwegian Pyrites.—Many mines of pyrites occur in Norway, but those in the vicinity of Drontheim are the most important. The Norwegian ore is pretty largely imported into this country, but chiefly to the Tyne. The largest quantity is raised in the mines Ytteröen, the annual product being from 6,000 to 8,000 tons. It consists of very small crystals, is of good quality, burns well, and does not slag in the kilns. It contains about 44 per cent of sulphur, and from 1 to 2 per cent of copper. Under this head Mr. Mactear referred to a second quality of this mineral; to an excellent variety obtained about 30 miles from Drontheim, and containing but a trace of copper; to the ores obtained from some mines opened lately near Bergen; and to another variety which comes from Nordland, yielding 42 per cent of sulphur.

Swedish Pyrites.—This is obtained in mining for copper ores, and is said to exist in enormous quantities, but at present the want of means of cheap transport to convenient shipping prevents it coming into extensive use. A few cargoes, however, have found their way to Britain, and the ore itself has been found to work.

Belgian Pyrites.—Large quantities of this ore are imported, especially to the Tyne—the freight from Antwerp being very low, sometimes only 6d. per ton. The mines yielding it are in the districts of Liege and Namur, and are worked primarily for lead and zinc ores. One kind is called alluvial pyrites, and has the form of coprolites, and few of the pieces weigh more than a pound each. It burns well if used along with Norwegian or other ores. Another resembles a slag or cinder, and often contains lead or antimony. Belgian ores contain from 40 to 50 per cent of sulphur, and traces of thallium.

Westphalian Pyrites.—This resembles a very poor fire-clay, or shale, in appearance, but it burns well. It is said to contain a considerable quantity of thallium. The analysis gives 42 to 45 per cent of sulphur.

Italian Pyrites.—Very little of this is imported, for although it contains about 45 per cent of sulphur, it also contains 9 or 10 per cent of silica, and its physical properties are not in its favour.

Irish Pyrites.—This is obtained from the Wicklow mines, where it occurs in beds of great thickness. It contains from 30 to 35 per cent of sulphur. A deposit occurs in the Vale of Avoca which contains about 44 per cent of sulphur.

Cornish Pyrites.—Under this head the author included the ore from the mining districts of Dorset, Devon, and Cornwall. It is got in the dressing of the lead and copper ores, and usually contains from 25 to 30 per cent of sulphur and 1 or 2 per cent of copper, and frequently arsenic. The author also referred to the fact that Mr. Vivian is making sulphuric acid by burning copper pyrites in kilns of peculiar construction.

Coal Brasses.—This material, also called "Scotch gold," is largely used, and is a cheap source of sulphur for acid to be used in making manures, &c. Even when well cleaned it always contains organic matter, and possesses other disadvantages; still it is useful in keeping up heat in the kilns if used along with other varieties.

Cleveland Pyrites.—Although interesting to the geologist, this mineral is of little interest to chemical manufacturers, and the author said he only knew of one establishment where it is used—one near Middlesbro'-on-Tees.

The author concluded his paper by referring to the sulphur-recovery process, patented by M. Mond, and now being practically applied under his (the author's) care at St. Rollox Chemical Works.

Several members of the Society spoke on topics suggested by the paper; and a vote of thanks was passed to Mr. Mactear.

ROYAL INSTITUTION OF GREAT BRITAIN.

Weekly Evening Meeting, Friday, March 13, 1868.

"*On the Probable Exhaustion of our Coal Mines*," by W. Stanley Jevons, M.A., Professor and Cobden Lecturer on Political Economy in Owens College, Manchester.

I. The coal raised from the coal mines of the United Kingdom in the year 1866 amounted to more than *one hundred million tons* (more exactly 101,630,544 tons), according to the excellent returns published by Mr. Robert Hunt, of the Mining Record Office. Reflecting upon the full significance of this fact it may be asserted:—

1. That the coal trade of this kingdom is the greatest trade, in regard to the bulk and weight of the commodity, ever carried on.

2. That every pound of that vast quantity of coal may be regarded as a pound of the most intrinsically useful and valuable substance ever discovered.

3. That the power and usefulness of coal is felt in every branch of industry, and in almost every operation which we carry on.

4. That Britain possesses the aid of this most invaluable substance in an altogether peculiar degree: and—

5. That we cannot hope to stand very long in this most happy position.

II. So vast a quantity as 100,000,000 tons cannot be represented to the eye or mind. Its bulk is 30 times as great as that of the greatest single work of human hands, the Pyramid of Cheops. Greater quantities of commodities are brought into British ports at present, than are recorded in the history of any nation, and yet it would take more than seven times as many vessels as those which enter our ports in a year to carry the quantity of coal we use.

More than half of the whole carrying power of the railways of the United Kingdom, devoted to goods traffic, is occupied in the conveyance of coal. So far as we can judge from returns, which do not always distinguish the kind of goods carried, the goods traffic of the railways of the United Kingdom in 1865 was as follows:—

	Tons.
General Merchandise*	36,800,000
Minerals	18,300,000
Total.. ..	55,100,000
Coal and Coke	59,500,000
Total.. ..	114,500,000

III. This vast trade in coal can only be accounted for by considering the wonderful qualities with which coal is endowed. It is the mainspring of our material industry. It may be called the real Philosopher's Stone, which supplies us cheaply and plentifully with everything that can conduce to the service of man. This extreme usefulness of coal is due—

1. To the enormous power which is latent in it, and is brought forth when we burn it;

2. To the fact, now so clearly revealed by science, that *force is the key to all the changes of matter.*

By aid of the mechanical equivalent of heat, we can ascertain that good coal contains latent force sufficient to raise its own weight 11,422,000 feet, or about 2100 miles against the force of gravity. The coal raised in 1866 may further be calculated to contain force equal to that which would be exerted by 530,000,000 horses, or 2,650,000,000 men, working eight hours a day for 300 working days in the year.

IV. This vast power is turned to use in an indefinite multitude of ways, which may thus be rudely classified.

CLASSIFICATION OF THE USES OF COAL.

(I.) As Source of Heat.

1. *For Household Use.*—Warming and ventilating houses, churches, public buildings, &c.
2. *For the Alteration of Cohesive Condition of Substances.*—Melting and casting of metals; softening and forging of metals—the blacksmith's fire.
   Manufacture of glass, bricks, earthenware, &c.
   Boiling salt, soap, &c.; brewing, distilling: drying substances.
   Chemical manufactures.
3. *For the Production of Power by the Steam, Gas, or Hot-air Engine.* Pumping water; draining mines; supply of water; removal of sewage.
   Steam navigation.
   Railways, and road locomotives.
   Hammering, rolling, and working metals.
   Mill and factory labour.
   Hydraulic and pneumatic machines.
   Small machines moved by gas engine.
   Machine agriculture; steam ploughing, &c.
   Manufacture of ice.

(II.) As Reducing agent; Source of Heat, with Chemical Affinity.

Smelting of the metals—iron, copper, lead, zinc, &c.
Chemical manufactures.

(III.) As indirect Source of Electricity by Magneto-Electric Machines.

Electro-telegraphy.
Electro-metallurgy.

(IV.) As Source of Light.

Gas manufacture; petroleum; paraffin candles.
Electric light-house illumination.
Photography by artificial light.

(V.) As Source of Material.

Tar, pitch, naphtha, lubricating oils.
Ammoniacal manures: carbolic acid; aniline dyes; ethereal odours and flavours, &c.

It is only by thus collecting together the multitudinous uses of coal that we can gain an adequate idea of its importance to us and the certainty that its use will extend.

V. Comparing, now, the present yield of coal (100,000,000 tons annually) with the quantity which Mr. Hull believes to lie in these islands, within 4000 feet of the surface and in workable condition (83,544,000,000 tons), we find that we might continue to consume coal at our present annual rate for 835 years at least; but when we remember that our consumption has increased by 36 millions in the last twelve years (from about 65 millions in 1854 to 101,000,000 in 1866), and that the causes of increase still continue in existence, we cannot attribute any importance to the above calculation. There is no appearance that steam navigation or railways have at all approached their full developement in this country; while in the steam plough, in schemes of steam-drainage or water-supply, the employment of steam-produced hydraulic pressure, in the use of small gas-engines in workshops, and in a multitude of other ways, we have some indication of the increased future demand for coal.

VI. Economy, it may be pointed out, does not tend to reduce the industrial consumption of coal, but acts in the opposite direction: by increasing the profitableness of coal-labour, it extends its use. Almost every improvement in the engine for the last century and a half has been directed to economising the consumption of coal; and yet the use of the engine and the quantities of coal consumed advanced *pari passu* with its economical performance.

It is altogether irrational to argue that progressive economy, which has coexisted with and been the partial cause of advancing consumption in the past, will have the opposite effect in the future.

VII. As regards the law of increase of coal consumption, both experience and theory lead us to believe that the increase takes place in a geometrical series, by multiplication rather than by mere addition. The following numbers will illustrate the difference in question:—

Arithmetical Series, increasing by addition .. 1 2 3 4 5 6 7 8
Geometrical Series, increasing by multiplication 1 2 4 8 16 32 64 128

The following table will show that when we can get accurate statistics of the consumption of coal we find the increments indefinitely increasing, in the manner rather of a geometrical than of an arithmetical series.

Year.	Total quantity of coal imported into London. Tons.	Increase in fifty years. Tons.
1650	216,000	—
1700	428,100	212,100
1750	688,700	260,600
1800	1,099,000	410,300
1850	3,638,883	2,539,883
1863	5,119,887	5,696,170*

The above and other statistics quoted in the 'Coal Question,' † Chapters IX. X. and XI. show that our industry grows by multiplication, and by multiplication at a rising rather than at a falling rate. The temporary depressions of trade which occur at intervals may sometimes seem to check the rapidity of this increase; but we have only to wait a year or two to see our industry advancing again with growing strides.

(To be continued.)

Solubility of Silicic Acid in Ammonia.—Richard Pribram has investigated this behaviour and found that SiO_3 is taken up by ammonia in the following proportions: natural anhydrous requires 6000, artificial anhydrous 260, hydrated dried 330, gelatinous 140 parts liquor ammoniæ of 10 per cent. Exposed to the air NH_3 evaporates, and finally a clear solution of $NH_4O,4SiO_3$ remains. By boiling, about 19-20ths of the remaining ammonia is expelled, and the clear solution contains about 80 of SiO_3 to one NH_3. Dried at ordinary temperature the residue has about the same composition, but water takes up mere traces of it. The results give a hint of the manner in which silica may be administered internally, and how plants most likely take up this compound.—(*Wittst. Viertelj.*, 1867, 30—41).

* Not including live stock, of which the weight is not ascertained.

* Increase as for fifty years, if continued at same rate as during the thirteen years experienced.

† "The Coal Question: an Inquiry concerning the Progress of the Nation, and the Probable Exhaustion of our Coal Mines." By W. S. Jevons, M.A. 2nd ed. revised. London, 1866. (Macmillan.)

FOREIGN SCIENCE.

Paris, May 6, 1868.

Reduction of phosphide of iron minerals.—Prisms of constant deviation.

Considering the extent of the iron manufacture in England, the abundance of phosphoric iron ores, and the prejudicial influence exerted by phosphorus upon iron, M. Caron's research on the reduction of phosphide of iron minerals deserves space in these columns, if not, which we are far from saying, from its own intrinsic merit. To this savant we are already indebted for a former research upon the metallurgy of iron; he then showed that the addition of oxide of manganese to the blast-furnace charge eliminated a considerable part of the sulphur and silicium which passed into the slag. Since then he has observed that this oxide (oxide of manganese) while acting energetically in the expulsion of silicium and sulphur, is powerless with regard to phosphorus. Many sterile experiments were made, but one method gave, under certain circumstances, appreciable and satisfactory results. In the majority of cases, the phosphatic minerals worked for iron contain the phosphorus in the state of phosphate of iron, alumina, or lime; to counteract the pernicious influence of phosphoric acid, it has been customary to mix these minerals with lime, which alone, up to the present, has appeared able to separate the phosphorus from the iron. Unfortunately these phosphates mixed with lime are almost infusible, and a large proportion of silica is therefore obliged to be added to render the slag sufficiently fluid. M. Caron inquires, What passes under these circumstances? Three substances are found together, phosphates, silica, and carbon, exactly as in the process indicated by M. Wöhler for the preparation of phosphorus: there is on the one side a siliceous slag, on the other iron, carbon, and free phosphorus, which naturally enters into combination, and forms a phosphoric iron. This reaction, M. Caron maintains, is undoubtedly that which takes place, for analysis shows the slags of the blast furnace, operating with phosphatic minerals, to be free from phosphorus, while the resulting iron seldom contains an inoffensive amount of this element. Admitting that lime removes the phosphoric acid from the oxide of iron, evidently what is required is a flux other than silica, capable of dissolving phosphate of lime without decomposing it. Fluoride of calcium has appeared *à priori* best able to fulfil these conditions. Kryolite and other fluorides would doubtless produce the same effects. A mixture of phosphate of lime and fluoride of calcium was placed in a crucible of gas-retort carbon, covered externally with wood charcoal, and enclosed in an earthern crucible; a mixture of phosphate of lime and silica was placed in the same conditions. Thus prepared the two crucibles were heated to the temperature of the fusion of steel. The carbon crucible containing the silica and the phosphate was completely pierced; there remained the melted silicate of lime, but the phosphorus had disappeared. The crucible containing the fluoride of calcium was intact, a slight bed had been eaten in, probably due to the presence of a little silica; the button was phosphoric, and became luminous under the blow of a hammer. It was then certain that fluoride of calcium could dissolve phosphate of lime without occasioning any decomposition. Experiments were therefore made upon phosphate of iron. (1.) A mixture of suitable quantities of pure phosphate of iron, lime, and fluoride of calcium was placed in a brass crucible. (2.) A mixture of pure phosphate of iron, lime, and silica, was placed in a similar crucible. The two crucibles were heated to the temperature of the fusion of steel. The crucible containing the silica was eaten into, and the button of iron, crystallised in large plates, broke with great ease. The crucible containing the fluoride, on the contrary, was scarcely attacked; the metallic button slightly flattened under the hammer, and finally broke, yielding a granular fracture. The first button contained three times the amount of phosphorus present in the second, When operating upon phosphatic minerals containing less phosphorus than pure phosphate of iron, a sensible improvement follows the substitution of fluoride of calcium for silica; nevertheless the improvement becomes less and less important as the quantity of phosphorus in the ore decreases. Other compounds besides the phosphates are soluble in fused fluoride of calcium without decomposition; sulphates, arseniates, &c., behave quite similarly; alumina and analogous substances dissolve in this flux, and can thus be removed in the slag; otherwise the intervention of silica would be necessary.

M. Bauernfeind has published in the *Comptes Rendus* of the Academy of Sciences of Munich some interesting facts with regard to a prism which deviates the incident ray by a constant quantity, whatever be the angle of incidence. This result is obtained by two interior reflections; it is, however, necessary that the first refraction angle of the prism be double that of the second. Designate by A, B, C, the three angles of the prism, and by *a*, *b*, *c*, the three opposite faces; the ray enters at the face *a*, it is reflected internally first on *b*, then on *c*, and passes out at the face *b*. For the deviation to be constant it is necessary that $C = 2A$. A prism satisfying this condition deviates the incident ray by a quantity equal to the first refraction angle C, and the angle of emergence is always equal to the angle of incidence. The intensity may be augmented by silvering the reflecting surfaces. If the edge B be cut in such way as to make a fourth face parallel to *b*, an object in the direction of the deviated ray may be seen through the two parallel faces. When $C = 120°$ and $A = 60°$, the angle B becomes zero, and the face *a* parallel to *c*.

A rectangular prism, in which $C = 90°$, $A = B = 45°$, was employed by M. Bauernfeind some years ago in the construction of certain surveying instruments. In an apparatus with crossed prisms, the two prisms being symmetrically superposed, one was enabled to sight at the same time two points situated on the right and left of the observer, and to determine thus a point of the straight line which would join them.

CORRESPONDENCE.

PROFESSOR GUTHRIE'S VOLTASTAT AND GRAPHIC FORMULÆ.

To the Editor of the Chemical News.

Sir,—The report which appears in your No. 438 of the meeting of the Chemical Society of the 16th of April is full, and in the main so accurate, that to those of your readers who were present on the occasion it might serve as a record.

As, however, you address a larger public, I beg you to allow me as concisely as possible, to clear away some misconceptions which seem to have arisen at the time, and which, judging from your correspondent "F.C.S." in the above number have not been dissipated by reflection.

With regard to my voltastat, I have only two things to add to your report. The first concerns the use of the instrument as a voltameter. In order to calibrate the apparatus in the sense of determining the height of the mercury in the manometer tube, which corresponds to a given rate of gas delivery (or *vice versâ*), it is necessary to collect the gases evolved. But, when this is once done, the height of the mercury in the manometer shows at once the activity of the battery. And here I think the instrument is of considerable value, because by a simple reading

off we are able to arrive at a measurement of the strength of the current as accurate as that attained by the actual collection of the gas.

The second point, which is of some technical interest, is that raised by the President, who expressed his opinion that the voltastat was not applicable to weak currents such as are required for the deposition of copper. I need only remind your readers that the tenacity of the copper film is not a mere function of the power of the battery, but also of the extent of the surface upon which the deposition takes place, and that the coveted state of aggregation of the copper is attained by extending the receiving surface, as well as by diminishing the strength of the current. *Cæteris paribus*, the deposited film is more coherent when the deposition takes place more slowly. Given a current of a certain strength—let the surface to be electroplated be exceedingly small, the copper deposit will crumble away as it is formed. Let now the current remain the same, but let the surface be extended, then in the same time the same amount of copper will be deposited, but being precipitated upon a larger surface it will be thinner;—the deposition will have taken place more slowly and the copper will be more tenacious.

From this cause alone the objection raised by the President would fall to the ground. But indeed no such extension of the polar surface is necessary. If a very weak current is required we have only to insert a tube of very small capillary opening. There is in fact no limit to the degree to which the strength of the current can be reduced by modifying the instrument.

The vivacious onslaught committed by Dr. Odling on graphic formulæ in general, and on Dr. Brown's especially, and of which a judiciously subdued account appears in your report, I would willingly leave for reply to Dr. Brown. But as the "humourous remarks" were called forth by my modification of Dr. Brown's scheme, I may be allowed a few words of reply.

In support of the use of graphic formulæ I am fortunately able to cite an authority of whom I have no doubt Dr. Odling entertains a very high opinion. This authority is Dr. Odling himself.

In his "humourous remarks" Dr. Odling appeared shocked at the idea of an atom of nitrogen supporting three "sticks," one in each hand, and one on its head. Strange objection from one who years ago trained his atom of nitrogen to the much more difficult acrobatic feat of balancing simultaneously three sticks on the tip of its nose,—N'''.

Or would not our Secretary rather liken his accents to an advertisement on the part of the element? "Willing to adopt, three hydrogen babies, or an oxygen boy and a hydrogen baby, or a full-grown phosphorus adult. Apply to N., care of Dr. O., &c., &c."

In seriousness, I am far from wishing to depreciate the system of accents introduced by our humourous Secretary. But as formulæ constructed with them are to all intents graphic formulæ, I am at a loss to understand why the graphic formulæ introduced by Dr. Brown should have so terrified our Secretary that he has sought refuge from Dr. Brown's "sticks" even beneath the once detested "buckle" of Kolbe.

It is surely a misconception on Dr. Odling's part to imagine Dr. Brown's double bond to indicate a double strength of attachment; a misconception arising from a confusion between number and quantity.

I refrain from following Dr. Odling in his connubial illustrations, which combined great humour with considerable pathos.

Your correspondent "F.C.S.," in the same number, arrives at the conclusion that there is little new in my scheme, and that that little is bad. Let me endeavour to reply to some of the remarks of "F.C.S."

Though my symbols for water and bromine resemble the ordinary symbols for division and multiplication, yet, since the latter operations are at present scarcely employed in chemical formulisation, there can be but little danger of ambiguity arising from this source. The dots and accents employed by Berzelius and others, for oxygen and sulphur may, perhaps, be still employed by "F.C.S.;" but, I imagine, by few others. The resemblance between my symbol for oxygen, and the minus sign, can be of little consequence, because the minus sign need never be used.

I am fully aware that I am not the first to employ differently shaped figures to represent different elements. The hieroglyphics employed by the alchemists, and which still linger in pharmacy, are arbitrary. So are those mentioned by "F.C.S." The symbols whose use I suggest have a meaning. The novelty which I claim for my modification of Dr. Brown's scheme, is this:—The figures employed by Dr. Brown are, I think, difficult to read at a glance; the only graphic difference between the symbols for different elements being the number of linear attachments. The "atomicity" in my symbols is shown by the form of the symbol itself. I need only add that the sole reason I have for adopting these special symbols is, that they are the simplest I can find to express my meaning.

With regard to incompleteness, I fully admit the charge, and invite "F.C.S." to make the system more complete. As it stands, my plan is, of course, little more than a sketch; but, unimportant as it may be, it is for the consideration of chemists rather than of humourists.—I am, &c.,

Frederick Guthrie.

Edinburgh, April 28th, 1868.

SCIENCE TEACHING IN SCHOOLS.

To the Editor of the Chemical News.

Sir,—I cannot see that it was unwise to form a judgment on the tone of Mr. Rodwell's teaching from the published conclusions to two courses of his lectures, when each conclusion seemed to give the same indications. A teacher of science generally sums himself up in the final remarks of his course of lectures; and Mr. Rodwell appeared to have followed the usual custom.

As a matter of fact I could not understand the conclusions—I found them, in a marked degree, scholastic; I then inferred that a scholastic tone ran through the lectures, and from this, that they "were not adapted to attract boys to science." Mr. Rodwell objects to the mode in which I arrived at this conjecture, and then quoting it, adds—"they would not be fulfilling their object if they sought to do this;" hence it appears that they were not meant to attract, and it is an obvious probability that they were not adapted to attract.

However, I cheerfully concede to Mr. Rodwell that I am not in possession of sufficient material for forming an adequate idea of his lectures, and if I have misjudged him, I ask his forgiveness.

Setting particular schools and individuals aside, the imperial question remains—How is science to be taught in schools? If boys are to be elevated by means of science, it must of course be made a serious study; but, on the other hand, it should not be taught in the same hard, dry way in which classics have been taught. Phenomena should be explained simply, and I assert that the most talented and laborious teacher will not find his powers any too much for this. Theories that can be appreciated only by savants should not be introduced.

This is what I mean by the popularisation of science, and we want men to carry out this work. We do not want a temple to worship in, nor a haughty priesthood to exhibit to us a shrine; we want men who have been endowed with the means, leisure, and love for the work, to discourse to us in our own language on the works of nature, and to walk with us in the paths of real knowledge.—I am, &c.,

D.

MISCELLANEOUS.

Caution to Bakers.—On Friday, April 24th, at the Winchcombe Petty Sessions, before the Rev. R. Wedgewood and J. Waddigham, Esq., county magistrates, George Smith, baker, was summoned at the instance of J. C. Dent, Esq., for having adulterated his bread with alum. The charge was clearly established by Mr. Horsley, of Cheltenham, county analyst, who made a series of interesting experiments, the result of which was that the defendant was convicted, and fined £8 and costs, including the analyst's charges.

New Training College, for Veterinary Students.—It is proposed to start a New Training College in September next, under the presidency of Professor Tuson. This College will be founded for the purpose of supplying an admitted want, namely, an institution in which youths intended for the professions of Veterinary Medicine and Agriculture can be furnished with that preliminary general and scientific education which is so highly essential—1stly. To enable them to quickly comprehend the technical terms employed in the various branches of Medical and Agricultural Science, to follow the reasonings of lecturers and authors, and therefore to readily and thoroughly master the subjects which engage their attention while pursuing the advanced and professional courses of study taught in Veterinary and Agricultural Colleges. 2ndly. To enable them to grasp principles, to appreciate their value, and to acquire the power of applying them with facility in the practice of their professions. 3rdly. To enable them to raise the dignity and increase the importance of their callings, and to fit them for occupying that social position which is allotted to every educated man. The curriculum of study will comprise English, Arithmetic, Latin, French, Geometry, Algebra, Zoology, Botany, Physics, Chemistry, and Practical Chemistry. For the latter purpose a laboratory will be provided, in which instruction will be given in the rudiments of Analytical and other branches of Experimental Chemistry. The curriculum is so arranged that youths of average ability and industry may pass through it in one session. The session will be divided into three terms.

Royal Institution of Great Britain.—At the annual meeting, held on Friday, May 1, 1868, W. Poole Esq., in the chair, the annual report of the committee of visitors for the year 1867 was read and adopted. The books and pamphlets presented in 1867 amounted to 131 volumes, making with those purchased by the managers, a total of 319 volumes added to the library in the year, exclusive of periodicals. Forty new members were elected in 1867. Sixty-three lectures and twenty evening discourses were delivered during the year 1867. Thanks were voted to the President, Treasurer, and Secretary, to the Committees of Managers, and Visitors, and to the Professors, for their services to the Institution during the past year. The following gentlemen were unanimously elected as officers for the ensuing year:—*President*—Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. *Treasurer*—William Spottiswoode, Esq. M.A. F.R.S. *Secretary*—Henry Bence Jones, M.A. M.D. F.R.S. *Managers*—Henry Wollaston Blake, Esq. M.A. F.R.S.; Charles Brooke, Esq. M.A. F.R.S.; Adm. Sir Henry John Codrington, K.C.B.; Captain Douglas Galton, C.B. F.R.S.; John Peter Gassiot, Esq. F.R.S.; William Robert Grove, Esq. M.A. Q.C. F.R.S; Cæsar H. Hawkins, Esq. F.R.S; Sir John Lubbock, Bart. F.R.S. Pres. Etom. Soc.; Sir Roderick I. Murchison, Bart. K.C.B. D.C.L. F.R.S.; William Pole, Esq. M.A. F.R.S.; William Frederick Pollock, Esq. M.A.; Robert P. Roupell, Esq. M.A. Q.C.; Lieut.-Gen. Edward Sabine, R.A. D.C.L. Pres. R.S.; Sir Charles Wheatstone, D.C.L. F.R.S.; Colonel Phillip James Yorke, F.R.S. *Visitors*—Andrew Whyte Barclay, M.D.; Charles Beevor, Esq. F.R.C.S.; John Ashton Bostock, Esq.; John Charles Burgoyne, Esq.; Rev. Charles Fynes Clinton, M.A. F.R.G.S.; Alfred Davis, Esq.; William Dell, Esq.; Rev. G. Codwin Pownall Glossop, A.M.; Alfred Gutteres Henriques, Esq.; Edward Henry Moscrop, Esq.; William Newmarch, Esq. F.R.S.; Arthur Giles Puller, Esq. M.A. F.S.A.; Samuel Scott, Esq.; Edward Owen Tudor, Esq. F.S.A.; Robert Ballard Woodd, Esq. F.S.A. F.R.B.S.

The Royal Society.—At the meeting of this Society last evening, the names of the 15 candidates whom the Council recommended for election into the Society were read from the chair. The only chemists in the list are—A. V. Harcourt, Esq., and P. Griess, Esq.

NOTES AND QUERIES.

Trinidad Pitch.—A corresnondent wishes to know where he can purchase Trinidad pitch.—L. Y. E.

Bleaching Woollen Rags.—George Watson, of Manchester, would be glad if any of your correspondents could inform him what is best for bleaching woollen rags previous to being torn up.

Colour of the Sea.—Cerebric Acid.—1. Where can I find the best account of Father Scechi's observations on the cause of the colour of the sea? 2. Is there any authority (and if so, whose) for the statement made in "Marshall's Physiology," vol. ii, p. 114, that cerebric acid is found in Indian corn, and to less extent in wheat? And if cerebric acid be really present does it of necessity arise from protagon?—F.R.S.

MEETINGS FOR THE WEEK.

TUESDAY.—Royal Institution 3. Dr. M. Foster "On the Development of Animals."
WEDNESDAY.—Society of Arts, 8.
THURSDAY.—Royal Institution 3. Professor Bain, "On Popular Errors."
FRIDAY.—Royal Institution 8. E. Deutsch, Esq., "On the Talmud."
SATURDAY.—Royal Institution, 3. Professor Bain, "On Popular Errors."

TO CORRESPONDENTS.

Science Teaching.—There is no better firm than the one you mention.

Enquirer.—The information is given in the present number.

Thos. Spencer.—Mr. Muter's letter on the permanganate water-test renders your communication unnecessary.

G. Benson.—Dissolve commercial aniline in dilute hydrochloric acid, evaporated nearly to dryness, and recrystallise two or three times from water.

G. Spencer.—We regret that it is out of our power to give the information required.

S. A. Sadler will receive a private communication in a few days.

Tyro.—Where the word salt is used by itself, common salt or chloride of sodium is intended to be expressed.

A. Smith.—A full account of the results obtained by the Editor, in his investigations on thallium, down to January, 1864, together with a list of memoirs, is published in the "Journal of the Chemical Society," vol. xvii., p. 112.

E. Corbett, Junr.—We cannot say for certainty that they are patented, but it is extremely unlikely that such valuable processes should not be patented in this country.

Johnson's Patent Stoppers.—A correspondent wishes to know where these can be obtained.

W. O. Gervaus, Ohio, U.S.A.—Robbins's oxygennesis is a mixture of binoxide of barium and bichromate of potash, both dry and in fine powder. On adding hot water or dilute acid to the mixture, oxygen is evolved.

C. L.—Mix modelling clay with glycerine instead of water. This will cause it to remain plastic.

Communications have been received from E. Corbett, junr.; H. Thompson; W. O. Gervaus, Delaware, Ohio, U.S.A.; W. Hackney; Professor Maynard, Troy, N.Y., U.S.A.; M. A. Whichelo; J. Spiller; W. F. Barrett; O. Scevola; W. White; Dr. R. Angus Smith, F.R.S.; H. Morton; W. Arndt; J. Heywood; J. Marples; Longmans and Co.; Churchill and Sons; J. A. Pooley; M. G. Maurice; W. A. Townsend and Adams.

Books Received.—"The Inductorium or Induction Coil," by Dr. Noad, F.R.S. London: John Churchill and Sons; "Tea," by E. F. Bamber. London: Longmans and Co.; "Sanitary Siftings on Results of Sewage Systems Compared," by a Naval Officer. London: E. and F. N. Spon; "On Liquid Fuel," by Benjamin H. Paul, Ph.D. London: E. and F. N. Spon; "The Colonial Monthly, an Australian Magazine," December, 1867, and February, 1868; "On Dr. Muspratt's Discovery of a new Spa at Harrowgate;" "The Chemical Manufactories' Directories of England, Scotland, and Ireland." London: W. Kent and Co.; "A Dictionary of Chemistry," by Henry Watts, F.R.S. Part xlv., completion. London: Longmans and Co.; "The Journal of Materia Medica:" "A Key containing Answers to the Exercises in Galloway's First Step in Chemistry." London: John Churchill and Sons; "Annual Register of the Rensselaer Polytechnic Institute, 1867-68."

THE CHEMICAL NEWS.

Vol. XVII. No. 441.

ON MONOCHROMATIC LIGHT.

By HENRY MORTON, Ph.D.,
Prof. Chem. and Phys., University, Penna.

The difficulty of producing monochromatic light in great quantity prevents us from demonstrating satisfactorily many points of interest in connection with the composition of light, and the theory of vision. Coloured glasses placed in the path of powerful beams of white light are doubly unsatisfactory ; they reduce the amount of light enormously, and, with few exceptions (*e.g.*, red glass coloured with gold), yield a beam of mixed colour.

The old experiments of snap-dragon, or the spirit lamp with salted wick, are admirable as far as they go, but yield a very faint light at best. Something far superior to this is furnished by the arrangement of a ring of cotton wick wound on a wire, and supported immediately over and around a large Bunsen burner, the wick being soaked with an aqueous solution of some flame-colouring salt. This plan in effect is suggested in Sir David Brewster's natural magic; but as the Bunsen burner was not then known, a less simple arrangement is described to perform the same office.

The drawbacks to this arrangement are : the trouble of adjusting the cotton wicks, the delay in changing to produce a new colour, and the brief duration and, to a certain extent, irregular amount of the effect.

After lighting, the burners increase their effect to a certain point, and then soon rapidly diminish in the intensity of their light.

When a large number of burners are to be used these difficulties become serious, and I have therefore devised the following arrangement, which has proved, on trial, thoroughly efficient :—The burners to be used, varying in number from 5 to 30, in different experiments, are enclosed below in a box with a single large entrance, opposite to which is placed an atomiser, operated either by steam or compressed air.

A spray of the colouring solution is thus mixed with the air supplying the burners, and their flames are thus tinged with the greatest ease, certainty, and intensity of effect, the whole action being entirely under control, and capable of being maintained indefinitely, while the change from one colour to another is effected by simply transferring the tube of the atomiser from one solution to another.

For experiments demanding diffused light, this apparatus is most satisfactory. Five large Bunsen burners, thus arranged, light up the lecture room of the Franklin Institute, which seats about 350 persons ; and with 60 burners, arranged in two groups of 30, I expect to illuminate sufficiently, at my next lecture, the Academy of Music, which has 3,500 stalls. Of course in neither case is the light brilliant, but simply sufficient to show in the most distant portions of the room the peculiar effects of this light on the faces and dresses of the audience themselves. Any object near the flame is, however, brightly illuminated.

The stage being set with the most brilliant scenery and coloured decorations, suddenly illuminated by yellow light thus developed, the ordinary gas-light being turned down, presents an appearance of ghastly change, which cannot be appreciated until seen, while sufficient light reaches all parts of the house to allow the audience to repeat the strange observation of metamorphosis upon their neighbours.

In certain experiments, however, it is necessary to have monochromatic light of great intensity and concentration.

This I have found could be furnished in the case of yellow light, to a certain extent, by substituting in the gas microscope or polariscope a soda-glass rod for the ordinary lime cylinder, and so adjusting its position that the rays from the heated glass should be cut off from the lenses, and only the light of the yellow flame should reach them.

We can thus show, upon a screen 5 ft. in diameter, the greatly-increased number of rings developed by a section of iceland spar in monochromatic light.

With the spectroscope we can also project the sodium line on the screen, so as to be well seen by an audience of 500 persons. By afterwards producing the absorption-band due to the vapour of the substance, with the aid of a small Bunsen burner and iron spoon with sodium, and again showing the absorption-bands of nitric peroxide and of cochineal, the characteristic phenomena of spectrum analysis may be sufficiently illustrated, without the trouble and expense of the electric light.

We have, in fact, with a large and efficient battery at command, generally employed the above arrangement by preference.

The details of adjustment we will furnish, with pleasure, should any one think it of sufficient interest to ask for them.

ON THE PREPARATION OF MAGNESIA FOR RESISTING HIGH TEMPERATURES.

By M. H. CARON.

About two years ago I briefly pointed out the advantages which would accrue to metallurgy from the employment of magnesia as a refractory material. I regretted, at the same time, the high price of this earth, the employment of which appeared likely to be confined to the laboratory. Now, circumstances have happily changed ; recent modifications introduced into the manufacture of cast steel, and especially the employment of Siemens's furnace and Martin's process, absolutely demand more refractory bricks than those at present in use, irrespective of price. On the other hand, native carbonate of magnesia, which formerly cost 250 fr. the 1000 kilos., may now be obtained at the price of 70 fr. delivered at Marseilles, or 100 fr. delivered at Dunkirk. Calcination at the place where the carbonate is obtained, may still further reduce its price.* I desire now to describe my processes for its agglomeration, which may be employed by the chemist for the ready preparation of refractory vessels of all forms ; by the physicist to obtain pencils for oxyhydrogen lighting purposes ; and also by the manufacturer, to replace, in some cases, fire-bricks which have become insufficient in carrying out different processes of heating.

The magnesia which I employ comes from the island of Elba, where it is found in considerable quantities as a native carbonate, white, very compact, and of great hardness. This carbonate contains traces of lime, silica, and iron ; it is, besides, interspersed sometimes with serpentine and large plates of silica, which would diminish the infusibility of the subtance, and render it especially unfit for oxyhydrogen illumination if their removal is neglected (I will presently give the reason). These plates are, however, easily recognised, and may be readily separated, even in working on the large scale. In the case of refractory bricks, the presence of a small

* This preliminary calcination requires less heat than burning lime, and diminishes the weight of the carbonate one half.

quantity of these foreign bodies would, at the most, give rise, under the influence of the highest temperatures, to a slight vitrification, offering no serious inconvenience.

Before powdering the carbonate, it is advisable to bake it at the temperature necessary for the expulsion of the carbonic acid; the material then becomes very friable, add may be pulverised more easily. It is then possible to separate the serpentine and silica, which do not become friable under the influence of heat. This preliminary treatment does not permit of the agglomeration of the magnesia, and even were this difficulty to be overcome, a temperature higher than that of the original calcination would cause an enormous contraction, producing fissures and alterations in shape, which would interfere with the use of this substance. It is, therefore, indispensable, before moulding the magnesia, to submit it to a very intense heat, at least equal to that which it is intended to support subsequently.

Thus calcined it is not plastic, its appearance is sandy, and compression does not cause it to acquire any cohesion; a mixture of magnesia, less calcined, imparts to it this quality. The quantity of the latter to be added necessarily varies with the degree of calcination of the two magnesias; it is scarcely one-sixth of the weight of that which has been exposed to the temperature of melting steel. It only now remains to moisten it with 10 or 15 per cent of its weight of common water, and strongly compress it in iron moulds, as adopted in making artificial fuel. The brick produced in this operation hardens on drying in the air, and becomes still more resisting when it is subsequently calcined at a red heat. The same process would appear practicable, varying the form of the moulds, for obtaining crucibles of great capacity; but compression is difficult in large masses, as well as when the moulds have a large surface, as the magnesia adheres strongly to the sides. Although I have been able to obtain small crucibles for the laboratory, I do not consider this process adapted to make the large crucibles employed in steel melting. In this and other cases it is preferable to agglomerate the magnesia in the humid way.

To endow magnesia with a sort of plasticity, I have profited by a property of this earth pointed out in "Berzelius's Chemistry." When strongly calcined, and then moistened, it hardens in drying. This fact is doubtless due to a hydration which takes place without sensible increase of temperature. I have also remarked that when solidified in this manner the magnesia only loses the assimilated water at a high temperature. Then calcination not only does not disintegrate it, but on the contrary confers upon it a hardness and resistance comparable to those of ordinary crucibles after their baking. This being understood, the application of this fact is obvious. Thus, the magnesia to be employed in the manufacture of crucibles, should only be moistened, moulded into shape, dried, and then ignited. For the construction of steel melting furnaces, a paste of moistened magnesia should be plastered over the walls; it will become ignited in due course without any particular precautions being taken. It sometimes happens, however, either owing to the magnesia being too much or too little hydrated, or owing to its containing siliceous matter, that the vessels before or after firing do not possess quite the desirable solidity; they should then, to acquire this, be simply moistened in a cold saturated solution of boracic acid, dried, and then fired as before. This operation does not render the magnesia more fusible: it only causes the grains of the substance to cohere more strongly together.

Magnesia, very pure, strongly calcined and finely pulverised, may be employed in the form of paste (*barbotine*) and yield the most delicate and translucent crucibles, as well as the sharpest and most complicated impressions. I am convinced that some time hence this earth will be advantageously employed in the ceramic art in spite of its difficulties of its moulding compared with those of porcelain paste.—*Comptes Rendus*, lxvi. 839.

ON A NEW PROCESS IN MINERAL ANALYSIS.

By FRANK WIGGLESWORTH CLARKE, S.B.

In the course of some experiments upon the alkaline fluorides, I found that the most refractory minerals, such as emery, chromite, tinstone, rutile, &c., were completely and easily resolved by fusion with a mixture of fluoride of sodium and bisulphate of potassa.

The operation is performed as follows:—One part of the finely pulverised mineral is mixed in a platinum crucible with three parts of fluoride of sodium, and upon the top of this mixture are placed twelve parts of bisulphate of potash, which may be either in powder or in small lumps.

Upon heating, the mixture boils up strongly, and after a while settles into a clear, tranquil fusion. The boiling is chiefly owing to the action of the reagents upon the mineral, and not, as might be supposed, merely to the influence of the bisulphate upon the fluoride. This is shown by the fact that, whenever the reagents are heated together without minerals, although some boiling takes place, the addition of a little powdered chromite or iron ore fully doubles the violence of the action.

In quantitative analyses it is necessary to keep the crucible closely covered in order to avoid loss from spattering; and to heat carefully, so that the mass may not boil over. The bisulphate should never be mixed with the fluoride and mineral, because a portion of the assay is then apt to escape action, being left on the sides of the crucible by the boiling of the mass; but should be placed upon the top of the mixture as above directed, as then the decomposition is complete. The mass obtained by this fusion is, in the case of some minerals, completely soluble in water. In other cases, basic salts are formed, which, although insoluble in water, dissolve readily in hydrochloric acid. Almost all of the latter class may be rendered soluble in water by the following process:—The fused mass, after cooling, without removal from the crucible, is treated with a small quantity of strong sulphuric acid, and again fused. The mass thus obtained is entirely soluble in water. There are exceptions to this rule, however.

I will now describe the results I have obtained with various minerals, and for the sake of brevity, will speak of the fusion with bisulphate and fluoride as fusion No. 1, and the subsequent treatment with sulphuric acid, as fusion No. 2.

Tinstone is completely resolved, giving a white mass almost entirely soluble in *cold* water. A very small quantity of stannic acid remains undissolved, however, but by first treating the mass with hydrochloric acid and then adding water, complete solution is ensured. By rendering the solution *nearly* neutral, taking care, however, to leave a slight excess of sulphuric acid, then reducing the iron by sulphuretted hydrogen or hyposulphite of soda, and then boiling for some time, all the tin is precipitated in the form of stannic acid. If the ore contains tungstic, niobic, or tantalic acids, these also will be completely precipitated. A second fusion is of no advantage with tinstone. This is the only mineral which I have yet tested which was not completely resolved by this process, over an ordinary Bunsen's gas burner. This substance required the heat of a blast lamp.

Wolfram is entirely decomposed, affording a pale yellowish mass, partly soluble in water. Hydrochloric acid dissolves a part of the residue, but a white powder, probably the hydrate of tungstic acid, remains unattacked. Fusion No. 2 possesses no advantages with wolfram. I should not recommend the use of this process for the analysis of wolfram, as it is so readily decomposed by nitric acid. I made the experiment, however, because as wolfram is so often mixed with tinstone, it seemed to me necessary to know what the reaction would be.

Rutile.—This mineral is rapidly and easily resolved, giving a mass when cooled, of a yellowish white colour, and, as I obtained it, presenting a beautiful mottled appearance, being crystalline in structure, and in some parts nearly transparent, but opaque in others. This mass dissolved entirely in *cold* water, rendering a second fusion unnecessary. From this solution all the titanic acid may be thrown down by boiling.

Niobite is decomposed with the greatest ease. The mass is pale yellow, and partially soluble in cold water, but a small quantity of niobic acid remains undissolved, even in hydrochloric acid. But it is always best to treat with hydrochloric acid, in order to dissolve any basic sulphates of iron that may be formed. Upon boiling the filtered solution, niobic acid is precipitated.

Fusion No. 2.—A larger proportion of the mass is soluble in water than with the first fusion, as no basic salts of iron are present.

Ilmenite is completely decomposed, giving a mass closely resembling that obtained from rutile. *Cold water* dissolves a large part of this, but leaves some basic salts which dissolve readily in hydrochloric acid.

Fusion No. 2.—The resulting mass dissolves completely in *cold* water, and by boiling the solution, the titanic acid can be precipitated.

Chromic iron ore is decomposed very easily. In one case in which I timed the operation, *the fusion was complete in less than three minutes from the time I began to heat*, and that over an ordinary Bunsen's gas burner. The cooled mass is light green, partly soluble in water alone, and entirely soluble in hydrochloric acid.

Fusion No. 2.—The mass possesses a deeper green colour than that obtained by the first fusion, and a larger proportion of it dissolves in water. In every fusion that I have yet made of chromite, however, a small quantity of basic salts was formed, requiring treatment with hydrochloric acid.

Emery is rapidly and easily resolved. The mass contains basic compounds that require hydrochloric acid for complete solution; although water dissolves a large proportion of it.

Fusion No. 2.—The mass thus obtained is entirely soluble in water.

Hæmatite.—(An exceedingly hard specular ore from the Tilden mine, Lake Superior). It was completely resolved, giving a mass partially soluble in water, but dissolving entirely in hydrochloric acid.

Fusion No. 2.—The mass dissolves completely in water.

Limonite and *magnetite* behave exactly like hæmatite.

Zircon is entirely decomposed. All its silica is converted into the gaseous fluoride of silicon, and driven off. The mass obtained resembled that given by rutile. It dissolved almost entirely in cold water, but the solution speedily became turbid and deposited a white precipitate, which was either zirconia or some basic salt of that oxide. By first digesting the mass with a little strong hydrochloric acid and afterward adding water, the whole went into solution.

Fusion No. 2, afforded a mass of a beautiful waxy lustre, which was completely soluble in water.

Kyanite is entirely resolved, and, like zircon, freed from silica. The white mass contained basic compounds, and consequently required hydrochloric acid for complete solution. A second fusion gave a mass entirely soluble in water.

Orthite is completely decomposed, and deprived of silica. The mass was white, and dissolved partly in water. The insoluble residue contained basic salts and some sulphate of lime, and hydrochloric acid dissolved all but the latter.

Fusion No. 2—With the exception of a little sulphate of lime, the mass dissolved in water.

Quartz sand.—I subjected some of this substance to fusion with the mixture of bisulphate of potash and fluoride of sodium, in order to ascertain to what extent silica would be converted into fluoride of silicon. The fusion took place very easily, giving a white mass which dissolved almost entirely in water. Only a very small trace of insoluble residue remained, probably not more than one-tenth of one per cent of the quantity of sand taken. It is very probable that more careful treatment would get rid of even that small amount. After this it seems impossible to doubt that any and all silicates would be decomposed and freed from silica by this process. A convenient method is thus afforded for the estimation of bases in silicates, bearing in mind, however, that a second fusion with sulphuric acid will in most cases be necessary.

As far as concerns the complete resolution of any mineral, pure, finely pulverised cryolite may be substituted for fluoride of sodium. It labours under the disadvantages, however, of introducing alumina into the mass obtained, but nevertheless it can be advantageously employed in cases where the introduction of alumina is a matter of indifference, as in the technical analyses of tinstone and chromite, and the estimation of iron in ores, slags, and cinders. The perfectly white translucent specimens of cryolite should be chosen for this purpose.

Either the bisulphate of potash or of soda may be used, and although neither seems to possess any advantage in point of thoroughness, the potassa salt appears to be the most readily fusible, and is therefore to be preferred.

The following are the advantages that I claim for this process.

First. Speed. Among the different minerals upon which I have tested the action of this mixture, I have found no case where I was obliged to heat longer than five minutes, and many fusions are complete in three, and even less.

When bisulphate of potash *alone* is used for a similar purpose, it is usually necessary to heat for from half to three-quarters of an hour; and even then in the cases of emery, chromite, and some other minerals, it is almost impossible to obtain absolutely complete resolution.

By my process, even when the second fusion with sulphuric acid is necessary, not more than twenty minutes should be consumed in both fusions and the time for cooling between them.

Second. In every case except tinstone that has come under my observation, in my process, an ordinary Bunsen's burner may be used as the source of heat; whereas, by most other methods, a blast lamp is necessary.

Third. When bisulphate of potash *alone* is used for the decomposition of chromite, &c., it is necessary that the mineral should be reduced to extremely fine powder; but, when the mixture of bisulphate and fluoride is employed, although the mineral should be in fine powder, such an extreme state of subdivision is by no means required, and thus much labour is saved.

Fourth. In all cases when this mixture is used, the resolution of the mineral is absolutely perfect. Furthermore, all the silica is got rid of at once, and all the bases present are converted into sulphates. This last is a great advantage whenever iron is to be determined volumetrically by means of hypermanganate of potassa solution. As far as thoroughness of action is concerned, or generality of application, I can claim no advantage for my process over the use of acid fluoride of potassium; but the last-named reagent is difficult to prepare, and when prepared, cannot be preserved in glass vessels.

Fluoride of sodium is subject to neither of these disadvantages, and in the mixture with bisulphate of potassa, has slight advantages as regards rapidity of action.

It may not be out of place for me to mention here a fact connected with the use of fluoride of ammonium for decomposing silicates, as described by Rose. After fusing the mineral with the fluoride, he directs treatment with sulphuric acid, for the purpose of converting the bases into sulphates. I find that if, in the first place, sulphate of ammonia is mixed in excess with the fluoride, all the bases are directly converted into sulphates, thereby obviating the necessity of treatment with sulphuric acid.

This method *can* be used for the purpose of determining alkalies in silicates, but is far inferior to J. Lawrence Smith's process.

Technical Determination of Chromium in Chromite.—After fusion with cryolite and bisulphate of potash, as previously directed, the mass is to be treated with a little strong hydrochloric acid, and allowed to digest for about ten minutes. Then, upon boiling with water, the whole dissolves. The solution should then be neutralised, acetate of soda added, and the chromium oxidised to chromic acid by a current of chlorine gas, or by boiling with hypochlorite of soda solution. The chromium may then be separated from other substances, as directed in Professor Gibbs's paper, (*Am. Journ. Sci.*, January, 1865). When chromite is fused with bisulphate of potash and cryolite, and saltpetre is added to the mass, as soon as clear fusion is obtained, the chromium is nearly all oxidised to chromic acid. If the mass be boiled with a solution of carbonate of soda, and the liquid filtered, a filtrate is obtained which contains nearly all, but not quite all the chromium as alkaline chromates, free from iron or alumina; but, invariably, the residue upon the filter contains traces of chromium. When chromite is fused with the acid fluoride of potassium, a part of the chromium is usually oxidised to chromic acid by the oxygen of the air; and in one case that came under my observation, when I came to heat the resulting mass with sulphuric acid, red fumes were given off, which were probably the so-called terfluoride of chromium.

Technical Estimation of Iron in Ores, Slags, and Cinders.—After fusion with cryolite and bisulphate of potash, the mode of treatment varies according to the method it is desired to use for determination of the iron. If Penny's process of estimating iron volumetrically with bichromate of potassa, or the ordinary method of precipitation with ammonia is to be employed, the mass may be treated with hydrochloric acid, and thus brought immediately into solution. If, on the contrary, the iron is to be determined volumetrically by means of hypermanganate of potassa, the use of hydrochloric acid is interdicted, and it becomes necessary to fuse again with sulphuric acid and dissolve in water. When volumetric processes are used, the whole determination of the iron, from the time the assay is placed in the crucible, should take less than an hour to perform.

This process has a great advantage over all others, in the examination of ores, slags, and cinders containing iron, both as regards speed and convenience. A perfectly clear solution is immediately obtained without filtering, all the silica is got rid of, and it is only necessary to reduce the iron with hydrogen, and then to titrate. If in an iron ore it is desired to determine also titanic acid and manganese, it is best to make the subsequent fusion with sulphuric acid. The clear solution obtained is diluted to a known quantity, and by means of a graduated pipette divided into several portions. In one part the iron may be reduced and determined volumetrically; in another, the titanic acid thrown down by boiling. In still another portion, the iron, alumina, and titanic acid may be thrown down by boiling with acetate of soda, and in the filtrate, the manganese may be precipitated by a current of chlorine gas, or by boiling with hypochlorite of soda. If fluoride of sodium be used instead of cryolite in the fusion, lime and magnesia also may be estimated. For determining silica, phosphorus, and sulphur, other methods must be employed.

Preparation of Fluoride of Sodium.—When pure finely pulverised cryolite is boiled with caustic soda solution, it is, as is well known, decomposed. Fluoride of sodium is deposited as a jelly-like mass at the bottom of the vessel, and the supernatant liquid contains aluminate of soda, a little fluoride of sodium, and the excess of soda. The deposit of fluoride is to be washed with cold water until the washings have no longer an alkaline reaction; and is then purified by repeated solution and evaporation. It is so difficultly soluble, and crystallises so badly, that this last is a matter of some difficulty; but the first part of the process is so very easy that the crude fluoride of sodium can be prepared by the hundred-weight without much trouble. Iron vessels are suitable for the operation, but must be very clean and free from rust. If caustic potassa be substituted for soda, the deposit of fluoride of sodium is smaller, and the supernatant solution contains aluminate of potassa, fluoride of potassium, and a little fluoride of sodium.

Possibly the fluoride of potassium might be prepared in a state of purity from this solution, but it is extremely problematical whether this could be done economically. When pure cryolite is fused with carbonate of soda, and the fused mass powdered and treated with water, fluoride of sodium is dissolved out. This method, however, cannot compare with the first for convenience and economy.

It may not be altogether out of place to remark in this connection, that I find that when fluoride of sodium is heated with sulphate of ammonia, fluoride of ammonium is formed and sublimes. Possibly this may be turned to advantage, although I have made no experiments upon obtaining fluoride of ammonium in quantity by this process.

Before closing this paper, I also wish to state that I made numerous experiments with a view toward applying the mixture of fluoride of sodium and bisulphate of potash to the estimation of both oxides of iron when they occur in minerals; but I met with no success.

During the course of the experiments described in this paper, I hade been largely indebted to Dr. Wolcott Gibbs for many very valuable suggestions and much excellent advice which aided me exceedingly.—*American Journal of Science*, xlv., 173, March, 1868.,

CHEMICAL RESEARCHES ON SUGAR REFINING.

By M. EMILE MONNIER.

If sulphurous acid gas is conducted into a chamber containing coarse sugar, the latter is promptly bleached, and about three-fourths of the colouring matter are entirely destroyed, whilst the sugar undergoes no change whatever in composition. After this treatment the sugar smells strongly of sulphurous acid, which presents no inconvenience in the process of refining. To bleach sugar in this manner, for 1,000 parts by weight of sugar about four parts of sulphur must be burnt, and the gas conducted into the chamber. When the operation is once set going, the proportion of sulphur may be notably diminished. The sulphur is converted into gas by combustion in a little furnace placed at the side of the chamber. When the action is complete, the sugar is dissolved in water, and its sulphurous acid neutralised by a small quantity of lime. This lime may be previously converted into sucrate of lime by M. Peligot's method, that is, by crushing it with a little syrup; for 1,000 lbs. of sugar three or four lbs. of lime are requisite in the form of sucrate.

M. Monnier has been at great trouble to ascertain whether the sulphurous acid gas thus used modified the sugar so as to produce a certain amount of grape or non-crystallisable sugar, and he has convinced himself that sugar bleached in this manner undergoes no change whatever. The quantity of non-crystallisable sugar found by analysis after the operation in question was, in each case, exactly equal to the amount which the sugar contained before being bleached; namely, on the average, about 2·15 per cent. In all these experiments the sugar was exposed about forty-eight hours to the bleaching action.

The above process gives most striking results with exotic sugars, which are highly coloured; with lighter coloured samples, the bleaching is not so marked; but in the former case, two-thirds to three-fourths of the heterogeneous colouring matters are eliminated completely.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, May 7.

Dr. Warren de la Rue, F.R.S., &c., President, in the Chair.

This meeting, as is usual on the special occasions set apart for lectures, was exceedingly well attended, and several visitors honoured the Society with their presence. Mr. E. Dowson was formally admitted as a Fellow, and Mr. Thomas Bournes, Teacher of Chemistry, 47, Rigby Street, St. Helen's, Lancashire, was duly elected. The names of candidates read for the first time were Mr. William Bowyer Miller, Assayer, Royal Mint, Sydney; and Mr. William Hustler, Mining Engineer, Rosemerryn, Falmouth.

Mr. C. W. Siemens, F.R.S., delivered a lecture "*On the Regenerative Gas Furnace as applied to the Production of Cast Steel.*" The lecturer announced himself to have been a pupil of Wöhler, and said he was prepared to estimate highly the value of chemical science in directing the studies of the engineer and electrician. The regenerative gas furnace, which has during the last few years been applied to the manufacture of iron and glass, and is destined to play an important part in other chemical operations, is the joint invention of Mr. Frederick Siemens and the lecturer. It was described in 1862 by Professor Faraday, and formed the subject of his last public lecture in the Royal Institution. Before proceeding to enter upon the discussion of the furnace itself, and its peculiar details of construction, Mr. Siemens briefly sketched the properties and modes of preparation of cast steel. This product was defined as being "a compound of iron and carbon, possessing the remarkable quality of becoming exceedingly hard when heated and suddenly cooled." The proportion of carbon determines its temper, and the following table, which was shown in the form of a diagram, indicates the average composition of steel of different qualities:—

Description.	Carbon per cent.	Authority.
Wootz	1·34	T. H. Henry.
Steel for flat files	1·2	Willis.
,, for turning tools	1·0	,,
,, (Huntsman's) for cutters	1·0	,,
,, for cutters	0·9	,,
,, for chisels	0·75	,,
Die steel (welding)	0·74	,,
Double shear steel	0·7	,,
Welding steel	0·68	,,
Quarry drills	0·64	,,
Mason's tools and ramrods	0·6	,,
Common steel for stamping	0·42	,,
Spades and hammers	0·3 to 0·32	,,
Bessemer steel for rails	0·25 to 0·30	Various.
Homogeneous metal (armour plates)	0·23	Percy.
Very mild steel from open furnace	0·18	Willis.
Sample, before adding spiegeleisen	0·05	,,
Bessemer iron (pure)	trace	Abel.

Steel containing 1·4 per cent of carbon partakes of the character of white cast iron, and below 0·3 per cent the metal is incapable of being hardened. M. Frémy's opinion, that nitrogen (or cyanogen) is a necessary component of steel, was alluded to, and the probability of other elements being essential to its constitution was guardedly asserted. The presence of sulphur and phosphorus are undoubtedly prejudicial when they occur in considerable quantities, but the lecturer considers that traces of these elements may sometimes prove advantageous by increasing the fluidity and toughness of cast steel. The use of manganese fluxes, according to Heath's patent (1839), rendered it possible to prepare good steel from ordinary qualities of English puddled iron; and Mushet's discovery of the important advantages to be gained by the employment of ferro-manganese, or spiegeleisen, had paved the way to Mr. Bessemer's later triumphs. In the opinion of the lecturer, manganese has a special action in improving the quality of steel, apart from its function in removing sulphur and other impurities. Silicon, when present to the extent of 0·5 per cent, rendered it impossible to forge the ingots, but small quantities seemed, on the other hand, to be advantageous by preventing the evolution of gas in the interval between casting and solidification. The evidence respecting the effects of titanium, tin, and arsenic, was not conclusive; but Dr. Werner Siemens showed, in 1853, that tungsten had a remarkable action upon steel, in increasing its power of retaining magnetism when hardened. This property of tungsten was illustrated in the lecture-room by a small permanent magnet of horse-shoe form, which supported twenty times its own weight from the armature; the famous Haarlem magnet being incapable of lifting a weight more than thirteen times heavier than itself. The steel of which Mr. Siemens's magnet was made, contained about 2 per cent of tungsten, and 0·4 of carbon.

Reference was then made to the several processes of preparing steel, which were considered under the following heads, viz.:—The direct process in the Catalan forge; the method of cementation; the decarburisation process, or that which furnishes "puddled steel;" the Bessemer process; and, lastly, other methods of producing cast steel by fusion of malleable iron, or iron-producing substances, with cast iron, spiegeleisen, or other compounds containing much carbon, such as those practised by Uchatius, Price and Nicholson, G. Brown, Attwood, and others.

Experiments on the direct production of steel by a fan blast on an open hearth, according to M. Sudre's method, were made under the supervision of M. St. Claire-Deville and other two members of the French Institute; but the rapid destruction of the furnace, combined with the great cost for fuel, rendered the process a doubtful success. The applicability of the regenerative gas furnace to the fusion of steel and other metallurgical products, was partly established by the trials of Mr. Charles Attwood, in 1862, and of M. Lechatelier a year later, at Montluçon in France; the gentleman last named melted together hot puddled blooms and cast iron upon a bed of the aluminous mineral known as bauxite, and subsequently upon a bottom formed of common white sand. Later, in 1864, MM. Emile and Pierre Martin melted steel, both in pots and on the open hearth, by the aid of the gaseous fuel of the regenerative furnace. Their products, consisting of cast steel of various qualities, were shown at the French International Exhibition, and rewarded by a gold medal. With the view of making for himself a series of practical trials on the production of steel, Mr. Siemens constructed two experimental furnaces, in Birmingham, and succeeded in preparing steel of good quality, not only in closed pots, but on a continuous system direct from the ore. The details of these furnaces were explained with the help of large diagrams and dissecting models, whereby the leading points of their construction were clearly demonstrated. The principle of Mr. Siemens's plan of heating is already well known; the ordinary form of regenerative gas furnace having, indeed, been recently described by Mr. Henry Chance, who advocated its use in glass making. The fuel is heaped upon an inclined fire-grate, where it undergoes a kind of slow combustion, which results in the formation of carbonic oxide; this inflammable gas is then conducted from the "producer," to the hearth or working platform of the furnace, where it meets a current of air already

raised to a high temperature, which enables it to burn with great intensity, and the excess of thermal power, instead of being allowed to pass directly into the chimney, and become thus wasted, is forced to traverse an intricate structure of brickwork, which absorbs so much of the heat, that the escaping gaseous products of combustion rarely indicate a temperature above 300° Fahr. At a suitable stage of the operation the gas valves are reversed, and both carbonic oxide gas and air forced to traverse these heated brick chambers in a contrary direction, so that they may in turn become the recipients of that heat which, in ordinary constructions of furnaces, would have been lost. The ashes and clinkers are in this way entirely separated from the region of manipulation, and are removed from the grate at intervals of one or two days. The composition of the inflammable gas varies with the nature of the fuel and the draught of the furnace; by way of illustration, Mr. Siemens quoted the following, as representing an average result for the gas from the producers at the St. Gobain plate glass works, where caking coal is employed in admixture with one-fourth of a non-caking quality :—

Carbonic oxide	24·2
Hydrogen	8·2
Carburetted hydrogen	2·2
Carbonic acid	4·2
Nitrogen	61·2
	100·0

Besides the above, there are always present aqueous vapour, tarry matters, and small proportions of sooty and earthy particles. The author estimates the temperature of the gas as it rises from the fuel at 1000 to 1300° Fahr., but it becomes considerably hotter in its passage to the area of combustion beneath the crown of the furnace. An ingenious electric resistance pyrometer, with platinum helix, which has done good service in measuring these elevated temperatures, was exhibited; and an experiment was performed in the lecture-room, for the purpose of demonstrating the principle of the "regenerators," lead being melted in the reversed current of hot air blown through a heated stack of flints previously ignited over a Bunsen burner. The special furnace, which serves for the direct production of steel, has a tunnel head, or cylindrical hopper, fed with iron ore and small coke passing through, and raised above, the crown of the regenerative gas furnace; the lower part of this upright cylinder rests in a bath of molten pig-iron, which dissolves the reduced (spongy) iron as quickly as it is separated from the ore. A blast-pipe descends through the stack of ore, and the process is interrupted when the steel is ready for casting, after which the charge of pig must be renewed.

In the case of using bar iron (worn out rails) in the preparation of steel, they were introduced into an inclined hopper with their lower ends resting in a bath containing 10 per cent (by weight) of molten pig; and the atmosphere of the furnace might be changed at will into an oxidising, reducing, or perfectly neutral condition, merely by altering the proportions of air and gas. It is important to keep the carbon at a minimum, for then the silicon is lowest.

The lecture was amply illustrated by a series of diagrams showing the construction of the several kinds of regenerative furnaces in plan and section, and a large model of a gas producer was upon the table. The following specimens were also exhibited :—

Cast steel produced by melting Bessemer scrap in Siemens's furnace.

Cast steel produced by melting iron rail ends from Hayance, France, containing nearly 1 per cent of phosphorus.—Siemens's furnace.

Magnet steel containing tungsten.—Pot furnace.

Steel made in open furnace from Staffordshire iron.—Carbon, 0·37 per cent.

Steel from puddled iron from Creusot.—Open furnace. Carbon, 0·38 per cent.

Ingot of steel made in open furnace from red and cold short metal.—Carbon, 0·4 per cent.

The same metal tilted.

Steel made from Bessemer scrap in open furnace, containing 0·46 per cent of carbon.

The President, having proposed a vote of thanks to the lecturer, took occasion to remark that the regenerating furnaces offered special facilities for making experiments upon steel, with the view of ascertaining the effects of different adventitious substances. The statement of the wonderful magnetic properties of tungsten steel surpassed all that the Dutch had accomplished, and it would now be interesting to study the influence of boron.

A discussion followed, in which Mr. Cowper, Professor Abel, Dr. Miller, Dr. B. H. Paul, Dr. Williamson, and Dr. Odling took part. The meeting was then adjourned until the 21st instant.

ROYAL INSTITUTION OF GREAT BRITAIN.

Weekly Evening Meeting, Friday, March 13, 1868.

"*On the Probable Exhaustion of our Coal Mines*," by W. Stanley Jevons, M.A., Professor and Cobden Lecturer on Political Economy in Owens College, Manchester.

(Concluded from page 226.)

No statements of the total amount of coal produced in this kingdom are the least to be relied on, except those collected by Mr. Robert Hunt, the Keeper of Mining Records, and the following is a statement of the general progress of the coal trade of the United Kingdom as ascertained by him :—*

Year.	Coal raised. Tons.	Coal exported. Tons.
1854	64,661,000	4,309,000
1855	61,453,000	4,976,000
1856	66,645,000	5,879,000
1857	65,394,000	6,737,000
1858	65,008,000	6,529,000
1859	71,979,000	7,081,000
1860	80,042,000	7,412,000
1861	85,635,000	7,222,000
1862	83,968,000	7,694,000
1863	88,292,000	7,529,000
1864	92,787,000	8,063,000
1865	98,150,000	8,585,000
1866	101,630,000	9,367,000

It is impossible to view, without some degree of alarm so rapid an increase of the coal trade as the preceding figures indicate. Without doubt our production will advance to 200 millions before very many years are past; and the alarming calculation may be made that if we went on increasing our production of coal for 110 years as rapidly as we have done during the last 12 years, our coal seams would be worked out to a depth of 4000 feet. But such a supposition is put forward, not as a serious possibility, but as a *reductio ad absurdum*. The conclusion to be drawn from it is simply that the nation cannot possibly progress in material wealth for 110 years more as rapidly as it has done in the present century. The limited extent of our coal-fields would not allow us to go on increasing the draught of coal as lavishly as we have done. But it is the very necessity of changing from a highly progressive to a less progressive or stationary condition, that is most grievous. Population and pro-

* I am kindly informed by Mr. Hunt that when the returns of the consumption of coal in 1867 are completed, the total will probably amount to 104,000,000 tons, showing continued increase in spite of the depression of trade.

duction, when once set in motion, move with a certain impetus, and it is the check to such motion which is distressing and threatening.

VIII. The subject wears a more serious aspect still when we consider the coal resources and production of other countries as well as our own.

According to the latest returns which are at hand, it would seem that the total known produce of coal in the world is thus distributed over the chief nations:—

	Tons.
Great Britain	101,630,000
United States	25,800,000
Prussia and the Zollverein	20,610,000
France	10,710,000
Belgium	9,935,000
Austria	4,500,000
British North America ..	1,500,000
Russia..	1,500,000
Spain	300,000
New South Wales	250,000
Ireland	123,500
Total	176,858,500

It would appear then that of the total *known* produce of coal in the world we raise considerably more than half (57 per cent), although we form probably not more than one in forty of the population of the world. If to our own coal produce we add that of the United States and our colonies, we may conclude that the Teutonic race enjoys 73 per cent, or almost 3 parts out of 4, of the coal raised. It is hardly possible to over-estimate the forces acting in our favour which are represented by this partial monopoly of the most powerful material agent of civilisation.

The total quantity of coal existing can hardly be said to be known in the case of any one country; but some notion of the comparative coal resources of different countries may be gained from the following statement of the area of the coal-measures in the chief coal-producing countries, as estimated by Professor Rogers:—

	Area of Coal Lands in square miles.
United States	196,650
British North American Possessions	7,530
Great Britain	5,400
France	984
Prussia	960
Belgium	510
Bohemia	400
Westphalia	380
Spain	200
Russia	100
Saxony	30

Though Great Britain is far more abundantly provided with coal than any continental nation, our resources sink nto insignificance beside those of North America, and no very long period will elapse before this comparative poverty in coal will make itself felt.

IX. It is continually suggested, indeed, that before coal is at all likely to be exhausted, some substitute will be found for it, and appeal is made to some old proverb, like "Necessity is the mother of invention." But it requires very little philosophy to see that the proverb is very partially true. We live in a chronic state of necessity and difficulty, and the great discoveries which we enjoy are but so many exceptional instances in which we have been unexpectedly relieved from labour and evil. We have no real ground for supposing that when one exceptional advantage is withdrawn from us, another will immediately be extended to us.

The favourite notion that electricity will be the future source of power is entirely fallacious; for the coal-driven engine moving the magneto-electric machine is now the cheapest source of electricity, and by gradual improvements, such as that in Mr. Wilde's machine, coal will become a still cheaper source of electricity. Even the elements of the electric battery have always been practically furnished by the reducing power of coal. If coal then become, as there is every reason to suppose it will, a cheaper and cheaper source of electricity, it is obviously absurd to suppose that electricity should supersede the power of coal.

It is conceivable, indeed, that in the course of ages some wholly new source of power might be discovered; but there is no reason to suppose that this island, which forms but the one four-hundredth part of the total land-area of the globe, would be as richly endowed with the new source of power as it is with coal. If the sun's beams are in the future to be the direct source of power, it is the plains of Africa or of Australia that will be the seats of industry and not this cloud-obscured Isle.

X. The conclusions we must come to on this subject are then as follows:—

1. The power of coal is extending itself and making itself more widely and deeply felt every day. It is more and more taking the place of wind, horse, or manual power, and is becoming the universal assistant.

2. We are naturally led every day to extend our consumption of so invaluable a substance, and experience shows that the more we use the more extensive are our augmentations.

3. Our consumption is already commensurable with our total supply; that is to say, we can form some notion how long our supply will endure with a stationary consumption.

4. As this consumption increases by multiplication, our national life becomes shortened, and it is apparent that the increase cannot go on very long at the present rate.

5. The moment we are forced to draw in, other nations, possessing far more extensive fields of coal compared with their annual consumption, will be enabled to approach and ultimately to pass us.

6. The exhaustion of our mines, as it will probably manifest itself within the next hundred years, will consist not in any stoppage of supplies, but an increase of cost, and the impossibility of increasing the consumption each year as at present.

XI. At some future time then, when coal will be even a more useful agent than at present, we shall stand in a position of comparative inferiority. For such a time we can best prepare ourselves, not by short-sighted restrictions on the consumption or evaporation of coal, but by freeing the nation from its burdens of debt and ignorance and pauperism. We have many great tasks to perform, which can only be undertaken with a fair hope of success when the nation is in a state of high prosperity and progress. It will be too late to think of such great undertakings when our progress is checked, and the pressure of population and the want of employment are grievously felt. It is in a period of free expansion like the present that we can alone take any effectual measures for raising appreciably the standard of education, comfort, and morality of the people; and if we do not use the abundant wealth which our coal resources now afford us to fulfil such duties, we undoubtedly misuse it.

FOREIGN SCIENCE.

Paris, May 13, 1868.

Sewage of Paris.—New process for the manufacture of sulphuric acid.—New method of preparing carbonic acid gas for mineral waters.—Academy of Sciences: Theory of electro-capillary phenomena.—Identity of artificial with natural nevrine.—New calorimeter for rapid combustions.—Laws of the transformation of paracyanogen into cyanogen.

The sewage question is one which has lately attracted considerable attention in Paris; the problem, of course, has been to remove the polluted waters from the town in the most advantageous manner. The volume of these waters is now 100,000 cubic metres a day, soon it will be double this amount, and in a few years

probably 500,000 to 600,000 cubic metres. We have three solutions of the difficulty. The first and most obvious is to carry the sewage into the Seine; this scheme has been well tested already, and the disadvantages seem generally more striking than the advantages. The advocates of a second plan would employ the sewage, which they would first raise by machinery to a considerable height, in the irrigation of the fields. The fertilisation of the sands at the mouth of the Thames by this means is cited in favour of the plan. The third scheme recommends itself as being the most scientific, and it is, perhaps, the best; experiments have been made with this scheme since the commencement of the spring. The sewage waters, collected in large basins, are mixed with a certain amount of sulphate of alumina—about 1 per cent to the cubic metre. Organic matters contained are rapidly precipitated, each cubic metre yielding about 3 klgrms. of solid manure. The decanted fluid, termed clear water, can be employed in the irrigation of soils, upon which it has a very fertilising action; it contains, in fact, small quantities of mineral matters in suspension, a little nitrogenous and organic matter, and the whole of the alkaline salts. The deposit obtained in the clarification is abundant and compact; it contains the whole of the phosphoric acid and nine-tenths of the nitrogenous and organic matter, and the mineral matters dissolved or in suspension; it constitutes an excellent manure, very fertilising, and easily transportable. Towns would thus be considered as manure factories, and it is believed that the value of the manure may be made to defray the expense of supplying the town with pure water.

M. Lardani has devised a new method of manufacturing sulphuric acid. The plan may be sketched as follows:—Sulphurous acid, in the presence of excess of air, is passed into dilute nitric acid, which becoming itself reduced, oxidises the sulphurous acid; the sulphuric acid, being very dense, sinks to the bottom of the reacting vessel; hyponitric acid escapes, and traversing the upper part of the apparatus, enters the regenerator, where, meeting with water and excess of oxygen, it produces nitric acid. The apparatus is composed of a furnace for burning sulphur, a washer or scrubber, a refrigerating apparatus, a reacting vessel, and a regenerator for nitric acid. The furnace in which the sulphur is burnt is traversed by a current of air obtained by a ventilator; this current, while furnishing oxygen, serves to chase out the sulphurous acid, the density of which hinders the rapid replacement of air, and thus the rapidity of combustion. Leaving the furnace, the warm sulphurous acid gas enters the scrubber, where it is freed from volatilised sulphur, and especially from arsenious acid when arsenical pyrites has been employed as the source. From this part of the apparatus the gas, passing through a pipe surrounded by cold water which condenses the water and cools the gas, becomes denser, and descends into a cascade apparatus, through which a current of weak sulphuric acid, still containing nitrous products, is made to flow. The reacting vessel into which the gas passes is composed of two parts: the lower portion contains weak sulphuric acid, upon which rests a thick stratum of nitric acid; the upper portion, separated by stoneware plates, or plates of lead or aluminium pierced with holes, contains pumice-stone saturated with water. The sulphurous acid, mixed with a powerful current of air, plunges into the fuming nitric acid, and the escaping gaseous products traversing the layers of pumice-stone, become exhausted by the time they reach the fifth receiving vessel destined to reoxidise the nitrous compounds.

M. Boudet has read a short report before the Academy of Medicine upon a new process, by means of which M. Ozouf obtains pure carbonic acid for use in the fabrication of mineral waters. At the present time the carbonic acid used in the preparation of these waters is produced by making sulphuric acid act upon marble; the calcareous carbonates, however, contain foreign substances not usually separated in the process, and the sulphuric is not completely separated in the washing. To avoid these defects, M. Ozouf has had recourse to the combustion of coke; he thus prepares pure carbonic acid. M. Boudet described at length the rather complicated apparatus employed, adding that the commission were able to testify to the excellent quality and purity of the products. The process obtained general commendation.

At the *séance* of the 20th of April, the following memoirs were communicated to the Academy:—"On the theory of electro-capillary phenomena, comprising endosmose, exosmose, and dialysis," by M. Becquerel; "On the identity of artificial nevrine with natural nevrine," by M. Wurtz; "A new calorimeter for rapid combustions," by M. Favre; "Laws regarding the transformation of paracyanogen into cyanogen, and the inverse transformation," by MM. Troost and Hautefeuille; "Note on the preparation of the salts of sesquioxide of iron, and on ferric chloroxide (Fe_2Cl_3,Fe_2O_3+Aq)," by M. Jeannel; "Note on the manufacture of phosphate of soda and fluoride of sodium;" "On a new acetate of chromium," by M. Schützenberger; and "On some derivatives of the radical silico-allyl." The number of papers of chemical interest is seen to be unusually large; an account of M. Wurtz's synthesis of nevrine having already appeared in this journal, an abstract of his present memoir will, doubtless, be read with interest.

M. Wurtz has obtained the chloride of nevrine extracted from the brain and the chloride of the artificial nevrine in long deliquescent needles, by dissolving the dry salt in absolute alcohol, and cautiously pouring on to the surface of the moderately concentrated solution anhydrous ether. The natural chloride of nevrine was separated from the auro-chloride by sulphuretted hydrogen, the solution filtered from sulphide of gold being evaporated, first on the water-bath, then in vacuo. The chloroplatinate of trimethyl-oxethylammonium is very soluble in water, but insoluble in alcohol. When the precipitate produced by alcohol in the aqueous solution is redissolved in water, and the solution allowed to evaporate spontaneously, magnificent clinorhombic prisms, of an orange-red colour, are obtained; crystals of regular and considerable dimensions may be obtained. Among the properties of chloride of nevrine which have been pointed out by M. Bayer, one of the most characteristic is its reduction by hydriodic acid. The oxethylic base is converted in this case into an iodethylic base. The iodide of trimethyliodethylammonium thus formed is little soluble in cold water, and is deposited in fine crystals from the boiling aqueous solution. M. Wurtz obtained abundance of this substance by reducing artificial chloride of nevrine by hydriodic acid in presence of phosphorus, at a temperature of 140°. By ebullition with water and oxide of silver, this iodide of the iodethylic base is converted into the hydrate of the corresponding vinylic base. M. Wurtz has performed this experiment upon the iodide made with artificial nevrine; M. Bæyer indicated this reaction for natural nevrine. In saturating with hydrochloric acid, the hydrate resulting from the action of oxide of silver upon this iodide, and adding chloride of gold, M. Wurtz has obtained a yellow precipitate, soluble in boiling water, and depositing, upon cooling, small crystals, which have the composition of auro-chloride of trimethylvinylammonium:—

$$\left.\begin{matrix}(CH_3)3\\(C_2H_3)'\end{matrix}\right\}NCl+AuCl_3$$

The dilute solution of hydrate of timethyloxethylammonium (free nevrine) may be boiled without undergoing sensible decomposition; but this is not the case with concentrated solutions: they disengage trimethylamine, a reaction already indicated for natural nevrine. This is not the only product of the decomposition. When a flask, in which the solution has been completely evaporated, and in which nevrine no longer remains, is allowed to cool, a small quantity of a thick, slightly brown liquid is condensed. This body only boils at an elevated temperature; a small quantity boiling above 190°, collected,

presented the characters of glycol. Heated with dry potash this body liberated, in fact, pure hydrogen; nitric acid vividly oxidised it. The formation is easily understood; hydrate of trimethyloxethylammonium, under the influence of heat, splits up into trimethylamine and glycol:—

$$\left.\begin{matrix}(CH_3)3\\C_2H_4OH\end{matrix}\right\}NOH=(CH_3)3N+C_2H_4\left\{\begin{matrix}OH\\OH\end{matrix}\right.$$

This reaction affords the first example of the formation of glycol at the expense of a natural substance.

NOTICES OF BOOKS.

The London Student. No. 1. 1868. John Churchill and Sons.

The appearance of this, the first number of a new monthly journal devoted to educational purposes, deserves a cordial welcome from all who are interested in the intellectual progress of this country. Whatever opinions may be formed as to the want of such a periodical—and it may be thought by some to be almost too serious and ambitious an undertaking to enter into successful competition with its lighter contemporaries—there can be no question that this number, from the intrinsic merit of its contents, deserves a circulation far and wide, and the most serious attention from all who are in any way connected with educational pursuits.

The work commences with a plea for more universities, by Professor Seeley, who strongly urges the advisability of a union between the London Colleges, and the consequent formation of a London University worthy of the name. "If," as Professor Seeley eloquently urges, "the British Museum is a university; if every hospital is a medical university; if the Royal Academy is a university of Art; if besides these there exist in London a number of so-called colleges, in which the fundamental condition of the university system is fulfilled—namely, that the teacher is not absorbed in teaching, but has leisure for study and research; lastly, if London is the head-quarters of all those learned societies—which are universities in the purest rudimentary form—may we not justly say that London contains the chaos of the vastest university in the world, and that little more than a word is wanting to call that university into being? These multitudinous institutions have but to unite—nay, they have but to will to unite, and the thing is done. If I may use a bold figure, London, the head of the empire, conceals behind its capacious forehead an intricate network of nerves, in the convolutions of which go on thought, observation, speculation, but no one has thought of giving a name to the mass. Let us once learn to think of it as a whole, and we shall see that it is the brain of the country."

But before the London Colleges can unite permanently they must make each other's acquaintance, and grow accustomed to each other's society, and on this point Professor Seeley writes:—

"It has long been discovered that as a cement and symbol of unity among men that have similar interests and are engaged in similar pursuits, there is nothing more useful than a magazine. The *London Student* has been projected with the view of representing the University in London both to its members and to the outer world. To the colleges themselves, both governing bodies and students, it will display the deficiencies of this great University; to the world it will display its merits. What these deficiencies and merits are, has now, I hope, been made clear. It wants unity and organisation; it possesses everything else which the age requires in a university—a vast assemblage of learned men, vast museums and libraries, variety of instruction, cheapness, absence of great endowments, a disposition to make progress and try experiments, and lastly religious comprehensiveness."

The most important paper, and certainly the one most likely to interest the readers of the Chemical News, is on "*Experimental Science the basis of General Education*," by Professor Williamson. We make no apology for placing before our readers the following extracts from this eloquently written article:—

"If a man were entitled to a rich and glorious inheritance, of which the possession would open up to him an extensive sphere of usefulness and happiness, and if he did not claim his rights, one would naturally suspect that he was not aware of their existence, or that he had not been correctly informed of their value.

"The English people is now in the position of such a man, for it is undisputed heir to the noblest estate in the world, and has never taken possession.

"The estate is managed by trustees who reside upon it and honestly devote their whole energies to its improvement, while supplying the heir with rich and abundant produce.

"From time to time the estate is visited by friends of the heir-at-law, but a strange fascination prevents their ever returning to him. Once their feet have touched the soil, they are drawn onwards and upwards till they find themselves among the trustees, with whom they set to work improving the estate. Meanwhile the heir is living in foreign parts, satisfied with the rich produce which reaches him. Messages are sent to him to come and take possession, and enjoy the estate himself, but he heeds them not, for travellers who pass by its outskirts tell him that they do not see much in it, and that the only good thing about it is the produce which he receives.

"This unclaimed inheritance will no doubt be named when it is sufficiently known. Meanwhile we may describe it as *knowledge*:—

"Knowledge of the the most trustworthy and serviceable kind:

"Knowledge of the phenomena of nature:

"Knowledge of man's own powers and their limits, just sufficient for the purpose of obtaining more:

"Knowledge of the order of nature and of the rudiments of her laws.

"Experimental science, which is the embodiment of such knowledge, is usually associated in England with the name of Francis Bacon, who so emphatically proclaimed that its methods afford the only safe guide to the human reason in the search for truth.

"The trustees whose labours develope it in all directions are those who question nature by rational experiment, and record her answers, arranging them in intelligible order like letters into words, and learning from these words the laws of nature. These experimentalists supply man with numberless useful and agreeable products. Their numbers are constantly increased by new members, who enter upon science with the desire of seeing what it is, and are drawn on by an admiration of its beauty and harmony to become themselves workers in its domain.

"Popular writers who hover upon its outskirts describe to the world what they have seen, and the world judges science to be mainly destined as a handmaid to the industrial arts, and claims that as her highest use.

"Take as an illustration the report of the great Macaulay; he says, that 'Bacon's philosophy aimed at things altogether different from those which his predecessors had proposed to themselves;' that its object was 'to increase human comforts, to relieve more effectually the inconveniences of human life, to endow human life with new inventions and forces;' that it 'began in observations and ended in arts.'

"Again: he says that a follower of Bacon, if asked what the new philosophy has effected for mankind, would name the prominent material results which have followed from it. 'It has lengthened life, it has mitigated pain, it has extinguished diseases, it has increased the fertility of the soil, it has given new securities to the mariner,' and so forth; but not one word of teaching a better use of

human reason, or of showing the true helps to the human understanding.

"So truly does this represent the commonly prevailing opinion of this country, that experimental science is almost exclusively studied by those who wish to apply it to some technical purpose; and most persons would be surprised to hear that the most important of all the applications of science is to the business of general education.

"A brewer wishes his son to learn chemistry, because he knows that the quality of beer depends upon chemical processes which can be regulated by one who understands them. A dyer values the science upon similar practical grounds. But a schoolmaster, who has to watch the direct growth of young minds, and to supply each with the food best calculated for its development, is satisfied that the knowledge of a couple of dead languages and of mathematics, fully qualifies him for the work; and while admitting the utility for technical purposes of scientific studies, he usually ignores their educational value."

. . . . . .

"A century has not elapsed since a discovery was made which must ever rank amongst the most remarkable achievements of philosophy. From the first dawn of human reason, the process of combustion had been an object of wonder not unmixed with awe. Men gradually learnt to obtain fire and to use it for various purposes, while the nature of the process by which it was maintained continued an impenetrable mystery to them. Various substances were found, such as wood and resin, capable of feeding a flame and of keeping it alive, but the fuel gradually disappeared whilst supporting the flame, and was supposed to be destroyed by the process. Powerful intellects strove to work out an explanation of the process of combustion, and gave words instead of facts. The process remained a complete mystery till experiment was brought to aid the reasoning powers.

"In the year 1774, Priestley obtained a gas by heating calx of mercury, as it was called. He found that combustible bodies burn in this gas with far greater intensity and brilliancy than they burn in common air; and that charcoal forms carbonic acid by burning either in air or in this gas. He declared it to be the active principle to which air owes its power of supporting combustion. We now know that combustion could not be explained without a knowledge of this gas, and that in reality Priestley's discovery supplied the key to the whole mystery.

"His mind was, however, so constituted as to discover facts by experiment, and not to discover their place in the order of nature. Priestley's energies were not devoted to explaining the process of combustion; he was a conservative in theory, and he called his wondrous gas 'Dephlogisticated air'—a name which was retained until the new theory of combustion suggested a better one.

"It was, however, not long before Lavoisier turned the discovery to account in his brilliant and masterly theory of combustion. He proved that this gas, which he named oxygen, unites with the materials of combustible bodies when they are burned in the air, and that the compounds thus formed contain the oxygen and the combustible materials.

"Combustion is a combination which gives off heat and light, but which does not destroy the elements taking part in it. If we burn wood or tallow or resin, and collect the products formed by their combustion, we can recover all the materials of the wood, or tallow, or resin, and the oxygen which had united with them.

"Those who know the wondrous light which this theory has thrown upon natural processes, and the vast amount of knowledge of the properties of transformations of matter which it has rendered accessible to us, cannot hesitate to class it among the most important results which man has yet attained.

"It enables us to understand, to classify, and to describe an infinite number of processes of change discovered by chemists; processes of which a few only have been explained in any other way at all, and those few in a most inconvenient and clumsy way.

"If we compare man's present insight into Nature (imperfect as it is), with that which he would have if this idea and its fruits were taken from him, it is like comparing daylight to the faint glimmers of starlight. We now see changes in the properties of matter which take place when different kinds come together under particular conditions, and we learn how to regulate those changes by our knowledge of their nature and conditions, where formerly our uncertain glimpses gave us confused and untrue impressions of destruction and production of different kinds of matter. We are delighted with a perception of natural forces working silently and irresistibly; and the more we observe and compare the results produced by these forces under various conditions, the more order and harmony do we find pervading the infinite variety of their manifestations.

"But while our power over matter has increased, we have become aware of its limits; for we now know that we can neither destroy nor create matter, only arrange, distribute, and alter its properties.

"Greatly as we must rejoice at these results, viewed as mere instruments of thought, they sink into comparative insignificance beside the method which led to their discovery, and which leads onwards to all future extensions of our knowledge. What made Priestley examine the properties of air itself and compare them with the properties of air in which a piece of charcoal had been burnt? What made him examine the gas given off by heating mercuric oxide or by heating nitre? He was cognisant of the facts and ideas established by previous investigators respecting combustion in air; and he put such questions to Nature as were suggested to him by a consideration of the results before him, receiving in reply facts of momentous importance. What made Lavoisier establish the theory of combustion by arranging Priestley's facts in natural order among other facts relating to combustion, and describing that order? Both men had the same materials of the thought before them, and both were interpreters of Nature; but two minds more different in their constitution could hardly have been, and two pieces of work more different than theirs can hardly be found. Priestley found the materials, but could not arrange or explain them. He could not even see the explanation when given by another. Lavoisier's chief merit was in arranging the facts given to him. He measured the amount of the changes which had been discovered, and his theory of combustion is the simplest statement of the result of a quantitative comparison of those changes. He had a genius for discovering order—Priestley had a genius for discovering isolated facts. The work of each harmonises with each other, and their very differences made them the more necessary to one another.

"Some persons might attribute this great result to chance (a common name for ignorance of causes); but those who examine carefully the course of discoveries and investigations prior to Lavoisier, will see that facts and ideas had been steadily accumulating for his theory; and that even if Priestley and he had not lived, others would doubtless have been led by the same laws of progress to make the discovery. Indeed it so happens that there lived in a little town of Sweden a quiet and modest man named Scheele, who, following quite independently the path of research, discovered oxygen nearly as soon as Priestley, and by a distinct process. Many a new truth was brought by that quiet honest man from the fountain head of knowledge, by the aid of the facts and ideas supplied by previous investigators.

"When we look back upon the work of that period, free and independent as the workers felt themselves to be, and various as were their aptitudes and peculiarities of mind, we see that in all parts of the field they were moving forward in the same direction, and helping one another by an involuntary division of labour. When we examine the work which has been done more recently, we see that

the results of those great men have been used as instruments of thought by the latter workers, and gradually developed by further investigations, while new discoveries have been made and used in their turn as additional instruments of thought to extend still further our means of gaining knowledge. The first discoveries alone were within reach of the limited knowledge then available for experimental purposes, and they were needed for the latter work.

"The mind of man has only progressed and can only progress in a particular order. It must begin with the simple and rise gradually and slowly to the complex. The very attempts to deviate from this order only serve by their failure to prove its inevitable necessity.

"One other episode of modern history seems worthy of consideration. It is not many years since there existed among active chemists a difference of theory which was considered important. The difference was expressed by the words Type and Radical. Some explained compounds as built up upon types, others contended that they were built up from radicals. Each party appealed to facts which favoured its own view, but neither party paid much attention to the facts quoted by its opponents. We now know that each party was right, except in its denial of the other statement. Each theory describes truly a certain number of facts, and a more general theory includes them all. Our present theory required the study of compounds from the point of view of radicals and also from the point of view of types, and they are, as it were, legs upon which our more general theory stands."

In the second part of his paper, published in the May number of the *London Student*, Professor Williamson points out some of the educational uses of experimental science.

CORRESPONDENCE.

SCIENCE TEACHING IN SCHOOLS.

To the Editor of the Chemical News.

Sir,—The difficulty for a conscientious lecturer on experimental science, seems to me to consist in his thinking himself obliged to go on from point to point with only a little reiteration, and with no oral questioning of his hearers. Grammar probably requires more reiteration, more catechetical friction and raking, than physical science; but, to a certain extent, their demands must be similar. A teacher of grammar can do nothing without the frequent, sudden, lively interposition of questions; he cannot be satisfied that grammatical conceptions are formed in the learner's mind, except by seeing that the learner works under varied conditions. I imagine that a teacher of science ought to make sure that he is teaching, not merely by one examination at the end of a course of lectures, but by frequent catechetical examinations.

I sometimes sit amongst the young listeners to an experimental lecture: they have an infinite advantage over me in observing the experiments; but I have an equivalent advantage over them in recognising at once the terms used by the lecturer, by which they are often bewildered; and I long to stop him, and insist on his waiting till he is sure that they understand him. He assumes, I do not assume, that they at once recognise and understand such terms as "suspension," "solution," "inverse ratio," "refraction," "specific gravity."

It would be a robbery of precious time if he stopped to grind such conceptions into the minds of the ordinary boy: that is his view. Be it so: then there must be a division of labour. Let the demonstrator go on with his lecture: let the hack schoolmaster at other times teach the terminology; any cultivated man ought to be able to do this. We, who live with boys, and become, or remain like boys, know better than the lecturer knows their shoals and fogs of misconception, their power of forgetting, their innocent hypocrisy in assenting, with a look of intelligence, when they are really perplexed.

Unluckily, there are but few men who care to go over the ground that another man has ploughed, and pick out the weeds that he has left behind.

Having now and then done this for your respected correspondent, Mr. Rodwell, I recommend it as an ancillary department of that science teaching which is required of our schools.—I am, &c.,

William Johnson.

Eton, May 5.

CHLOROCHROMIC ACID TEST.

To the Editor of the Chemical News.

Sir,—This test, which is implicitly asserted by some text-books and teachers to give sure and distinctive evidence respecting the presence of chlorine or a chloride, seems to me to fail in the case of $PbCl_2$, Hg_2Cl_2, and AgCl.

In operating, I powdered and intimately mixed in a dry mortar about ½ a grm. of each of these materials with double the quantity of neutral chromate of potassium; after drying the mixtures they were well moistened with strong, pure, warm H_2SO_4, when there occurred no visible evolution of any vapour, either red or yellow; the silver mixture smelled feebly of chlorine.

Hence it appears that the chlorine in an insoluble chloride may escape detection by this method, and the process will only therefore be reliable after we have replaced bases furnishing such, by others (say the alkalies) which will admit of solution in the menstruum sulphuric acid.

Since both $BaCl_2$ and $CaCl_2$ yield abundant fumes of CrO_2Cl_2 on similar treatment, it would seem that the preceding instances of the failure of the test are not due to any insolubility of the after-products; but I must leave others to theorise.

Should there be any novelty (which I can hardly imagine) or utility in this observation of a student at the threshold of the gate of science, perhaps your impartiality will lead you to communicate it to others similarly situated.—I am, &c.,

B. W. Gibsone, M.A.

Eaton Square, May 13, 1868.

MISCELLANEOUS.

Galvanic Action of Copper-Bottomed Ships in Dock.—The *Elk*, 2, twin-screw (composite-built) gunboat, 465 tons, 120-horse power, launched during the past winter from Portsmouth Dockyard, and subsequently fitted with her engines and boilers, was placed 10 weeks since in the old shipping-basin of the yard, to wait there the finishing of her pair of screws, which were ordered to be cast from enlarged patterns. On Tuesday last she was taken out of the basin again and docked to receive her screws, which had in the meantime been completed for her. On attempting to clean the ends of the shafting, however, to receive the screws, it was discovered that galvanic action had been at work to such an extent that the "key" pieces on the shaftings were reduced to plumbago, and other parts of the metal "honeycombed." The fact appears to be that the small area of water in the old ship basin is but seldom open to the admission of the tide. has always three or four copper-bottomed vessels floating upon it, and is, therefore, a chemical bath, whose power has been so unexpectedly, yet convincingly, displayed upon the screw shafts of the *Elk*.

NOTES AND QUERIES.

Nitrate of Iron.—Can any of your readers inform me how to make a good and cheap nitrate of iron? By inserting the above in the columns of your valuable paper you will much oblige—A CONSTANT READER.

Preparation of Subchloride of Copper.—The directions are:—Digest 4 parts finely divided metallic copper, and 5 of common black oxide of the metal in hydrochloric acid: prolonged digestion is necessary. Regarding the above I used the proportions given, and left them to digest in an open porcelain basin, using strong hydrochloric acid; after a week the whole of the metallic copper was not dissolved. 1. Should strong acid and a wide open basin be used of digestion? 2. Should it be digested with application of heat or not? 3. When the reaction is complete, should any of the metallic copper be left if used in proportions given? 4. Is this (Mallet's) method the cheapest and best for making oxygen in large quantities, or is the chloride of lime, or the other French processes, better and cheaper?—ENQUIRER.

Extract of Madder.—There are met with in the trade several preparations of madder, known as garancine, flowers or bloom of madder, garanceux, azale, and colorin; the two latter are, in a sense, extracts. Azale is obtained by exhausting flowers of madder, fleur de garance, with boiling wood-spirit (not methylated spirit, but the methyl-alcohol of commerce); the mass is put on a filter, and to the filtrate water is added, whereby a copious yellow residue is thrown down, which is washed with water, dried, and afterwards applied for dyeing. Colorin, however, is obtained from madder by exhausting it with boiling alcohol, filtration, evaporation on a water bath, when an extract will be left which contains all that is soluble of madder in alcohol; it is better to use garancine for this purpose, than madder, as it yields a better and far more pure product. If your correspondent "W. B." requires more details, he had better consult the numerous books on this subject published.—Dr. A. A.

TO CORRESPONDENTS.

Authors of Papers, desirous of having extra copies printed for their own use, are requested to communicate with the Printer, Mr. DUTTON, at the Office, Boy Court, Ludgate Hill, E.C.

Messrs. Townsend and Adams, N.Y.—Mr. Horsley's address is Laboratory, Police Station, Cheltenham, England.

J. Mayer.—The report is unavoidably postponed till next week.

Natrium.—Hyposulphite of soda is made at alkali works as a by-product in the manufacture of soda. You cannot make it economically from sulphate of soda.

A. B. C.—Olive oil is sometimes adulterated with poppy, sesamé, earth-nut, or colza oil. The methods of detecting these adulterations are not very satisfactory; a good account of them is given in "Watts's Dictionary of Chemistry," under the heading "Oils."

J. Denham Smith.—A proof shall be forwarded. The letter arrived too late for insertion this week.

W. Berrie.—It is said that aniline colours may be discharged by a printing paste of powdered zinc and starch, and subsequent washing with acid.

W. B.—We can only insert your queries as an advertisement. It is not suitable for our Notes and Queries column.

A Manufacturer.—See answer to W. B.

M. A. Whichelo.—In commercial examination of tobacco, the leaf should first be examined physically and microscopically, to detect adulteration. Then examine for ammonia and nicotine. Usually an estimation of the latter alkaloid present will suffice, in addition to that of the water, ash, and total nitrogen. Consult on this subject a very good article, by F. F. Mayer, of New York, reprinted in the CHEMICAL NEWS, vol. xii., pp. 74-88.

Communications have been received from W. O. Reichardt; W. Chapman; G. F. Rodwell (with enclosure); M. Carmichael; C. Chateau (with enclosures); W. White; Dr. R. Angus Smith, F.R.S. (with enclosures); Walter Woodbury; J. Samuelson; Dr. B. H. Paul (with enclosure); E. A. Teschemacher and J. Denham Smith (with enclosure); F. W. Brearly The Aëronautical Society of Great Britain; Dr. E. O. Röhrig; Rev. F. Sonley Johnstone; H. B. Condy (with enclosures); W. Briggs; W. Berrie; J. Mayer (with enclosure); Alfred Bird (with enclosure); Wm. Briggs; F. C. Calvert & Co., Manchester; Mawson and Swan, Newcastle-on-Tyne; W. A. Larder, Louth; Sampson Low & Co.: Dr. T. Wood (with enclosure); R. Campbell, jun., Montreal.

We are indebted to correspondents for the following periodicals, containing reports and articles of Chemical or Scientific interest:—"The Acadian Reporter;" "The Darlington and Stockton Times;" "Mining and Scientific Press;" "American Artisan;" "American Journal of Mining;" "Journal of Gas Lighting."

MEETINGS FOR THE WEEK.

TUESDAY.—Royal Institution, 3. Dr. M. Foster, "On the Development of Animals."
—— Pharmaceutical. Conversazione.
WEDNESDAY.—Society of Arts, 8.
—— Pharmaceutical. Annual meeting, 11.
—— Geological, 8. 1. Dr. J. Schmidt, "On the Eruption of the Kaimeni of Santorin." Communicated by Sir R. I. Murchison, Bart., K.C.B., F.R.S., &c. 2. J. Prestwich, Esq., F.R.S., F.G.S., "On the Structure of the Crag-beds of Norfolk and Suffolk, with some observations on their Organic Remains.—Part II. Red Crag." 3. James Thomson, Esq., "On some Carboniferous Corals." Communicated by Dr. P. Martin Duncan, Sec. G.S., &c.
THURSDAY.—Royal Institution, 3. Prof. Grant, "On Astronomy."
—— Chemical, 8. Dr. W. J. Russell, "On Eudiometrical Analysis." Mr. W. H. Perkin, "On the Combining Powers of Carbon."
FRIDAY.—Royal Institution, 8. Prof. Odling, "On some effects of the Oxyhydrogen Flame."
SATURDAY.—Royal Institution, 3. Prof. Grant, "On Astronomy."

PATENTS.

Communicated by Mr. VAUGHAN, F.C.S., Patent Agent, 54, Chancery Lane, W.C.

GRANTS OF PROVISIONAL PROTECTION FOR SIX MONTHS.

3625. B. Engel, Lawrence Lane, Cheapside, London, "A new and improved composition for extracting ink and iron-mould from linen and woollen fabrics."—A communication from L. A. de L. d'Herlen, Boulogne-sur-Mer.—Petition recorded December 21, 1867.

176. E. Dorsett, London Street, London, "Improvements in the utilisation of coal tar and the products arising from the distillation thereof, and in the apparatus employed therein."—January 18, 1868.

504. J. A. Hogg, Montague Street, Edinburgh, "Improvements in the method of, and means for, increasing the combustion and illuminating power of gas."—February 15, 1868.

747. G. Davies, Serle Street, Lincoln's Inn, Middlesex, "Improvements in the manufacture of gas for lighting and heating."—A communication from L. C. E. Vial, Paris.

753. C. Schinz, Faubourg de Saverne, Strasbourg, France, "A process for the partial elimination of the nitrogen from the products of combustion and furnaces and apparatus connected therewith."—March 4, 1868.

785. J. Houston, jun., Glasgow, N.B., "Improvements in the manufacture of composite candles and in the machinery or apparatus employed therein."—March 6, 1868.

821. C. D. Abel, Southampton Buildings, Chancery Lane, "Improvements in the production of brown colouring matters for dyeing and printing."—A communication from J. G. A. Gros, Mulhouse, Haut Rhin, France.

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THE CHEMICAL NEWS.

Vol. XVII. No. 442.

ON SCIENCE TEACHING IN SCHOOLS.

By OSCAR BROWNING, M.A.

As the subject of the teaching of science in schools is being discussed in these columns, it may perhaps conduce to the settlement of the question if I offer a few suggestions from a practical point of view.

It will be scarcely necessary to consider the position of those who think that science should be made the basis of education. It is possible that the training of the future may take this form, which is so ably advocated by Mr. Herbert Spencer; the attempt which is being made in France to create an "enseignement secondaire special," of which the backbone is mathematics, and in which science takes precedence of literature, may offer a lesson or a warning to coming generations. But in our time the supremacy of literature is not likely to be overthrown, partly because it is probably the best training for the majority, partly because it has in its favour the prescriptions of many years, but chiefly because adequate teachers of science cannot be found. The new scheme of education in France, to which I have above referred, cannot be put into action until the Normal Schools of Cluny and Mont Masan have sent forth several generations of professors. The problem which we have to solve is, after admitting that science is ancillary and subordinate to literature in education,—to decide what science is to be taught and what means are to be used in teaching it.

There is at present great want of a recognised programme of scientific study. In founding scholarships for natural science there appears to be a great difficulty in arranging the limits and scope of the examination. The witnesses examined on this subject before the Public Schools Commission do not agree; each one asserts that his own subject is admirably fitted to be an educational instrument, but he does not attempt to define the place it should hold in a general scientific education, or the relations which it bears to its competitors. The only subject which several scientific men agree in recommending is applied mathematics. They urge the study of mechanics and hydrostatics at schools. There is scarcely a school in England where mathematics are not taught to every scholar, and there are, I should imagine, few minds which are incapable of apprehending mathematical reasoning. At Eton it is frequently found that the best classics are also the best mathematicians. There can be no reason why applied mathematics should be more difficult of apprehension than the pure science, and it would seem advisable to introduce the teaching of statics, dynamics, and hydrostatics, into all schools, treating the subjects both mathematically and experimentally, so that they may form a link between the two branches of science, and there can be no reason why these subjects should not be obligatory upon all the scholars.

After this we leave our certain ground, and are obliged to feel our way with greater caution. Various authorities have recommended the teaching at schools of botany, geology, physiology, natural history, physics (*i.e.*, heat, electricity, magnetism, &c.), and chemistry. It is obvious that if science in the whole mass is to be subordinate to literature, and if pure and applied mathematics are to be taught compulsorily to all, that the whole range of these subjects cannot be made part of the regular school course, unless they are to be taught with an unreal and delusive shallowness. We must make a selection, and the only safe principle appears to be that each of these studies is suited to a different mind, and that Nature herself indicates which is the most proper to be taught to each individual.

We cannot speak with any certainty of the processo growth in the human mind. But by careful watching we seem to arrive at the conclusion that the faculties of each mind are evolved in an order peculiar to that individual. There is nothing more capricious or inexplicable than the steps by which the full manly growth is arrived at from the size of childhood. If you take twenty boys at eleven years old, with some knowledge of their parentage, you may predict what development they will have reached at twenty-five. But you cannot indicate the steps by which this development is to be attained, or the age at which the most critical change will occur. It would seem to be the same with the mind. Experience of boys' minds tends to show me that they are not generally indolent or inactive; that if they are surrounded by healthy conditions, they are really desirous of growth and nourishment. The chief difficulty of the teacher is to discover the precise food which is required at a given time. If this is offered it is received and assimilated with the greatest ease and rapidity. The most perfect possible education would be given by supplying at the right moment the intellectual food for which the healthy mind was craving. The most astonishing results in education have been produced where very able men have given their whole thoughts to the education of very able boys. This is impossible at a public school; there must be a fixed curriculum of some kind, and this we have decided is to be literature and mathematics. But the various branches of science I have above enumerated are admirably suited to be applied as alteratives to the jaded mind, and to excite the appetite for knowledge which may have been dulled by application to the regular studies.

Sciences which possess a complex terminology are often acquired with ease by very young children. A boy whose tastes lie in that direction will learn the names of a great number of flowers and insects before he is ten years old, and in cases where this appetite for accumulating names is not directed to a useful subject it has recourse to heraldry or stamp collecting, or some similar pursuit. The existence of this power in childhood may not indicate any faults of grasping scientific truth in the mature man, but it stores the mind with facts which would be learnt far more laboriously at an advanced age. It frequently happens that a dull sluggish scholar has his mind assailable by some particular branch of science. I may mention some cases of this which have come under my notice. It would be well if schoolmasters could adopt the plan of describing their cases of education as methodically and accurately as a doctor describes his cure. In this way a mass of information might be collected which would be of the greatest service in forming a true theory of education.

A., aged 14, stupid in classics and mathematics, fond of chemistry experiments, and of mechanical contrivances. Attended a course of lectures with "Roscoe" as a textbook; answered questions on the first twelve chapters of "Roscoe;" read "Hofmann's Chemistry" with a competent teacher; begged to be allowed a chemical tutor in the holidays, and worked with him through "Conington's Analysis;" if he works well, will be fit for a foreign university in a year's time; his general intelligence has wonderfully brightened, and his love of work increased.

B., aged 14, clever in classics, fair in mathematics, generally cultivated in language and history. Studies chemistry in "Roscoe" and "Hofmann;" gives most of his time to geology in "Lyell" and "Juke," which he knows thoroughly; is well acquainted with the geology of Scotland and the Isle of Wight; spends his leisure time in arranging fossils; read "Darwin's Cruise of the Beagle" twice, and the "Origin of Species" with avidity and intelligence.

C., aged 13, very fair in classics and mathematics; has read a great number of books on entomology, which is his special study; knows "Kirby and Spence" almost by heart.

D., aged 16, very dull in the regular work; has written a good and exhaustive book on the birds of two English counties; is devoted to ornithology.

These cases are drawn from a small area, and are perfectly well known to me. In each case the study of their particular science has made them more generally bright and intelligent, but they do not show any desire to learn the sciences which lie outside the circle to which they have devoted themselves.

Now, science may be taught in three ways—1st, as a collection of facts useful to be known; 2nd, as a training of the observing and reasoning powers; and 3rd, as a means of educating those who are destined to be workers in that particular field. The first of these is not to be despised. A knowledge of leading physical facts should be a part of all education; it may help to guard against misconceptions where it does not give actual knowledge. It is best imparted by lectures; and I should believe that one or two lectures given by the very best informed men in the several subjects, would be more effectual than many lectures given by men of inferior brilliancy. A system of itinerant lectures to great schools by great men would be a powerful educational engine. Besides, it is only by these lectures that the special aptitude can be discovered or called into existence. The sight and voice of great discoverers may fertilize latent germs of kindred genius.

The second aspect of scientific study has been dwelt upon with force and eloquence by Mr. Wilson on several occasions. He has shown how the study of science may train the mind in that which ought to be the end of all teaching, accuracy of thought and accuracy of expression. But nearly all sciences will do this equally as well, and many branches of a literal and mathematical training will do it also, and no branch of science is likely to do it effectually unless the interest and attention of the learner be first excited. It is quite true that the pursuit of truth is at present a characteristic of science, but it should also be a characteristic of theology and morals, philology, and history. Teaching has by the indolence and bigotry of mankind degenerated in many cases into the mere enouncing of ready made conclusions, which are to be accepted without question by the hearer. Science still dwells upon the heights and heathers, the pure air of divine philosphy. But is there no trace of such attachment to ascertained truths in the history of science itself? Were not the discoveries of Fresnel suppressed by older savants who feared their revolutionary character, and does not M. Duruy suspend the professor of physiology whose conscience leads him to the conclusions of materialism? The introduction of science into schools may serve to emancipate our existing teaching of other subjects; but it may also find that it has entered into the house of bondage, and in any case it is not the only study which is fitted to trace and exercise the mind for the search after pure and simple truth. The calculus of reasoning which science imparts is analogous to that of mathematics, philology, logic, or metaphysics. But I question myself if it is not a coarser and less powerful engine of thought than that by which a sheet of Thucydides is translated, or a passage of the Choephoræ restored. The third end of scientific teaching is all important. It is of the utmost moment to the country that no special talent should be lost to it. Our little army of explorers, in all branches of art, science, and literature, is not so large that it can dispense with recruits, while the adaptation of the outward circumstances of existence to the requirements of the development of the inner nature is the only means of assuring an harmonising and happy life. Every boy who is capable of receiving an education by means of any branch of physical science should have the fullest opportunity and encouragement to do so. I think I have by this time indicated the precise action which I would advocate.

1. That no exact study of science should be compulsory except that which is closely connected with mathematics.

2. That lectures should be given on all branches of science by distinguished men, for the purpose of giving useful information, and stimulating the zeal of those who are likely to devote themselves to any branch of the subject.

3. That accurate teaching in chemistry, botany, geology, natural history, and the various branches of physics not included in applied mathematics, should be provided to all who show a special aptitude and desire for the study, and that such boys should be excused so much of the school curriculum as would enable them to study the subject with profit and advantage.

I must now conclude this very hasty sketch of the kind of science teaching which I think should be provided in schools. The greatest danger which we are running in England in the matter of education is of adopting a too extended curriculum which must necessarily be shallow. They are discovering in France, Germany, and Holland, that they have made this mistake. But in these countries everything is regulated by a ministerial programme, and the only method of consulting special aptitudes is by introducing a system of bifurcation, which answers as badly abroad as it does in England. But in our great English public schools, the freedom from control among the boys, and the close and affectionate relation between the tutor and the pupil, gives scope to a liberty of learning and a liberty of teaching, which we are told is the informing spirit of the German universities. To these two great principles we ought to cling. The advocates of new studies only succeed in proving that they are good engines of education. They do not convince us that we should abandon the old studies or force the new upon all. De Tocqueville left us a warning that we should not become *polytechnise's*, as Frenchmen are. The real advance in education lies in the reverent and loving watching of each human soul, the guarding it from all influences which may confine or distort its growth, and the surrounding it with all the food it asks for the support of its daily life, that by the gradual progress of individual development it may arrive at a mature character which will find the world in which it lives in accord and harmony with its aims and aspirations. If this training were adopted by us, a public school would present a rich variety of character which philosophers think that our modern society is certain soon to lose. And in obtaining this diversity, science has to play a very important if not the chief or leading part.

Eton, May, 1868.

ON THE ESTIMATION OF POTASH.

By F. T. TESCHEMACHER & J. DENHAM SMITH.

In the Chemical News of the 24th ult., No. 438, we read, well nigh with dismay, an abstract of a memoir on "*The Estimation of Potash*," by Messrs. James Chalmers and R. R. Tatlock.

For years we had pinned our faith on the truth of the results indicated by the chloride of platinum and potassium; so clean, so exact, and so unvarying were they, with the added and vast advantage that an error in the platinum salt, unless gross indeed, affected the outcome of potash but slightly; and now we are told that this faith is naught, and that we have been blundering all our days.

May we be permitted to make some cursory remarks on the memoir in question, with the view of showing wherein we agree or differ from these chemists, of describing a process, in detail, which will yield exact results, and of the issues of some few experiments, made with the object of re-assuring ourselves, after the rude shock our belief in the platinum salt had sustained, and thus defending the true faith in this matter.

We are told that "Glasgow is the destination of almost all the muriate of potash manufactured from the interesting deposit in the vicinity of Stassfürth," &c.: a statement which, if true, would be much to the advantage of Glasgow and its chemists; but, in our present sceptical state of mind, we are curious as to the source of the saltpetre used in France, Germany, and Central Europe, if Glasgow monopolises the Stassfürth muriate; and also how it happens that both France and Germany now-a-days export refined saltpetre to this country; a manufacture, especially the German nitrate, of singular excellence often containing less than one ounce of impurity to the ton of refined saltpetre.

Our authors, on the plea that "they have made thousands of potash determinations, urge that they may fairly claim to have some acquaintance with the subject;" a claim to have been admitted, had these potash determinations been correct; but as the very gist of this memoir is that all potash estimations, necessarily including these thousands of their own, have been wrong, or at the best doubtful, such acquaintance, that of erroneous practice, must needs be worse than none at all. Several remarks now follow to show that "the general tendency is to report potash too high:" that they have found "results giving a total of 100 per cent in a muriate of potash, from potassic chloride and water alone." We presume this sentence means, that the authors have seen a certificate of an analysis of a sample of muriate of potash setting forth that water and chloride of potassium were its sole constituents, and consequently "they feel warranted in saying that serious errors were made," a statement in which we heartily agree. "They admit that such remarks may seem to involve rather strong and severe strictures on experienced analysts;" and here again we are at one with them, only we should omit the qualifying words with which they dilute their judgment, and limit our adherence to its justice, by assuming that these remarks relate to the chemists of Glasgow only; subject to which provisoes we cordially concur in all they have said in this division of their memoir, as it exactly tallies with our own experience of Glasgow estimations of potash salts.

Here we fear that our agreement with the authors of this memoir ceases for a while, for we deny, *in toto*, that the errors in question "are chiefly due to an unsuspected source of error in the reagent employed," maintaining that these errors, which, beyond question, are of constant occurrence, are due to imperfect manipulation, and to this alone. We must also deny that "the purity of the platinic chloride solution, the key-stone of their process, is requisite to ensure accuracy," and as we are not by any means certain that "crime and punishment are inseparably associated," we also are latitudinarian enough to assert that "impure platinum," at least that which our authors regard as such, "and false results," have no necessary connection, and that true and accurate results depend solely on manipulation.

We presume that these gentlemen, and Glasgow chemists generally, obtain the double salt of platinum and potassium as the bright yellow powder, the characteristic test for potash, (if so here is one source of error); we have long since found it difficult to obtain reliable results from this salt in the pulverulent condition, and possibly also they have been sparing in their platinum, another fatal error; but on these points we are left in the dark, so far as the abstract in your Journal can inform us. We gladly note the excellence and value of most of the points in manipulation described under "II.," p. 200, as part of a system we hold to be indispensable to accuracy. This brings us to our second object, that of describing in detail a process for estimating potash which will yield exact results; and here, Sir, we fear that we must trespass on your space, and possibly on the patience of your readers, as we fully share the objection these Glasgow gentlemen raise to the meagre nature of the details of analytical processes furnished in many books on chemical analysis, far preferring the extended and exhaustive descriptions of H. Rose, to the concise and bald methods, interrupted by continual references, adopted by his successors.

We assume the salt under examination to be a sample of commercial muriate of potash, or that the alkalies exist in such a condition that by the addition of hydrochloric acid in excess, they will be converted into chlorides. Like Messrs. Chalmers and Tatlock, we also take 500 grains of the salt, previously carefully ground and mixed, and dissolve it in water, filtering if requisite, and washing the insoluble portion till solution and washings measure 5000 grains. Mix this solution by pouring from one glass to another, and take 500 measured grains of this solution (we must lay stress on accurate measurement, always at one level of the eye and one level of liquid and line of measurement), and dilute these 500 grains till they measure 5000 liquid grains, and mix: 1000 measured grains of this solution contain 10 grains of the original salt.

Now, to 1000 measured grains of this solution, add excess, say 50 grains of hydrochloric acid if the alkalies are not present as chlorides, and pour into a shallow porcelain dish, making with the rinsings of the measure, &c., some 1500 grains of solution. Heat the dish and contents nearly to ebullition, and add to the hot solution so much solution of chloride of platinum as is equal to 20 grains of the metal. Evaporate this mixture on water-bath nearly to dryness; that is to the point when the thick syrupy liquid, on the momentary removal of the dish from the bath, passes into an orange-coloured pasty mass. At this point remove the dish from the bath, and at the same instant and before the dish and its contents have had time to cool, drench it with, say 500 to 600 grains, of rectified methylated spirit containing about 15 per cent of water to 85 of the alcohols; mix rapidly by imparting a rotary motion to the contents of the dish; then cover it and allow it to digest for, say 5 minutes. Have not too small a filter ready—of some 400 to 500 grains capacity—in a funnel with a cover, it will facilitate filtration by washing the filter first with hot water and then with spirit, after this short digestion pour the alcoholic solution of the platinum salts on to the filter, draining the crystalline solid scales of the potassium salt as dry as possible, and again drench, agitate, and digest the insoluble salt with spirit; repeat this a third time, when the spirit will come away nearly colourless. Now collect the light orange crystalline scales of the chloride of platinum and potassium on the filter, by means of a wash-bottle, and, if need be, wash with spirit till it passes perfectly colourless. It is very advisable to wash the platino-potassic chloride by decantation and finally by a stream of spirit from a wash-bottle, to avoid the use of stirrers so as to prevent the scales being broken down, and to keep both dish and funnel lightly covered till the washing is completed.

There is now nothing more to be done than to dry and weigh the platino-potassic salt, and to ignite and weigh the filter and add this to the weight of the salt. Owing, however, to the crystalline nature of the salt thus obtained, but a slight stain adheres to the filter, so that the loss by ignition is infinitesimal and does not affect the result. As this crystalline precipitate is not hygrometric, its weight is easily and accurately determined.

We trust that this description of the process, which we have purposely made exhaustive in its details, admits of easy comprehension by those readers who may be interested in the subject, and especially by the authors whose memoir has induced us to bring this process to your notice; but for the sake of clearness we beg permission to recapitulate its leading and important points:—

I. Dilution of solutions.

II. Use of chloride of platinum in large excess, about 20 grains of metallic platinum to 10 grains of salt examined.

III. Heating of solutions, and evaporation so conducted as to obtain the potassic salt in a crystalline scale-like condition.

IV. Evaporation on water-bath to a pasty condition—no further.

V. Drenching with spirit whilst salt and dish are hot, and instantly on removal from water-bath.

VI. Washing by decantation, and avoiding breaking down of the crystalline precipitate.

In practice, the process is a rapid one, from beginning to end requiring about two hours' less time, indeed, than was frequently expended in merely washing the dense pulverulent precipitate we denounce as liable to many sources of error.

We have little more to add besides the results of the experiments we made to reassure ourselves of the thorough trustworthiness of this mode of estimating potash.

No. I. The pure German saltpetre before referred to, yielded by the foregoing process, from 1000 measured grains of solution = 10 grs. of salt, 24·20 grs. of the double salt of platinum and potassium = 10·009 of nitrate of potash, being an error, in excess, of 1-3500th of the potassium present.

No. II. 400 grs. of the same saltpetre, 100 grs. of common salt, and 6 grs. of sulphate of magnesia were dissolved in the requisite quantity of water, an imitation of an ordinary sample of the Stasfürth muriate of potash, *quoad* the potash; two separate portions of 1000 measured grains acidified with hydrochloric acid, = 8 grs. of the potash salt, yielded respectively 19·20 and 19·30 grs. of the platinum salt = 7·941 and 7·982 of the potash salt, showing in both cases a loss of 0·059 and 0·018 respectively, instead of a gain, the mean loss in potassium being 0·0148, a result, we venture to submit, close enough for any purpose.

It so happens that our chloride of platinum is the produce of years, and from repeated use of the metal over and over again it is probably pure, or nearly so, by this time; we therefore procured some spongy platinum from Messrs. Johnson and Matthey, the source of platinum which, in Messrs Chalmer's and Tatlock's hands yielded results they characterise as an "alarming state of things, showing that ordinary spongy platinum is not in a fit state for procuring pure platinic chloride."

We dissolved this spongy platinum just as it came from the makers, without any attempt at preliminary purification, and as this solution, when of equal strength to ours, was somewhat darker and redder, it probably was not quite so pure.

This solution with 1000 grs. of No. 1 = 10 grs. of nitrate potash, duly acidified with hydrochloric acid, yielded 24·10 grs. of the platinum salt = 9·860 of nitrate of potash, being a loss instead of a gain, and the worst result of our tests; whilst 1000 grs. of No. 2 solution = 8 grs. of potash salt, yielded with this "impure" solution of platinum 19·35 grs. of platinum salt = 8·003 of potash salt, a result which should satisfy the most exacting chemist.

None of these tests were made with any special care, but were treated in the usual way we adopt and have already described for estimating potash. We must add that we take 244·20 as the equivalent of the double chloride of platinum and potassium, which we think is that of the authors of the memoir.

In conclusion, we venture to submit to the judgment of yourself and the readers of your Journal, that we have proved that the estimation of potash by means of platinum may be implicitly relied on as yielding most accurate results, provided a proper and uniform mode of manipulation be adopted; and also, that the absolute purity of the platinum solution is a non-essential, that made from the usual spongy platinum of commerce answering the purpose as well as the purest.

NESSLER'S TEST FOR AMMONIA.

We have had repeated applications for information on the subject of Nessler's test for ammonia. This is frequently referred to, but always in a way that assumes a knowledge of its details. We shall perhaps best consult the wishes of a large number of the readers of the Chemical News if we condense here the best descriptions hitherto published of this test. We take the following from two papers: one published by Professor W. A. Miller, in the *Journal of the Chemical Society* for May, 1865, and the other published by Professor Frankland and Mr. Armstrong, in the *Journal of the Chemical Society* for March, 1868. In each instance the authors recommend Mr. Hadow's modification of the Nessler test. Professor Miller writes:—

"Make a concentrated solution of an ounce or more of corrosive sublimate; having dissolved 2½ ounces of potassic iodide in about 10 ounces of water, add to this the mercurial solution until the iodide of mercury ceases to be dissolved on agitation; next dissolve 6 ounces of solid hydrate of potash in its own weight of water, and add it gradually to the iodised mercurial solution, stirring whilst the mixture is being made; then dilute the liquid with distilled water till it measures one quart. When first prepared it usually has a brown colour of greater or less intensity, owing to the presence of a little ammonia; but if set aside for a day or two it becomes clear and nearly colourless; the clear liquid may then be decanted for use. For a litre of the test liquid of equal strength 62·5 grammes of potassic iodide, and 150 grammes of solid caustic potash will be required. About 50 grains (3 c.c.) of this solution are drawn off by a marked pipette and added to one half of the distillate; * if no ammonia be present the mixture remains colourless, but if ammonia be present the liquid will assume a yellowish tinge of greater or less intensity. The liquid will remain clear if the ammonia do not exceed 1-200th of a grain in the 5 ounces, or about 0·25 mgm. in 125 grammes of the distillate. The quantity of ammonia in such a case may be very accurately estimated in the following manner:—A solution of sal-ammoniac is prepared containing 3·17 grains of the salt in 10,000 grains of water (or 0·317 grammes of salt per litre), which is equivalent to 1-10,000th of a grain of ammonia (H_3N) in each grain of this solution, or 0·1 gramme in 1 litre. Suppose that a tint is obtained in the distilled liquid which experience leads the observer to estimate say at 5-10000ths of a grain; 50 grains of the standard sal-ammoniac solution are placed in a beaker similar in size to that used for the distillate under trial, then diluted with 5 ounces of distilled water previously ascertained to be free from ammonia (an impurity not unfrequently met with in the first portions of water which come over in distillation); lastly, 50 grains of the mercurial test liquor are added.

"If the tint coincides in intensity with that furnished by the distillate which has received an equal quantity of the mercurial test, the amount of ammonia may be considered to correspond with that taken in the liquid for comparison. If the distillate appear to have a deeper or a paler tint, a second approximative trial with a larger or a smaller quantity of sal-ammoniac must be made, and so on until the operator is satisfied that the tints coincide. On multiplying the number of grains of sal-ammoniac solution employed by 8, the product will give in 10,000ths of a grain the quantity of ammonia per gallon in the water under examination. Suppose that the observer estimates the amount of ammonia in the 125 c.c. on which he is operating, at 0·25 mgm., he takes 2·5 c.c. of the sal-ammoniac solution, and dilutes it with distilled water to 125 c.c.; he then adds 3 c.c. of the mercurial liquor, and compares it with the tint produced in the distillate by a

* The author is here referring to an operation described in a previous part of his paper, in which a distillate is obtained in which is present all the ammonia which is required to be estimated.

like addition of the mercurial test. If the two tints correspond multiply by 2 the number of c.c. of sal-ammoniac solution required, and the number obtained will give the proportion of ammonia per litre in tenths of a milligramme. When the quantity of ammonia exceeds the 20th of a grain per gallon, or 0·6 mgm. per litre, it is necessary to determine the amount by neutralisation."

The description given by Messrs. Frankland and Armstrong is as follows:—

"Unless the amount of ammonia obtained by distillation alone, or with sodic carbonate, be considerable (about ·01 part in 100,000 parts of water), Hadow's modification of Nessler's process is all that could be desired for its accurate determination. But if a larger proportion than this be obtained, the presence of urea [in a potable water] may be suspected, and it becomes necessary to make the Nessler ammonia test directly in the original water without the intervention of distillation. For this purpose, however, the water should be colourless, and free from calcic and magnesic carbonates. Any tint which is appreciable in a stratum 6 or 8 inches thick would obviously vitiate the result of a colour-test, whilst if calcic or magnesic carbonate be present, the addition of the Nessler solution will infallibly produce turbidity; moreover, we find that the slightest opalescence in the water, under these circumstances, is absolutely incompatible with an accurate determination. Both these difficulties might be effectually removed by adding to the water, first, a few drops either of ferric chloride or aluminic chloride in solution, and then a few drops of a solution of sodic carbonate, so as to precipitate ferric hydrate or aluminic hydrate. The precipitate completely decolourises the water, and no turbidity is caused by the subsequent addition of the Nessler solution; but, unfortunately, the precipitate carries down with it an amount of ammonia which, in the case of the ferric hydrate, sometimes amounts to one-third of the total quantity present. Remembering the beautiful blue-green tint—the natural colour of absolutely pure water—which is presented by a reservoir of water that has been softened by Clark's process, we tried upon peaty water the effect of precipitating in it calcic carbonate, and found that the decolourisation was as complete as could be desired, and that no appreciable amount of ammonia was carried down with the precipitate. The amount of calcic carbonate present in a coloured water is rarely sufficient to enable the operator to carry out this reaction with sufficient rapidity and completeness; it is therefore best in all cases to add a few drops of a concentrated solution of calcic chloride to half a litre of the water. The subsequent addition of a slight excess of sodic carbonate then produces a copious precipitate of calcic carbonate, which should be allowed to subside for half an hour before filtration. 100 c.c. of the filtrate is a convenient quantity to take for the direct Nessler determination of ammonia. To this volume of the filtrate 1 c.c. of the Nessler solution is added, and the colour observed in the usual way. [*See* Professor Miller's paper, quoted above.] By this direct process, the ammonia in fresh urine can be readily estimated; for this purpose 5 c.c. of the urine should be diluted with 95 c.c. of water free from ammonia. We have ascertained that known quantities of ammonia, added in the form of ammonic chloride to urine, can be determined with great accuracy.

"The colour observations of the Nessler determination are best made in narrow glass cylinders, of such a diameter that 100 c.c. of the water to be tested form a stratum about 7 inches deep. The depth of tint is best observed by placing these cylinders upon a sheet of white paper near a window, and looking at the *surface* of the liquid obliquely."

It would be a great advantage to all who are engaged in the analysis of waters if gentlemen who have been in the habit of employing Nessler's test, and have by experience learnt useful modifications in its application, would communicate them for insertion in our columns.

RESEARCHES ON DI-METHYL.*

By W. H. DARLING,
Dalton Scholar in the Laboratory of Owens College.

The synthesis of carbon compounds forms perhaps the most important and interesting branch of modern chemical enquiry. The most recent developments of these synthetical processes are the now well ascertained facts of the dependence of the chemical properties of the molecule, upon the position of the individual atoms of which that molecule is built up.

Any isomeric modifications of the saturated monovalent compounds containing one or two atoms of carbon can only be explained by the existence of a difference between the four combining powers of each carbon atom, whilst in the tri-carbon and higher series isomerism indicates the difference in the power of combination existing between the end and the middle carbon atoms of the chain.

From Frankland's original observations concerning the difference between the action of chlorine on the so-called di-methyl—

$$\left.\begin{matrix}CH_3\\CH_3\end{matrix}\right\}$$

obtained by the electrolysis of an alkaline acetate, and on the hydride of ethyl—

$$\left.\begin{matrix}C_2H_5\\H\end{matrix}\right\}$$

obtained from ethyl compounds, the existence of a difference in the four combining powers of a carbon atom was rendered probable.

The subsequent researches of Schorlemmer have, however, proved that only one hydrocarbon of the formula C_2H_6 exists, inasmuch as he succeeded in preparing ethyl chloride from the hydrocarbon di-methyl—

$$\left.\begin{matrix}CH_3\\CH_3\end{matrix}\right\}$$

obtained by the electrolytic decomposition of an alkaline acetate (*Proc. R. Soc.*, xiii., 225); as well as from ethyl hydride, obtained from ethyl compounds.—(*J. Chem. Soc.* N.S., ii. 262.)

It appeared of great interest to repeat this synthesis, and to prepare the chloride in larger quantity, from which to prepare ethyl compounds and ascertain their chemical and physical properties.

At the request of Mr. Schorlemmer, I undertook this investigation. I take this opportunity to express my thanks to that gentleman, and also to Professor Roscoe for the kind assistance rendered to me throughout this research.

I prepared the di-methyl by the electrolytic decomposition of acetate of potash, according to the process described by Kolbe. The gas, evolved from a platinum plate contained in a porous cell, was passed first through a solution of caustic potash, to absorb the carbonic acid, afterwards through nordhausen acid, and over pumice-stone moistened with oil of vitriol, to free it from a trace of oxide of methyl or hydrocarbon absorbable by this acid, and finally through a solution of caustic potash, to absorb acid fumes, any carbonic acid which had escaped the first wash bottle, or traces of sulphurous acid. The gas thus prepared had a very slight odour, it burnt with a non-luminous flame.

The following analysis of it is according to Bunsen's method:—

	Volume.	Pressure.	Tempt. C.	Volume at 0° C. and 1m. Pressure.
(1.) Original volume of Gas	183·48	0·1402	8·5	24·94
(2.) After addition of oxygen	333·41	0·3919	8·0	127·00
(3.) ,, ,, air	567·52	0·5221	8·5	287·5
(4.) ,, explosion	505·38	0·4591	9·2	224·7
(5.) ,, absorption of carbonic acid (dry)	447·58	0·4156	12·3	173·9

Contraction observed	62·80	calculated	62·35
Carbonic acid ,,	50·80	,,	49·83
Hydrogen in two volumes of gas found	6·076	calculated	6·000
Carbon ,, ,,	2·036	,,	2·000

* Communicated to the Man. Lit. and Phil. Soc. by Professor H. E. Roscoe, Ph. D., F.R.S.

This gas, treated with an equal volume of chlorine, was exposed to the diffused sun-light, after allowing time for the two gases to mix until nearly colourless, and completed by means of direct sun-light when quite colourless. The bottle was opened under warm water; the hydrochloric acid was absorbed equal to half the capacity of the bottle. The remainder of the gaseous contents not absorbed were displaced by warm water into a receiver, in which a few pieces of stick potash were placed, surrounded by a freezing mixture of salt and ice—a colourless, volatile liquid was condensed.* One hundred grammes of chloride were prepared by the repetition of this process. This first product was separated by distillation into two parts, one which distilled below 30° C., and the other above 30° C. On still further fractionating the first distillate, a portion was obtained boiling at 11—13° C., whose sp. gr. was 0·9253 at 0° C. Pierre found the specific gravity of ethyl chloride to be 0·9241 at the same temperature.

The chloride boiling below 30° C. gave on heating in sealed tubes with acetate of potash and glacial acetic acid to a temperature of 130°—140° C. for three or four hours, a volatile fragrant liquid, having the characteristic odour of acetic ether, which after drying over chloride of calcium and magnesia, boiled at 74·00—75·5° C. Kopp gives the boiling point of ethyl acetate at 74·3 under a pressure of 760 mm. of mercury.

In order to prepare the alcohol, the acetate was heated with crystals of baryta hydrate in sealed tubes for one or two hours, to a temperature of 120° C.; after cooling it was distilled, and the distillate treated with dry carbonate of potash until it separated into two layers, the upper one was decanted upon fused carbonate, afterwards upon anhydrous baryta, from which it was distilled, when it had assumed a light amber colour. It began to boil at 78·1°, the whole coming over before the temperature exceeded 79·0° C. Kopp gives the boiling point of ethyl alcohol prepared by fermentation at 78·4° C. under a pressure of 760 mm. mercury, and the specific gravity at 0° C. as 0·8095; calculating by means of his coefficient of expansion the sp. gr. at 6° C. would be 0·80446, whilst I found the same to be 0·80302 at the same temperature.

The alcohol thus prepared had very little odour, agreeing in this respect with the observation of Meudelejeff (*Zeitschrift Chemie*, 1865, 257), though the specific gravity is slightly higher than his at the the same temperature calculated from his coefficient of expansion, viz, 0·80123.

On submitting this liquid to combustion analysis the following numbers were obtained.

No. 1. 0·2834 grm. of liquid gave 0·5357 grm. of carbonic acid, and 0·3314 grm. of water.

No. 2. 0·5533 grm. of liquid gave 1·0481 grm. of carbonic acid, and 0·6480 grm. of water.

	Percentage No 1.	No. 2.	Calculated from the formula C_2H_6O.
C	51·56	51·65	52·17
H	12·99	13·02	13·04
O	35·45	35·33	34·00
	100·00	100·00	100·00

The numbers are not all that could be desired when compared with the calculated, this is owing to the difficulty in burning so volatile a liquid, and to the small quantities taken. Dumas and Boullay (*Ann. de Chimie*, 1827, 36, 299) state that in order to obtain agreeing results upwards of a grm. of liquid was necessary, in one combustion 1·742 grms. were used; with ether a still greater quantity was required.

* If the gas or the chlorine was not pure, being mixed with air, very little or no liquid was condensed, being carried off by the current The same was observed by Mr. Schorlemmer. This will probably account for Frankland's observation that no liquid was condensed at −18° C.

The alcohol still remaining in the carbonate of potash, and in the dilute solution was separated by distillation, this distillate was oxidised by a mixture of bichromate of potash and sulphuric acid, when the characteristic odour of aldehyde was recognised, the oxidation was continued until it had disappeared, on distilling to dryness an acid distillate was obtained, this was neutralised with pure carbonate of soda, and yielded on evaporation needle-shaped crystals of acetate of soda; the mother liquor was distilled to dryness with sulphuric acid, the distillate neutralised with pure carbonate of silver, filtered and boiled, on cooling it yielded colourless transparent flat needles, which, after drying over sulphuric acid, gave on analysis the following numbers.

No. 1. 0·4142 grm. of salt gave 0·2663 grm. of metallic silver.

No. 2. 0·5095 grm. of salt gave 0·3274 grm. of metallic silver.

No. 3. 0·3650 grm. of salt gave, after drying at 100° C. in a water bath for one hour, 0·2349 grm. of metallic silver.

No. 4. 0·2483 grm. of salt gave, after drying at 100° C. in a water bath for two days, 0·1604 grm. of metallic silver.

Estimation No. 1 gave 64·30 per cent of silver.
,, No. 2 ,, 64·25 ,,
,, No. 3 ,, 64·36 ,,
,, No. 4 ,, 64·60 ,,

Calculated from the formula of acetate of silver

$$\left.\begin{matrix} C_2H_3O \\ Ag. \end{matrix}\right\} O$$

yields 64·68 per cent of silver.

That portion of the mixed chlorides which distilled above 30 C. was fractionated, when two-thirds of the total volume distilled over between 57°—59° C. The specific gravity was found to be 1·198 at 6.5° C. Regnault found the sp. gr. to be 1·174 at 17° C. and the boiling point to be 64° C. (*Ann. Ch. Phys.* [2] lxxi. 355)* Submitted to analysis the following numbers were obtained.

No. 1. 0·5206 grm. gave 0·3691 grm of chlorine.
No. 2. 0.4491 ,, ,, 0·3186 ,, ,,
No. 3. 0·4292 ,, ,, after drying over stick potash for a week, 0·3010 grm. of chlorine.

Percentage of chlorine found by No. 1, 70·89
,, ,, ,, ,, No. 2, 70·94
,, ,, ,, ,, No. 3, 71·76

The percentage required by the formula—

$$\left.\begin{matrix} C_2H_4Cl \\ Cl \end{matrix}\right\}$$

of monochlorinated ethyl chloride is 71·73.

Hence the mono-chlorinated ethyl chloride was formed in quantity by the action of excess of chlorine on di-methyl.

Having commenced the examination of the di-methyl obtained, 1st, by Frankland's method from iodide of methyl and zinc, and 2nd, by Schützenberger's process, I hope to communicate the results to the Society.

Cabalistic Formulæ.—In a scientific contemporary published abroad, the following cabalistic equations occur in the description of some improvements which have been patented by Prof. E. N. Horsford, of Cambridge, Massachusetts. 3 Cal. Po_5 + 2 [Ho, no_5] = Cal. 2 Ho. Po_5 + 2 [Cal. no_5] and again: Cal. 2 Ho. Po_5 + 2 [Cal. no_5] + 2 [Ho. So_3] + 4 Ho = 2 [Cal. So_3, 2 Ho] + 2 [Ho. no_5] + Cal, Ho. Po_5.

* Beilstein (*Ann. Chem. Pharm.* v. 113—110) has shown that monochlorinated ethyl chloride, and chloride of ethyleden, obtained by action on aldehyde with perchloride of phosphorus, are identical, the boiling point of the latter as observed by Wurtz is 58—59° and the specific gravity as determined by Geuther is 1·189 at 4·3° C.; these numbers agree with those I found. Beilstein remarks that the higher boiling point as observed by Regnault would result from the presence of higher chlorinated products.

PROCEEDINGS OF SOCIETIES.

PHILOSOPHICAL SOCIETY OF GLASGOW.

Chemical Section, 4th May, 1868.

Alexander Whitelaw, Esq., Treasurer, in the Chair.

At this, the last meeting for the session, a paper was read by Dr. Wallace, F.R.S.E., F.C.S., analytical chemist, on "*The Animal Charcoal used in Sugar Refining.*"

Dr. Wallace stated that his communication to the Section was not to be dignified by the name of a research, but as his name had been associated with the chemistry of sugar-refining during the last ten years, he thought it likely that as a result of his observations and experiments he might be able to mention some things not contained in books. He stated briefly the mode of making animal charcoal from bones, and mentioned the principal Scottish manufacturers of the substance. All the bone char made in Glasgow, Paisley, and Greenock, in addition to a large quantity imported from France and Russia, is used in the Clyde refineries. A large amount of it is manufactured; for notwithstanding the fact that the charcoal lasts a considerable time, it has to be occasionally renewed, owing to the stock gradually dwindling down from various causes. The author calculates that the quantity of animal charcoal in actual use in the Clyde refineries is probably well nigh 5,000 tons, and that the annual renewal is probably about 1,500 tons.

The carbonisation of the bones is usually continued for twelve hours, that length of time generally giving better charcoal than when the bones are only carbonised for six hours, even though in the latter case the heat is made stronger. After the bones are charred they are crushed between rollers, and the charcoal is ready for use when the dust is removed. The quality of the char varies with the kind of bones employed, and the care observed in the charring operation. Hand-picked home-collected bones make the best charcoal. Besides these there are the shank bones from the *saladeras* of Brazil and Buenos Ayres; camp bones dug up from old battle fields, and bearing evidence of having been buried for a long time, and the charcoal from which may be easily distinguished from that prepared from home-collected bones; and there are also large shipments from Italy and Turkey, including the bones of the camel along with those of cattle, antelopes, sheep, and horses.

In the analysis of the best charcoal from home-collected bones, carbon usually occurs to the extent of about ten per cent, the remainder being the phosphates and other mineral ingredients of the natural bones. The carbon varies with the amount of grease and gelatin left in the bones after boiling, and it also varies in the different parts of the same bone,—the hard exterior containing less than the extremities and spongy interior of the bone. Charcoal as met with in commerce usually contains about ten per cent of water, as water is used to cool the char as it is drawn from the retorts.

The author then stated generally the method of conducting the analysis. It presents certain difficulties notwithstanding the apparent simplicity of the composition of bone charcoal. He instanced carbonic acid as offering some difficulty, and said that his own method of estimating it was less complicated than the one given by Fresenius, although the latter is otherwise equally good. The method used by some chemists of precipitating the phosphate of lime, and then throwing down the lime in the filtrate in the usual way, he characterised as giving entirely fallacious results. Free lime has been occasionally found by the author, but only in minute quantity. Before estimating the carbonic acid the finely pulverised charcoal should be treated with solution of carbonate of ammonia.

Chemists have long been aware of the presence of nitrogen in animal charcoal, but the custom hitherto has been to include it in the carbon. Dr. Wallace finds it to vary in amount, and to diminish in quantity as the char is used. He found it to be 1·55 per cent in a total of 8·5 of so-called carbonaceous matter in char made from home-collected bones; and in another sample made from foreign bones it was 1·08 out of 9 parts of carbonaceous matter. Two samples of moderately old charcoal gave respectively ·3 and ·55 of nitrogen, while the carbon was reported at 15 and 17, respectively. The author did not venture to say whether or not the nitrogen plays any important part in the decolourising action, although, he said, no really good decolourising agent exists that is not made from an animal substance. It is not enough that the charcoal should be porous, for wood charcoal is so, and yet it is practically useless as a decolouriser.

Traces of ammonia always exist in new char, but the amount is so minute that the author has never estimated it. Frequently, however, it exists in the form of sulphide of ammonium, a compound which greatly damages sugar run through it. Such char should be well washed and reburned before being used. The washing removes common salt, ammonia, and the sulphide of calcium which is likely to be present if the char has been overburnt. The last-mentioned substance is due to the decomposition of the sulphate of lime, and acts injuriously on the sugar. Sulphuretted hydrogen is also given off from overburnt charcoal when treated with water or acid. A sample of new char gave ·08 per cent of sulphuretted hydrogen on treating it with hydrochloric acid. Combustible gases are frequently given off by both old and new char, and sometimes they form explosive mixtures with the air of the cisterns.

Speaking of the mechanical properties of animal charcoal, Dr. Wallace said that he had long regarded the bulk occupied by the char as compared with its weight as a property of great importance. He stated that a ton of new and dry char fills a space of about 48 or 50 cubic feet, while a ton of old char may fill no more space than 40, 35, 30, or even 28 cubic feet, the apparent density of dry charcoal thus becoming sometimes nearly double what it was. But the absolute specific gravity of old and new charcoal varies but very slightly. That the charcoal rapidly diminishes in bulk while the real gravity remains practically unaltered is a point of great importance, and one to which the author thinks he was the first to direct the attention of sugar refiners. The inference is that by frequent re-heating, the particles of charcoal become smaller owing to the diminution of the pores; hence the apparent gravity of char gives a ready and certain indication of its value in sugar refining. From experiments made by Dr. Wallace, a specimen of new char lost as much of its porosity by burning in a covered crucible during eleven hours as it would have lost by re-burning about one hundred times in a sugar-house. He is of opinion that the porosity of the charcoal is diminished by a sort of agglutination of the particles of phosphate of lime during the re-heating. New charcoal will hold from 80 to 100 per cent of water, and is made perceptibly wet with 20 per cent of water, while old charcoal (two or three years in use) will only hold from 30 to 45 per cent, and is made perceptibly wet with only 5 per cent of water. These facts prove that the pores become either smaller or less numerous as the charcoal is used, and point to the conclusion that, to keep animal charcoal in the most efficient state, it should be re-burned in such a way as to lessen the porosity as little as possible.

Dr. Wallace finds that although the action of heat is the main cause, it is not the only one concerned in producing an increase in the apparent gravity of the charcoal. The proportion of carbon may increase during use from 8 or 9 per cent, to 14, 15, or even 19 per cent,—this increase being derived from the organic impurities in the sugar. The carbon obtained in the carbonisation of these impurities is deposited partly on the surface, but largely

also in the pores. This is found to be a great evil, and a great point is gained if it can be prevented. That it can be prevented is proved by the fact that in some refineries the amount of carbon does not increase, while in others it even decreases, but this is owing to mismanagement in the re-heating. When the retorts are quite tight, and the heat not excessive, the carbon necessarily increases if the precaution be not taken to wash well before re-burning, so as to remove all the organic matter absorbed from the sugar liquor. To do this, boiling water is requisite, and one of the most advanced of the Clyde sugar-refiners insists that *the charcoal should even be boiled with the water.* The author's experience accords with that of the sugar-refiner referred to.

In ordinary cane sugars there is from ½ to 1 per cent of soluble mineral matter, consisting of salts of potash, soda, lime, and magnesia; and in beet sugars there is much more—from 1½ to 3 per cent, and sometimes as much as 6 or 7 per cent. The highly soluble salts, such as the salts of potash, do not affect the charcoal, and only annoy the refiner by accumulating in the syrups; but the sulphate of lime is detrimental, owing to its comparative insolubility, and to the fact that it is readily absorbed from the sugar liquor by the charcoal. It may be removed, however, by copious washing and boiling, and it is even removed in solution by washing with weak sugar liquors. Such charcoal is sure to have its sulphate of lime increased if it be washed with water naturally containing that substance in large quantity. The author stated that he had analysed charcoal containing 2½ per cent of the salt, and that the usual amount in old char is from ½ to ¾ per cent, but that in extreme cases it may be present in Clyde charcoal to the extent of 1 per cent. The carbonate of lime present in some hard waters is also absorbed by the charcoal.

The author briefly considered the offices fulfilled by the various ingredients of the charcoal. Animal charcoal has great power of absorbing gases, colouring matters, and such mineral salts as are but slightly soluble in water. The removal of colouring matter is the chief object in view; but gummy and other extractive matters have also to be removed. Animal charcoal extracts them both with equal facility. The author finds that it readily absorbs ordinary egg albumen and gum, and he thinks that the circumstance that each of these bodies has an insoluble modification may have something to do with their absorption by the charcoal. Iron is readily removed from sugar liquors by passing them through a cistern of new char. It is the nitrogenous carbon which is the most powerful decolourising ingredient of the char, for if the char be burnt perfectly white, on the surface and without, it does not remove the slightest trace of colouring matter. This fact the author has demonstrated by actual experiment: it is with him no mere opinion.

The highly porous carbon is not the only useful ingredient, although it is essentially the decolourising agent. Carbonate of lime is also of use in neutralising the small proportion of free acid present in almost all sugars except beet; and it is still more important in neutralising the lactic and other acids formed in the weak liquors, by a process of fermentation which it is difficult to prevent. Hence charcoal deprived of its carbonate of lime is objectionable, and its use is certain to produce sour liquors and give rise to the presence of iron in the low-class sugars. As the water of Greenock and Glasgow contains only traces of carbonate of lime, the quantity of that salt naturally present in charcoal gradually lessens, until in pretty old char it is sometimes reduced to about 1½ per cent. In refineries conducted on scientific principles the amount is never suffered to fall so low. If it falls below 2½ per cent, sour liquors are sure to follow. When very hard water is used the carbonate of lime either decreases very slightly or it increases, and sometimes even to an alarming extent, as in the continental beet refineries, where the evil is a very serious one.

Dr. Wallace referred to the various methods—especially those of Beanes and Gordon—for getting rid of any excess of carbonate of lime in the charcoal; and then spoke of the inconvenience attending the use of animal charcoal in sugar-refining, owing to its oxidising influence upon the organic matters extracted by the char from the sugar, and to the alteration of the nitrogenous compounds by means of which fermentation is induced, and ultimately organic acids are formed at the expense of the sugar. These acids make the washings sour and putrid, and a necessary and evil result of that is that they decompose sulphide of calcium or sulphide of iron in the charcoal, and dissolve carbonate and sulphate of lime and oxide of iron; then, as the washings are either thrown back amongst the other products of the refinery, or mixed with a fresh lot of raw sugar, they occasionally cause an immense amount of mischief. The author said that, owing to its importance, this department of sugar refining had occupied a good deal of his attention, and he claimed to have made known to refiners the means of entirely preventing such injurious results as those referred to. His method is as follows:—While the liquor is on, the char cisterns are kept at a temperature of at least 150° Fahr., so long as the liquor is strong, to prevent fermentation. Then the water used for washing down the sugar is to run on quite boiling, and this is done with all the washings which are to be preserved. When these directions are attended to, there is no difficulty with sour washings, or with the presence of iron in the low-class sugars. The char should ultimately be washed with boiling water for 10 or 12 hours, or even boiled with it.

The author concluded with a detailed account of the modes of re-burning charcoal, and of the good and bad qualities of the different kinds of re-burners now in use, and suggested the directions in which improvements might be made.

An interesting discussion followed, and various questions were asked and answered in reference to the condition in which the nitrogen exists in the charcoal, and in reference to the use of blood and clay, soot and clay, and gluten charcoal, in sugar refining.

On the motion of the CHAIRMAN, a hearty vote of thanks was accorded to Dr. Wallace.

ROYAL DUBLIN SOCIETY.

May 4th, 1868.

DR. J. M. BARRY read a paper "*On the Physical Geography of the Southern Ocean.*" The cause of the calms, trade winds, &c., was discussed. Dr. Barry concluded his paper with some remarks on the magnetic phenomena of the Southern Hemisphere. The point of least intensity on the globe was stated to be in the middle of the South Atlantic.

Mr. WALTER G. SMITH, M.B., then read an exhaustive paper upon the subject of "*A Black Wax that was Imported from Madras in the year* 1862." (The specimen is at present in the Museum of Materia Medica, Trinity College, Dublin.) The cake consists of two distinct layers—the upper one of a jet black colour, and its recent fractured surface being somewhat conchoidal; the lower layer is browner, with a granular fracture. The cut surface exhibits a waxy lustre. The specific gravity of the upper layer is 0·985, a somewhat higher density than that of bees-wax, which averages 0·96, and the specific gravity of the thinner brown layer is 1·462. The fusing point of the black portion is about 148° F., or nearly the same as that of ordinary wax. Very little could be said as regards its natural history, but the author concluded, from reasons that he gave, that it owed its origin to a particular species of bee. No mention is made of any similar substance in any of the modern works, except

an early volume of the *Pharmaceutical Journal* (vol. xii., 1853, p. 401). There is an allusion in that book to a curious wax which had been imported from Brazil, the product of a black bee which lived under ground. It was soft and exceedingly tenacious, and of a dark mahogany colour.

The following is the *modus operandi* by which the anthor examined this wax :—

3·009 grains were exhausted with boiling rectified spirit, filtered and washed. The dark brown residue left undissolved by the alcohol was treated frequently with ether, and filtered. On the spontaneous evaporation of the ether, a brown colouring matter remained. This colouring matter was easily soluble in bisulphide of carbon, in benzol, and in chloroform, and when heated with hydrate of potash, turned black and evolved ammonia. It fused very readily, and did not appear to be acted upon by a strong alcoholic solution of potash. The black substance insoluble in the ether was digested with chloroform, which immediately formed with it an opaque, treacly fluid.

This substance also evolved ammonia when heated with caustic alkali.

The first alcoholic filtrate was concentrated to a small bulk, and cooled, to allow the cerotic acid to deposit. The precipitated acid was then filtered off. The cold alcoholic solution was evaporated to dryness, and the residual cerolein weighed. The results of the analysis are as follows :—

	Black wax.	Bees-wax.
Myricin	—	73
Cerolein	15·054	5
Cerotic acid	63·602	22
Colouring matter insoluble in ether	17·088	—
Ditto soluble in ether ..	4·253	—
	99·997	

The identity of the cerolein with that obtained from bees-wax is shown by its being acid to litmus paper, fusible at a very low temperature, soluble in ether, and quickly purified by a hot alcoholic solution of potash.

The percentage amounts of cerolein and of cerotic acid are each about three times as great as in bees-wax, but the proportion of cerotic acid varies in different samples of wax. (It was entirely wanting in a specimen of Ceylon wax, and in a wax made by wild bees in Surrey.—Brodie.)

The myricin, *i.e.*, the portion insoluble in alcohol, and which constitutes nearly three-fourths of the weight of common wax, seemed to be replaced by the two brown colouring matters.

The author does not state if the wax is capable of being practically bleached.

FOREIGN SCIENCE.

Paris, May 20, 1868.

Crystallisation of Sulphur.—Production of Paracyanogen and its transformation into Cyanogen.—Manufacture of Phosphate of Soda and Fluoride of Sodium.—A Sulphur Shower.—Academy of Sciences.—Association of the Incandescence of Magnesia to that of the Carbons in the voltaic arc.

An interesting experiment has been made by M. Schützenberger upon the crystallisation of sulphur. He filled a mattrass of a capacity of 150 or 200 grammes, with refined sulphur, commercially pure, so that when fused the liquid occupied the whole of the space below the neck; the upper part of the neck was drawn out into a capillary tube which was twisted several times but left freely open to the atmosphere. The sulphur being melted in a bath of oil heated to 120°, the flask was placed in water heated to 95°. In these conditions, the sulphur remains perfectly fluid for hours, even when occasionally moved and drawn out of the hot water. If the temperature be made to fall very slowly, transparent crystals possessing the same density as the melted sulphur form either on the surface or in the midst of the fluid at about 90°. The mass of crystals gradually augments, but with great slowness; sometimes they are isolatd, at others united in groups of two, three, four, &c. The amount of crystals being considered sufficient to separate them, the mattrass is sharply inverted, so as to cool and solidify the melted sulphur in the neck. Thus the crystals are separated from the rest of the sulphur, and only remain suspended by their peaks. They are transparent and remain so indefinitely; in form they are octahedral and bear close resemblance to natural crystals. Measurement of the angles has confirmed their identity. The experiment is surer when two or three drops of sulphide of carbon are added to the sulphur before fusion, the phenomenon takes place, however, independently of this admixture. The results are similar to those of M. Pasteur, who observed the formation in a solution of sulphur in a hydrocarbon, first of prismatic crystals, afterwards, when the temperature was sufficiently low, of octahedra. By this experiment of M. Schützenberger's, it is proved that melted sulphur crystallises below 100° in octahedra of the fourth system without the aid of any solvent. The facts will probably be turned to account in the study of the formation of natural crystals.

MM. Troost and Hautefeuille contributed a memoir at a recent meeting of the Academy, on the production of paracyanogen and its transformation into cyanogen; in undertaking the investigation they considered it interesting to examine whether the relations of cyanogen and its isomer were analogous to those of white phosphorus and amorphous phosphorus. In the present memoir they consider the transformation of cyanogen, free or in combination, into paracyanogen. An examination of the action of heat upon cyanide of mercury has shown that the proportion of paracyanogen formed is influenced equally by the temperature at which the decomposition of the cyanide is effected, and by the pressure exerted by the cyanogen upon the decomposing salt. A number of experiments were made in sealed tubes placed in various heated fluids; one indispensable precaution to be attended to in making these experiments, MM. Troost and Hautefeuille have found to be the equal heating of the tube at all points, otherwise a portion of the cyanide of mercury escapes decomposition, apparently volatilising, and condenses upon the cool portions in colourless crystals belonging to the system of the right prism with square base. This experiment confirms the observations of M. Des Cloizeaux. A table giving the results of experiments shows the advantage, in regard to the yield of paracyanogen, of the decomposition being made at a low temperature and under strong pressure.

According to Thaulow, cyanide of silver, under the influence of heat, yields half its cyanogen in the gaseous state, incandescence takes place at the same time, and the other half of the cyanogen becoming paracyanogen remains united to the silver in the form of a paracyanide. M.M. Troost and Hautefeuille find that cyanide of silver does not decompose at 350° (boiling mercury), but that the decomposition ensues at a temperature very little higher. Heated gradually to 440° and maintained at this temperature it is decomposed completely without fusion or ignition taking place. The proportion of cyanogen which, under these conditions, passes into the state of paracyanogen is about 17 per cent if made in vacuo; it attains 20 per cent under atmospheric pressure; and in sealed tubes, where the pressure is about 60 atmospheres, it may amount to 64 per cent. Cyanide of silver, slowly heated up to about 600°, under ordinary atmospheric pressure, decomposes without either fusion or ignition, while if the temperature be rapidly elevated, both fusion and ignition take place; in the two cases, more than half the cyanogen is disengaged in the gaseous state; the yield of paracyanogen never exceeds 41 per cent. Operating

at the same temperature in closed vessels, 76 per cent is obtained. The experiments of M.M. Troost and Hautefeuille exclude altogether the idea of the paracyanogen remaining combined with the silver as paracyanide; the paracyanogen is simply disseminated in the silver, and may be separated by mercury. Paracyanogen is most easily prepared from cyanide of mercury: quantities of 5 grammes are placed in strong glass tubes of about 10 centimetres capacity; the tubes being sealed, they are heated to 440° (boiling sulphur). To separate the paracyanogen from mercury, which is intermixed, the tube opened at both ends is heated again to 440° in a current of cyanogen gas. This mode of purification is preferable to the heating to dull redness with sulphuric acid.

A note on the manufacture of phosphate of soda and fluoride of sodium has been published by M. Jean. The process usually adopted in the fabrication of neutral phosphate of soda consists in attacking the tribasic phosphate of lime, either in the form of nodules or calcined bones, with sulphuric acid; thus a solution of acid phosphate of lime containing some sulphate and excess of sulphuric acid is obtained. This solution of acid phosphate of lime is treated with carbonate of soda, when phosphate of soda and carbonate of lime result, but a large quantity of sulphate of soda, or otherwise of sulphate of lime, and free sulphuric acid requires separating: repeated and carefully conducted crystallisations are therefore necessary. M. Jean made experiments with a view of founding a better process.

The following are notes of experiments:—(1.) In fusing a mixture of one equivalent of tribasic phosphate of lime, two equivalents of sulphate of soda and carbon in excess, and extracting with cold water, a strongly sulphurous liquor containing phosphate of soda was obtained. The residue, chiefly oxysulphide of calcium contained about two-thirds of the phosphate of calcium unattacked. (2.) A second experiment made with three equivalents of sulphate gave soluble acid phosphate 14 per cent, acid in an insoluble form 13·71 per cent: with three equivalents, then, about half the phosphate is obtained in the form of neutral phosphate of soda. (3.) By increasing the amount of sulphate to six equivalents, 20·6 per cent was the yield of soluble phosphate, while the residue contained 1·933 per cent of acid combined with lime. M. Jean attributes this small quantity of phosphoric acid to the action of caustic lime, always present to a slight extent in the residue, upon the phosphate of soda after lixiviation. The decomposition of the tribasic phosphate of lime into neutral phosphate of soda may then be considered complete. Unfortunately the phosphate obtained in this process is mixed with a large proportion of sulphide of sodium, rendering the separation by crystallisation very difficult, and consequently destroying the industrial value of the process. The reaction has given better results in the preparation of fluoride of sodium. A mixture of 40 parts of fluoride of calcium, 80 of sulphate of soda and carbon in excess, was fused in a metal crucible. The mass exhausted with water gave a solution containing sulphide and fluoride of sodium; only a trace of hydrofluoric acid was detected in the residue, composed as before of oxysulphide of calcium. Another fusion was made, in which carbonate of lime was added to endeavour if possible to remove the soluble sulphur compounds. The mixture was made of 100 parts of fluoride of calcium, 140 of carbonate of lime, 200 of sulphate of soda and carbon. In treating the mass with water, a limpid solution of fluoride of sodium was obtained quite free from sulphide. Traces of hydrofluoric acid were detected in the residue. By concentration and crystallisation, fluoride of sodium in great purity is obtained. Should the application of this salt at any time become important, this process will enable fluoride of sodium to be manufactured in large quantity and at a low price.

The "*Messager de Toulouse*" records an interesting phenomen which occurred at Toulouse a few evening ago, called by the people a rain of sulphur. The earth was covered with a yellow powder bearing great resemblance to pulverised sulphur. The powder was composed of the pollen of flowers borne by a strong wind from the pine forests.

The meeting of the Academy held on the 27th of April requires little space. The only two memoirs of chemical interest were, the preparation of magnesia as a refractory substance, by M. Caron, already translated in full, and the association of the incandescence of magnesia to that of the carbons in the voltaic arc, by M. Le Roux. By placing the base of a cylinder of magnesia about eight millimetres in diameter at a little distance from the carbon points, so that the arc may just touch it, the magnesia acquires a degree of incandescence comparable to that part in the bordering carbon craters. At the same time the light acquires a remarkable degree of constancy.

CORRESPONDENCE.

SCIENCE TEACHING IN SCHOOLS.

To the Editor of the Chemical News.

Sir,—I am glad to see that the important question, "How to teach Science in Schools?" is being discussed in your Journal, since your readers are qualified, not only to understand the arguments advanced, but also to give reasons of their own in justification of their views.

Perhaps it may be allowed to one who has taught science in schools during five-and-thirty years, and is now about to retire, to give his views, so far as they can be expressed within the short space you can afford him.

The teacher should, I think, start from the fact that boys are good observers, and bad theorists. They are fond of observing the habits of animals, and delight in anecdotes respecting them; they take every opportunity of witnessing and performing experiments, not forgetting those of the fifth of November; their tops, marbles, kites, and squirts, to say nothing of cricket and racket, are all experimental, and gratify their love of active observation. I don't know how it is, but as boys grow up into men, they become worse observers, and not always better reasoners. But taking it for granted that boys love experiments, and hate the explanation, we have, I think a clue to the nature of the boy's mind and the mode of operating upon it in the way of scientific teaching.

The lecturer ought, according to my view, to adhere strictly to the Baconian, or if you like, to the Comtean method of dealing only with phenomena and their laws. Let him say nothing of theory: let him avoid the causes of things, and not seek for the origin of light and electricity: let him avoid all those subtle notions connected with heat, and the convertibility of force: let him eschew molecular motions and molecular changes, and never give as a law what is only an hypothesis.

I know it is difficult to do this, because our knowledge advances by trying what we suppose to be true, by what we know to be true. This is fair work for societies, transactions and journals; but it should be carefully kept out of schools. There is a vast, but pleasant field for the earnest teacher to travel over with his pupils, if he strictly confine himself and them to the laws of phenomena.

Science is constantly engaged in solving three great problems—1st, given the phenomena to find the law; 2nd, given the law, and some of the phenomena, to find those quantities which are to serve as *data* for the prediction of other phenomena; 3rd, given the law and these data to foretell the phenomena. Now it is the first only of these problems that can ever be completely solved, and it is this only that we should introduce into schools. If this be well done, it will lead a few out of many boys to seek out at a later period the means for following up the

other two. They may even do so to some extent while at school, if the mathematical master happen to sympathise with experimental science.

The lecture should be experimental ; only the simplest numerical data being used. The leading points should be exhibited in clear writing on a large sheet of paper. The lecturer should guide his class in the difficult art of taking notes, and every boy should send up a written account of each lecture. This will not only tend to fix the teaching, but will give the boy a wholesome exercise in writing English about what he knows, instead of the ordinary subject of themes, such as "love of country," &c., of which he is ignorant.

If the written accounts are two numerous, the masters of the school may assist the lecturer in reading and correcting them. The lecturer should be deliberate and distinct in his utterance, and avoid eloquence with as much horror as a sensible man avoids fine writing. Moreover he should encourage his pupils to ask questions, and to request the repetition of such or such an experiment.

Much more might be said, but as I do not intend to trouble you with a long letter, I conclude.—I am, &c.,

C. Tomlinson.

King's College, May 16th, 1868.

MISCELLANEOUS.

Electric Light.—Some recent correspondence between the Trinity-house and the Board of Trade shows that the electric light at Dungeness can now be worked by either of the two engines, so that no disturbance occurs when one requires repair. the services of the high-class engineers and firemen have been dispensed with, and the Elder Brethren have since been enabled to do that which the connexion of the men with the trades union prevented —viz., to have their own ordinary keepers trained to drive the engines, as well as to attend to the lamps, a steady old experienced keeper being placed at the head of the establishment. The magneto-electric apparatus shown at the Paris Exhibition presented several improvements. The working by either of two machines showed that the power or the light can be duplicated in thick weather; and the engines were utilized for working the pumps of an air fog-trumpet. The electric light was compared with the flash of a first-order revolving oil apparatus belonging to the French authorities ; and at 15 miles' distance the Trinity-house engineer, Mr. Douglass, estimated the power of the fixed electric light at twice that of the flask of the oil light. The superiority of penetrating power of the electric light in fog was shaken by some experiments made by the Royal Engineers, but it turned out that this result, so different from all other experience, arose from a settlement in the wood-work supporting the electric lens, causing the lens to be out of its proper position. Since the alterations made at Dungeness the light there has worked with great regulaaity and efficency; and the Elder Brethren have proposed to place similar lights at the South Foreland, Lowestoft, and Souter Point. The Board of Trade approve the extension of this mode of illumination to the South Foreland and Lowestoft, but at present suspend their decision respecting Souter Point. The Committee of Elder Brethren who attended at the Paris Exhibition say :—"As far as the eye is an test, the power of the English fixed light was considerably in excess of the French, and when both machines were in use and there was a good current the fixed beam of the English light did not contrast unfavonrably with the revolving one of the French, the flash of which is of great power. The contrast of the electric fixed light with the French first-order oil dioptric revolving light was very marked : indeed, the one may be said to put the other out. But the most beautiful feature of the electric was the extraordinary beam it gave. It shone night after night, large, steady, and lustrous as a planet, and you could see in the darkness a beam passing as far as the eye could see. From the tower, with the light at our back, it was very marked, and quite lit the hills round Paris. The whole horizon in the plane of the light showed the white beam, and at the distance of four miles it shone upon the windows of some houses, making them appear to be lit up. By extinguishing and relighting quickly several times this was very plain. Altogether the light was very remarkable, and the committee are glad to be able to report such an advance as the powers of the light show over that at Dungeness ; indeed, the latter gives to the observer no conception of what the present one is, and it is satisfactory to know that the result of five years' work and observation, with imperfect and ill arranged apparatus, has now borne such good fruit, and that as England was the first to test and adopt this adjunct to the sources of lighthouse illumination, so she still retains her superiority. It is due, however, to Mr. Holmes to say that great as are the improvements already effected, he states thst he is confident he can yet greatly increase the illuminating power before the present apparatus is re-erected at a permanent station."

A Telegraph Feat.—At an early hour this morning (Feb. 1) the wires of the Western Union Telegraph Company from San Francisco to Plaister Cove, Cape Breton, and the wires of the New York Newfoundland, and London Telegraph Company from Plaister Cove to Heart's Content, were connected, and a brisk conversation commenced between these two continental extremes. Compliments were then passed between San Francisco and Valentia, Ireland, when the latter announced that a message was just then being received from London direct. This was said at 7.20 a.m., Valentia time, Feb. 1. At 7.21 a.m., Valentia time the London message was started from Valentia for San Fransciso ; passed through New York at 2.35 a.m., New York time ; was received in San Francisco at 11.21 p.m., San Francisco time, Jan. 31, and was at once acknowledged—the whole process occupying two minutes actual time, and the distance traversed being about 14,000 miles ! Immediately after the transmission of the message referred to, the operator at San Francisco sent an eighty-word message to Heart's Content in three minutes, which the operator at Heart's Content repeated back in two minutes and fifty seconds. Distance about 5,000 miles.—*N. Y. Journal of the Telegraph.*

Aluminium Bronze.—M. Evrard, in order to manufacture aluminium bronze, does not combine copper and aluminium directly together. He makes use of a kind of pig iron containing aluminium. This is slowly heated to fusion, when copper is added to the melted mass. Aluminium, having more affinity for copper than for iron, abandons the latter and combines with the copper. After the entire mass has been well stirred it is allowed to cool slowly, so as to permit the aluminium bronze, which is denser than iron, to find its way to the bottom of the crucible. The same process may be employed, according to the author, to obtain a bronze of silicium. Indeed, the affinity of copper for silicium is energetic enough to induce M. Evrard to try this method for separating silicium from pig iron by adding a proper quantity of copper.—*American Artizan.*

"Essay" on Coal, delivered in 1858, by an Oxford candidate to the examiners at Cheltenham :—"Coal is a black mineral. The way they produce it is this: First they dig a large pit in the earth. Then they cut down a quantity of timber and put it in the pit, and cover the whole with peat. Then they burn the timber. After it has been burnt once it becomes charcoal, and out of the charcoal they make *oxygen* gas, with which we light our streets and houses."

Alcohol from Wood.—In the process of making paper from wood, round disks of wood are first subjected to the action of hydrochloric acid to dissolve out the spongy cellulose. This latter has, until lately, been a waste product, but is now converted into alcohol in this way. The wood is boiled for twelve hours in hydrochloric acid, diluted with ten times its volume of water. The acid liquid, which is charged with grape sugar formed from the spongy cellulose, is then withdrawn, the excess of acid saturated with lime or chalk, and a small quantity of yeast is added, the temperature being kept at about 68° Fah. Fermentation soon ensues, and when bubbles of carbonic acid gas are no longer evolved, the liquid is distilled to obtain the alcohol.

Phosphorus in Iron.—The remarkable deteriorating effects of phosphorus in iron furnish a curious and striking illustration of Lord Palmerston's definition of "dirt"—*matter in the wrong place.* The *Colliery Guardian* says: "The phosphorus in Cleveland iron, which so seriously reduces its value in the market, and renders it necessary to bring iron from other districts to mix with the iron of the district in the puddling furnaces, and to use the ores of other districts to mix with its own—would, if extracted, even in its lowest priced form—as a manurial ingredient—be worth at least £66 per ton; as one ton of phosphorus is equal to 2 tons 5 cwts. of phosphoric acid, or 4 tons 10 cwts. of the highest qualities of Patagonian, or seven tons of Peruvian guano. There is therefore a tolerably good margin for working expenses if the process by which the phosphorus is extracted is carried out on tolerably economical principles. For instance, iron which is now worth 47s. per ton, when containing one per cent of phosphorus, would, if freed from this element, be worth at least as much as hæmatite iron, or say 54s. per ton."

CONTEMPORARY SCIENTIFIC PRESS.

Under this heading it is intended to give the titles of all the chemical papers which are published in the principal scientific periodicals of the Continent. Articles which are merely reprints or abstracts of papers already noticed will be omitted. Abstracts of the more important papers here announced will appear in future numbers of the "Chemical News.")

Bulletin de la Société Chemique de Paris.
December, 1867.

BOURGOUGNON, "An Improvement in Saponifying Fats." MARTIN "On the Preparation of Alizarine from the other Colouring Matters extracted from Garancine." J. PERSOZ, "A new Aniline Black Dye."

Dingler's *Polytechnisches Journal.*
January, 1868.

F. VARRENTRAPP, "On the Electro-deposition of Iron in a Solid Form." FORTMANN, "On the Theory of the Process of Roasting Iron Pyrites." R. BRIMMEYR, "On Dinitronaphthol, a new Colouring Matter extracted from Naphthalene." P. ALFRAISE, "On J. A. Schlumberger's Method of obtaining a Green Dye from Toluidine." J. VON LIEBIG, "On the Nutritive Properties of Undressed Flour and Uufermented Bread." "On the Use of Chemical Products in the Manufacture of Bread."

Bulletin de la Société d'Encouragement.
December, 1867.

E. PELIGOT, "On Sacc's Notes on the Colouring Matters in use at Neuchâtel for Dyeing Fabrics, and on the Use of Silicate of Soda for Dyeing." DUMAS, "On the Use of Tartaric Acid for Dyeing." E. PELIGOT, "On the Use of Garancine for Dyeing." THILLOY, "On a new Bitter Principle extracted from the Bark of the Hazel, to be used as a Substitute for Quinine."

NOTES AND QUERIES.

Bleaching Woollen Rags.—These are most effectually bleached by the application of sulphurous acid. Of course, in many instances, the colour of the rags, supposing the same to belong to the class of dyed, or printed goods, will be also destroyed. Chlorine cannot be used for this purpose, for two reasons—first, because it causes woollen and silk fabrics to become yellow; and, secondly, it impairs the strength of the fibre, by entering into chemical combination with the wool, silk, and other similar substances of animal origin; as, for instance, sponge, animal gut, isinglass, &c., all of which, if requiring bleaching, are bleached by sulphurous acid.—Dr. A. A.

Lepidolite.—Your correspondent can utilise his lepidolite by extracting lithium, rubidium, cæsium, and thallium from it. The following process will answer well on a large scale:—Fuse the mineral at a red heat, pour it into cold water, pulverise and wash it, treat the washed mass to twice its weight of hydrochloric acid. After several hours' boiling, separate the greater part of the silica, add nitric acid and protoxide of iron, and precipitate by carbonate of soda, the solution being made so weak that the carbonate of lithia will not be thrown down. After evaporation in this way in an iron vessel, to separate more carbonate of magnesia, saturate with hydrochloric acid and add the proper quantity of chloro-platinate of potassium to precipitate all the rubidium, cæsium, and thallium. The filtered liquid, containing an excess of platinum and lithia, is treated with sulphuretted hydrogen to separate the platinum, then concentrated and treated with the carbonate of soda to precipitate the carbonate of lithia. By this method 1000 parts of lepidolite will give 78 parts of carbonate of lithia, 6·5 parts of chloride of rubidium and cæsium, and 0·6 parts of thallium, supposing the operation to be continuous. The advantage of this process consists in the direct fusion of the mineral, and may be applied to all lithian micas.—A MANUFACTURER.

MEETINGS FOR THE WEEK.

MONDAY.—Royal Geographical, 8½.
TUESDAY.—Royal Institution, 3. Dr. M. Foster, "On the Development of Animals."
WEDNESDAY.—Society of Arts, 8.
THURSDAY.—Royal Institution, 3. Prof. Grant, "On Astronomy."
—— Royal, 8½.
—— Zoological, 8½.
—— Royal Society Club, 6.
—— Philosophical Club, 6.
FRIDAY.—Royal Institution, 8. W. E. H. Lecky, Esq., "On the Influence of Imagination upon History.
SATURDAY.—Royal Institution, 3. Prof. Grant, "On Astronomy."

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THE CHEMICAL NEWS.

Vol. XVII. No. 443.

ON THE PROPOSED WATER SUPPLY FOR THE METROPOLIS.*

By EDWARD FRANKLAND, Ph.D., F.R.S.
Professor of Chemistry, Royal Institution.

Out of every thousand people existing upon this planet at the present moment, three live in London. Any matter, therefore, which intimately concerns the health and comfort of this vast mass of humanity, cannot but merit earnest attention; and, moreover, if that matter be connected with scientific research, I feel sure that the members of this Institution will require no apology even for its being brought under their notice a second time.

A year ago, I discoursed to you about the chemical considerations respecting the present Metropolitan Water Supply, and I mentioned the five schemes then proposed to remedy its obvious and serious defects—excessive contamination with sewage, and great hardness—the first rendering it unfit for drinking, and the second disqualifying it to a certain extent for washing and cleansing purposes. Those schemes to which I alluded on the last occasion were the following: *First*, the sources of the Severn, proposed by Mr. Bateman; *second*, the Cumberland lakes, proposed by Messrs. Hemans and Hassard; *third*, the Thames water filtered through the Bagshot sands, suggested by Mr. Telford Macneill; *fourth*, extensive reservoirs constructed near the sources of the Thames, the scheme of Mr. Baily Denton; and *fifth*, the waters flowing down the slopes of the Derbyshire and Staffordshire hills, proposed to be brought to the Metropolis by Mr. Remington.

At that time the quality of the waters obtainable by any of these schemes had been but little investigated, and that remark still applies to the last three schemes. But in the interval, the water yielded by the two first-named districts has been, at the instance of the Royal Commission on Water Supply, submitted to a searching chemical investigation by Dr. Odling and myself, and I am therefore enabled on the present occasion to speak with confidence as to the quality of the water from both these districts.

There are also one or two points of general scientific interest which have been brought to light during this inquiry, and which I also propose to touch upon:—These are, first, the curious effect of detritus from mines upon the quality of the water with which it is mixed; and secondly, the conditions which determine the action or non-action of water upon lead.

During the past year the processes of water analysis have undergone a complete revolution. It is one of my duties to report monthly to the Registrar-General upon the quality of the Metropolitan waters, and in carrying out this work I found the methods of water analysis hitherto employed so untrustworthy as to render an almost entire remodelling of them absolutely necessary.

I propose, therefore, first to glance shortly at some of the innovations which have been made in this branch of chemical analysis. When water is to be submitted to chemical examination, it is of the utmost importance to have a sufficient and well-collected sample. On this occasion the completeness of the investigation, as regards the two proposed schemes, has been very materially assisted by the judicious choice of samples supplied by Dr. Pole, F.R.S., who went down to the districts and collected the samples which were afterwards submitted to chemical analysis by my colleague and myself.

The first thing to be determined in a water analysis is the "total solid impurity," as it is termed, *i.e.* the total amount of solid matter with which the water has been contaminated since it was submitted to the natural process of distillation. This quantity of solid impurity is determined by taking a known volume of the water and evaporating it down to dryness in a previously weighed platinum vessel. The solid impurity contains both organic matter and inorganic or mineral matter. The most important of these two classes of substances contained in the solid residue is undoubtedly the organic matter. Now, even at the present moment, the actual weight of this organic matter cannot be determined by chemical analysis; in fact there is no process known to science by which its weight can be even approximately estimated; but it is possible to determine, in a given bulk of water, the quantity of the two principal constituents of this organic matter, *viz.* the carbon and nitrogen which enter into its composition. For this purpose a separate quantity of the water is evaporated down to dryness; but in this case the process is conducted in a glass vessel, and before evaporation the water is mixed with sulphurous acid in order to expel the carbonic acid, which is partly dissolved in the water and partly combined with lime and magnesia. Other precautions also have to be taken, but I hesitate to enter into the details, which I fear would only weary you. However, I think that it is desirable just to show you the general plans on which the determination of the organic carbon and nitrogen, in the residue thus obtained by evaporation in the glass dish, is effected. The operation is performed in the following manner:—The contents of the glass vessel are very carefully scraped out and rubbed off the sides of the vessel by a substance known as chromate of lead, a finely powdered somewhat gritty material, which very completely effects this object, and enables us to transfer the water residue gradually into a piece of hard Bohemian glass tube closed at one end. This tube is then filled up to within about four inches of the mouth with coarsely granulated oxide of copper, and upon that is placed a small quantity of bright metallic copper to decompose oxidised compounds of nitrogen. The tube is then laid in a gas furnace, called "the combustion furnace." Before combustion commences the entire tube is made perfectly vacuous, all the air is pumped out of it, so as to get rid of the atmospheric nitrogen which would vitiate our result. This is done by means of a mercurial pump invented by Dr. Sprengel, by means of which we can extract almost the last trace of atmospheric air contained in the tube. The latter is then gradually heated to redness, during which process the carbon and nitrogen of the organic matter in the water residue are converted, the first into carbonic acid gas, and the second into nitrogen and nitric oxide gases. From the volume of each of these gases the weights of carbon and nitrogen can be calculated with great precision. (Experiment performed.)

Now the nitrogen in the result of the analysis is also derived from any ammonia present in the water, and it is therefore necessary to determine how much is due to that source. This estimation of ammonia is perhaps the only *rapid* and *easy* process connected with water analysis which may at the same time be regarded as satisfactory. For these simple processes of analysis when they come to be rigorously tested generally prove to be very incorrect; but this has survived the test of experience, and is capable of determining the result with great precision and readiness. I have here five glass cylinders. The water in the first contains no ammonia at all; the second contains a certain small quantity; the third twice as much as the second; the fourth three times as much, and the fifth four times as much as the second. To each of these vessels I shall now add an equal volume of a test solution, which strikes a peculiar yellow or orange-yellow colour with the ammonia in the vessels. This is known as the

* Read before the Royal Institution of Great Britain, Friday, April 3, 1868.

Nessler test, having been invented by a German chemist of that name. (The experiment was performed, the water in the four last vessels assuming different shades of orange colour, in proportion to the quantity of ammonia contained in them; the water in the first vessel remaining colourless.)

Now we have still one other process at which it is necessary to glance for a moment, viz.—the process for determining the nitrogen existing as nitrates and nitrites. It is called combined nitrogen, but it is not organic nitrogen, although it has in most cases been derived from organic matter. The water residue used for the determination of the amount of solid impurity is dissolved in a small quantity of water; sulphate of silver is then added, by which the chlorides are converted into sulphates. The resulting liquid, after filtration, is transferred to the upper part of a glass tube filled with mercury. It requires to be mixed with rather more than its own weight of sulphuric acid, which is introduced in the same way. It is then only necessary to shake up this mixture, the mouth of the tube being closed with the thumb. Very soon the mercury begins to act on the nitric acid, converting it into a colourless permanent gas called nitric oxide, which only requires to be measured in order to determine the amount of nitrogen originally present in the water in the shape of nitrites and nitrates.

There is only one other determination I will trouble you with, and this I do principally for the purpose of introducing to your notice a very ingenious piece of apparatus, an application of the Sprengel pump, which has just been contrived by my assistant, Mr. McLeod. It is designed to extract the gases which are dissolved in waters. By this instrument we can not only measure the whole of the gases present in the water, but we can determine how much of the gases can be expelled at the ordinary temperature, and how much more will come off when you boil the water *in vacuo*. This gas is then submitted to the usual eudiometrical investigation, to ascertain the quantity of carbonic acid, nitrogen, and oxygen—the three gases which almost invariably occur in the waters submitted to analysis. (Apparatus shown at work.)

Now it is not necessary for me on the present occasion to go at all into the details as regards the sources of the two proposed water supplies for London. This I did on the former occasion pretty fully. I will only refer you for a moment to the large map before you, which shows the districts from which the supplies would be taken and the course of the conduits to the metropolis. By the Welsh scheme, the water would be collected on the slopes of Cader Idris and Plynlimmon, from whence it would be brought by a conduit to within 10 miles of London, where it would be stored in reservoirs 400 feet above high-water mark. The other scheme proposes to bring the water from the lakes of Cumberland, past several large towns, laying under contribution the Bala Lake, in Wales, if necessary, and the combined waters would then be brought to the metropolis after distributing a certain amount to the large towns on their route.

It is, perhaps, necessary just to say a word or two in order to disabuse your minds of the idea that these schemes are intended to inflict any injury upon the present water companies. Ample provision is made in these schemes for the complete compensation of the existing companies, and the only conceivable mischief in this respect which can be done by the adoption of one scheme or the other, would be the abolition of certain Boards of Directors which now exist, for the administration of the affairs of the eight or nine companies which supply London.

These schemes are of course very costly. It quite staggers one at first to think of the amount it is proposed to expend upon them. Thus, Mr. Bateman's scheme, which is to bring water from the mountains of North Wales, is calculated to cost, for a supply of 220,000,000 gallons per day, the sum of £10,850,000; whilst the scheme for bringing water from the lakes of Cumberland is put down, for 250,000,000 gallons a day, at £13,500,000. Now these are startling figures; but I imagine that all we have to look at is the simple question, How much shall we have to pay for the water when these schemes are carried out? If you go into that matter you will find, according to the calculations of the engineers—I will not say they are always to be implicitly relied upon, perhaps a certain percentage must be allowed—but taking their calculations as correct, it actually follows that after compensating the existing companies, and after expending this enormous amount upon the works, we shall be supplied with this very pure water at a less cost than that which we pay at the present moment. We pay at present about 1s. 5d. in the pound of rent for water. By Mr. Bateman's scheme we should be charged a domestic rate of 10d. in the pound, or two-thirds of what we now pay, *plus* a public rate of 2d. Messrs. Hansard and Heman's scheme would be met by a domestic rate of 1s. 1d. in the pound. Now I think, if we are actually to be gainers by this transaction, the enormous sums necessary to be expended on these works need not frighten us, and need not prevent us from taking them into our serious consideration.

Let us just pause for a moment to consider the purely mechanical relations of the proposed to the present Metropolitan supply, because this will somewhat help you to comprehend how it is that, having expended all this money upon the works, we shall still have water cheaper. In the first place, every gallon of water which is now delivered in London has to be pumped up from nearly the sea level, to an average height of about 250 feet, Then, again, the present supply is intermittent; the proposed will be constant. With regard to the pumping part of the process, that in the proposed scheme would be replaced by the work of gravitation. The gigantic and magnificent engines employed at the present moment in London for raising this vast volume of water—100,000,000 gallons daily—are painful for the philosopher to contemplate. You have here a stupendous waste of power employed in doing over again an amount of work which was previously executed for us gratuitously. The sun, in his prodigality of power, flings up far above the cross of St. Paul's this daily supply of 100,000,000 gallons, and we, in our imbecility, allow it to soil itself by flowing down again nearly to the level of the sea, and then we erect immense pumping engines and expend 200 tons of coal daily to raise this water a fraction of the height from which we had previously allowed it to fall. All this will be saved by the proposed schemes.

We talk of the exhaustion of our coal fields and of the necessity of conserving our supply as much as possible, and although the amount thus saved would make but a poor figure in Mr. Jevons's 100,000,000 tons a year, yet this is a kind of work which can be done better by solar heat than by the action of coal; and it is not very often that we are thus able to substitute, with advantage, natural for artificial force.

Now with regard to the quality of these waters which it is proposed to bring to London, you have in the following tables a comparative statement, showing the results obtained by the analysis of the proposed Welsh and Cumberland waters, and of the present Metropolitan water supply:—

TABLE A.—*Results of Analysis of Welsh, Cumberland, and London Waters.*

100,000 PARTS OF WATER GAVE—

	Welsh. Mean.	Cumberland. Mean.	London. Mean.
Total solid impurity ..	4·85	4·74	32·66
Organic Carbon	.460	·276	·270
Organic Nitrogen	·006	·010	·025
Ammonia	·903	·002	·003
Nitrogen as Nitrates and Nitrites	·017	·009	·323
Total combined Nitrogen	·025	·021	·354
Previous sewage or manure contamination	47	6	2930

	Welsh. Mean.	Cumberland. Mean.	London. Mean.
Hardness	1·4	2·2	20·13
Lime	·599	1·113	9·822
Magnesia	·288	·272	·890
Potash	·126	·158	·851
Soda	·679	·532	1·666
Sulphuric Acid	1·093	·969	3·674
Carbonic Acid	·201	·691	7·187
Silica	·254	·133	·834
Chlorine	·876	·490	1·480

Table B.—*Analysis of London Waters*, 1867-68.

100,000 Parts of Water contained—

	Total Solid impurity. Mean.	Organic Carbon Mean.	Organic Nitrogen. Mean.	Previous Sewage Contamination. Mean.	Hardness. Mean.
Thames.					
1867	28·5	·272	·013	2062	19·3
Jan., 1868 ..	30·9	·399	·048	3150	17·3
Feb. ,, ..	31·4	.339	.043	3010	19·3
Mar. ,, ..	30·0	·216	.028	2388	19·3
River Lea.					
1867	27·5	·196	·005	1611	19·3
Jan., 1868 ..	33·1	·131	·019	3030	21·6
Feb. ,, ..	32·6	·244	·031	3320	20·5
Mar. ,, ..	28·7	·088	·016	2115	19·5
Kent Co.					
1867	39·3	2·13	·002	3619	25·6
Jan., 1868 ..	44·8	·064	·013	3770	26·2
Feb. ,, ..	59·2	·081	·013	5330	30·0
Mar. ,, ..	70·2	·093	·029	3680	32·3

The quantity of the solid impurity contained in a water is a very important matter, apart from the consideration of the quality of the substances which compose this impurity. Waters leaving a small amount of residue upon evaporation are usually well fitted for domestic use. They are invariably the best for manufacturing purposes. as they effect a great saving in heat when used for steam boilers. I was shown the other day some cakes of carbonate of lime, a quarter of an inch thick, which had been removed from a locomotive boiler at the Deptford Railway Station, in which they had been formed in forty-eight hours; through this substance heat passes with extreme slowness, so that a considerable quantity of fuel is wasted. It will be seen from the first of the above tables that, on an average of all the samples, the total solid impurities amount in the two schemes to about 1-7th of those in the present water supply; but if we might venture to take the water in the proposed large storage reservoirs as equal in this respect to the water now stored in the lakes, it would be about 1-10th of that which is found in London waters.

Now this solid residue is partly mineral and partly organic. Let us glance first at the organic portion. This organic matter present in the original water may be either living or dead. The detection of the former class of impurities belongs more to the province of the naturalist than to that of the chemist; but it may be remarked in passing, that this form of organic impurity must necessarily be in suspension and not in solution. We cannot conceive of organised beings existing in solution—it is impossible. But it does not from this follow that these suspended matters can be removed from water by filtration.

It is well known that the ova of many species of animalculæ cannot be removed thus, they pass through the best filters; and it has also been proved that what is believed to be the cholera poison passes through filters, and cannot be arrested. This is a most important consideration in connection with water which is contaminated with sewage and manure matters; and it is necessary that such water should, at all events, be as well filtered as possible. The present water companies supplying London cannot possibly be blamed for the original quality of the water which they supply. They cannot hinder the 600,000 persons who live on the banks of the Thames from pouring their refuse into the river; but they can filter this impure water. They can, and indeed by Act of Parliament they are supposed to be compelled to deliver this water in a bright, transparent, and filtered condition; and they can in this way, as far as it is possible by filtration to do it, remove these suspended organic contaminations from the water.

But how does the matter stand? Here is a sample of water which I drew from the Lambeth company's main on the 4th of March. You see that the water is not filtered. It is filtered by Act of Parliament! but it is curious to observe that so much pollution can pass through an Act of Parliament. Here too is a sample of the same company's water collected on the 21st of January; and it is a fact, that during the whole of that interval and almost up to the present time, this water has been much in the same condition. Those of my audience who are supplied by the Lambeth company, or the Southwark and Vauxhall company, or by the Chelsea company, will bear me out as to the condition in which those companies have delivered water during the past two months. In fact, not only for the past two months, but during the entire year, water is often delivered in London very imperfectly filtered. The Southwark company during the whole of last year, with one exception, delivered from its mains, when the samples were drawn for analysis, turbid water, imperfectly filtered—most of the other companies were to a less extent guilty of the same thing. Of the companies which draw from the Thames, the West Middlesex and the Grand Junction are the two which filter their water best; but the only company which delivered water uniformly transparent and well filtered was the New River company.

I have stated that the absolute quantity of the organic matter in solution in water cannot be ascertained, but the amount of carbon and nitrogen contained in this organic matter can be estimated by the process of combustion which I have exhibited to you. The amount of organic carbon and nitrogen in the several waters I have referred to, is represented in the second and third lines of table A, and in the second and third columns of table B you will see that, with regard to these elements of the organic matter in solution, there is not a very striking difference between the three different classes of waters. There is an excess of organic nitrogen in the case of the London waters, and of organic carbon in the case of the Welsh waters.

The organic matter, of which the elements are thus determined, may be either animal or vegetable, and the nature of it has much to do with the probability of its being noxious or innocuous. The animal or vegetable source of the organic matter may be judged of by the proportion of nitrogen to carbon, as determined by analysis: that from animal sources contains a larger proportion than that derived from vegetable sources; and in this way it is easy to see that the organic matter in the Welsh and Cumberland waters is of a different character from that contained in the London waters. The London river-waters, especially when turbid, contain a much larger proportion of nitrogen to carbon than is contained in other waters, thus proclaiming the animal origin of some portions of the organic matter.

When I addressed you on this subject last year I stated that by operating upon one litre of water, one per cent of unchanged sewage could be detected with certainty, but that smaller percentages ought, in operations upon such a small quantity of water, to be considered as falling within the possible errors of experiment. In like manner, by operating upon 10 litres of water 1-10th of a per cent of unchanged sewage could be detected. During the past year, however, this process of analysis has been so improved that an amount of organic nitrogen corresponding to at most 3-100ths of a per cent of unchanged sewage can now be detected with certainty in *one* litre of water. Now about 4-5ths of the organic nitrogen contained in *perfectly fresh sewage* exists there as urea, which undergoes such rapid decomposition into the mineral compound carbonate of ammonia, that little or none of it ever

reaches the Thames from the towns whose sewers debouch into this river. As average London sewage contains 10 parts of combined nitrogen in 100,000 parts, it follows that 100,000 parts of this sewage as it flows into the Thames will contain only 2 parts of organic nitrogen. Further, if the sewage of the 600,000 persons who drain into the Thames above the point whence the water companies draw their supply have the strength of average London sewage, it will amount to 18,000,000 gallons daily, and if the average flow of the river at Teddington be taken at 800,000,000 gallons daily, it follows that the river will there contain 2250 parts of sewage in 100,000 parts, or 2¼ per cent. This quantity of sewage, if in the condition as delivered at the sewer outfall, would contaminate the whole volume of the river, only to the extent of ·045 part of organic nitrogen in 100,000 parts of water. Now on the 21st of January last the water delivered by the five companies drawing their supplies from the Thames contained the following amounts of organic nitrogen in 100,000 parts:—

Chelsea (turbid) ..	·058	Grand Junction (clear)	·031
West Middlesex (clear)	·027	Lambeth (turbid) ..	·062
Southwark (turbid) ..	·061		

It will be seen, therefore, that three out of the five samples of water actually contained more organic nitrogen than would be due to the admixture of the 18,000,000 gallons of sewage which are poured into the Thames above the point from which these samples came. But Thames water holds in solution a certain amount of peaty matter which contains organic nitrogen; a sufficient proportion of this substance, however, to furnish the above larger quantities of organic nitrogen would render the water brownish yellow when viewed in a quart decanter, whilst these samples of Thames water were, when filtered, colourless or nearly so. I am therefore of opinion that the Thames water delivered in London by the Chelsea, Southwark, and Lambeth companies on the 21st of January last contained unoxidised sewage. This opinion is confirmed by the results of some experiments which I have recently made in my laboratory, and which show that, contrary to the generally received opinion (which is, however, based upon no reliable experimental data), sewage in which the urea is already decomposed undergoes further change with extreme slowness, even when freely exposed to the air and mixed with large volumes of water. Thus I find that a mixture of weak sewage from one of the London sewers with nine times its volume of water (containing bicarbonate of lime in solution) at a temperature of 20° to 25° C., and well agitated every day by being made to flow in a thin stream through 3 feet of air, oxidises but to a slight extent in the course of eight days. Immediately after mixture this sewage-contaminated water contained ·267 part of organic carbon and ·081 part of organic nitrogen in 100,000 parts, whilst after 96 hours it still contained ·250 part of organic carbon and ·058 part of organic nitrogen, and even after the lapse of 192 hours the undecomposed organic matter still contained ·200 part of organic carbon and ·054 part of organic nitrogen.

In connection with the organic matter in water, the investigation of the Welsh and Cumberland samples revealed a very curious effect produced by the admission of the detritus from lead and other mines into the waters of the streams and lakes. It was found, upon analysis, that water thus mixed with the milky streams from the crushing engines of mines, contained a wonderfully small quantity of nitrogenous organic matter. You will see this brought out in the following table:—

Effect of Detritus of Lead Mines upon the Organic Matter in Water.

	Organic Carbon in 100,000 parts of Water.	Organic Nitrogen in 100,000 parts of Water.
Cumberland Waters.		
Glenridding Beck	·116	·000
Stream flowing into Thirlmere	·066	·001
Goldrill Beck	·262	·001
Welsh Waters.		
Ceryst	·209	·000
Upper Clywedog	·544	·000
Lower Clywedog	·242	·001
Tarannon and Ceryst	·304	·001

This table shows that whilst some of these waters exhibit a rather large quantity of organic carbon, they contain very little or no organic nitrogen. And further, these waters, though they hold in solution a considerable amount of peaty matter, are perfectly colourless when seen in a quart decanter; but when viewed through a stratum 15 feet thick they exhibit the magnificent blue-green tint of absolutely pure water, a tint which is brought out when water is passed through animal charcoal. We may illustrate the action of this crushed quartz of lead mines and of animal charcoal, by three samples of the water delivered to this Institution by the Grand Junction Company, and which are contained in the tubes before you, each of which is 15 feet long. The centre tube contains the water just as it passes into the cistern, the water in the second tube has been shaken with powdered flint, whilst the water in the third tube has been passed through animal charcoal. If we now send through each tube a parallel ray of electric light, which ray will have to pass through a stratum of about 15 feet of water, you will perceive that the first gives a yellow-brown tint upon the screen; the second, a beautiful green tint, and the third, a turquoise colour: the last two powerfully reminding the observer of the lakes of Lauerz and Zug, as seen from the summit of the Rigi. (Experiment performed.) In fact this is doubtless the chief cause of that magnificent colour which we witness in many of the Swiss lakes, and which we see for instance in the Rhone when it leaves the lake of Geneva, and the Limmat as it flows from the lake of Zürich. The streams running into the heads of these lakes come in turbid and filled with finely crushed quartz and other minerals, the detritus from the glaciers which are the source of those streams. In the lakes, these fine peaty particles of mud subside and attract to themselves the colouring matter which is to be found in almost all waters.

We see in two of the English lakes some indications of this blue-green tint appearing, and it is precisely in the localities where the streams from the lead mines come down into the lakes. You see near the mouths of those milky streams which come down into Ullswater from Glenridding, and from the "Old Man," into Coniston Lake, the indications of this precipitation and removal of those brown substances which discolour the natural waters of our lakes. We have thus here, perhaps for the first time, evidence of improvement of the quality of water by the admission into it of manufacturing refuse. Hence the diversion of these waters coming from lead and other mines, which would seem at first sight to be necessary, need not be effected; on the contrary, their admission into the lakes would be of great benefit to the waters, they would to some extent decolourise them, and would tend to reduce the nitrogenous organic matter to the lowest possible amount. There appears to be no need to fear that such streams will carry anything into the lakes which will be deleterious to the drinker. All those streams have been carefully examined for lead, arsenic, copper, &c., and only in two cases has the faintest trace of lead been discovered, and the quantity was so minute that it is absolutely impossible it could be deleterious, even if the water coming from the mines themselves were to be drunk, but mixed with the large quantities of the lake water, it becomes utterly inappreciable.

The fatal effect said to be exerted upon fish by these milky streams from mines is most probably due to a mechanical action of the finely divided quartz upon their organs of respiration—an effect analogous to that (but of an exaggerated kind) from which the Sheffield grinders notoriously suffer.

(To be concluded in our next.)

ON THE

ESTIMATION OF SULPHUR IN COAL GAS.

M. Albert Ellissen, Engineer, chief of the Experimental Works of the Paris Gas Company, has forwarded the following communication to the Editor of the *Journal of Gas Lighting*, in reply to the article by Mr. Valentin, principal assistant in the Royal College of Chemistry. (*Vide* Chemical News, Feb. 21, 1868. Vol. xvii., p. 89.)

"The question of the estimation of sulphur in purified coal gas, which has for many years occupied the attention of engineers and of the public in England, should be considered from two points of view altogether different, which I shall call the theoretical and the practical views of the question.

"Looking at it theoretically, or from the scientific point of view, the object is to determine the mathematically exact quantity of sulphur in the gas. When viewing the question practically, it is necessary to ascertain the causes which obstruct the detection of that substance, and to determine the quantity which, during the combustion of the gas for domestic purposes, might produce an effect injurious to health, or be destructive to furniture, books, &c. Does the process mentioned by Mr. Valentin accomplish either of those objects? I think not, and I will endeavour to show why.

"His process, which consists in burning gas mixed with air when passing over spongy platinum, is not new, a similar arrangement having been used in 1864 by Dr. Letheby, in his experiments on the sulphide of carbon in gas. Even according to Mr. Valentin, that method does not determine the total quantity of sulphur in the gas, and I think that is less owing to the imperfect absorption of the sulphur products, or to the difficulty of extracting them from the platinum by washing, than to the imperfect combustion of the sulphur; and I have no doubt that, if Mr. Valentin were to substitute pure oxygen for atmospheric air in his experiments, he would obtain much more favourable results, and probably obtain the whole of the sulphur contained in the gas. But it appears to me that the process adopted in my experiments, afterwards repeated by Mr. Alfred Anderson and by Mr. Valentin, which consists in passing the gas over quick lime or soda lime, heated to redness in a drawn-iron tube, is more simple than that of Mr. Valentin, as it does not require the making of oxygen for special aspiration, and gives exact mathematical results of the quantity of sulphur in the gas. I cannot admit that the use of nitric acid or of hypochlorites can occasion the least difficulty to experimental chemists.

"In reply to the doubt expressed by Mr. Valentin as to the correctness of the proportions I obtained by means of lime in 1864, I wish him to bear in mind that those experiments were made at the experimental works of the Paris Gas Company, expressly on gas obtained by varied distillations of coal of the most different kinds possible; which circumstance explains the great differences in the results obtained.

"Let us look, however, to the practical part of the question, which, in point of fact, will constitute the only interesting part of the discussion that will probably take place this year, in considering any measure on the subject to be brought before Parliament. In the reports of Dr. Letheby to the Commissioners of Sewers of the City of London, on the sulphur compounds in gas, which must be first considered, he states that, during the combustion of gas, the sulphur it contains produces sulphurous and sulphuric acid, which injure furniture, and especially books in libraries. I will not again discuss what value is to be placed on that statement, for I have previously shown how many other causes contribute to the production of sulphuric acid in the atmosphere of large towns; but there can be no doubt that, if it be admitted that any damage is really done by the sulphur in gas, it can only be the portion of it which is converted by combustion into sulphurous or sulphuric acid that can really produce any injurious effect. The other portion of the sulphur, which is not consumed, and which consequently escapes into the atmosphere in the form of sulphide of carbon or vapours of sulphuretted organic products, cannot have any corrosive action. Why, therefore, should any trouble be taken to devise complicated apparatus to make it apparent? The practical problem then remains, to provide an apparatus by which all the sulphurous products during the combustion of gas, in the ordinary methods in use, can be completely ascertained; and I think that the apparatus contrived by Dr. Letheby, which is generally employed in England, fulfils all the conditions required for that kind of experiment.

"In that apparatus the gas is consumed with a Leslie's burner, under the best practical conditions for a good oxidation of the sulphur contained in the gas. The consumption with that burner is so slow, that certainly during the ordinary burning of gas so perfect a combustion does not take place, and which may be made similar to the blue flame necessary in heating apparatus—a mode of consumption which should be taken into consideration in the use of gas for domestic purposes. The condensation of the sulphur products (sulphurous and sulphuric acids) in the glass cylinder is complete; experiments having proved that a sensibly greater amount of sulphur is not obtained by increasing the number of those vessels, not by filling them with ammonia. The apparatus of Dr. Letheby has the additional advantage that the experiments may be made on the average quality of the gas manufactured during the day of twenty-four hours, and does not require the least superintendence. An interval of a few seconds is sufficient to empty the liquor collected in the cylinder, the quantity of which affords a valuable check on the correctness of the progress of the experiment, and a fresh experiment may be immediately commenced by merely opening the stop-cock for the gas.

"In a practical point of view, Mr. Valentin's apparatus is far from offering the same advantages. In the first place, the gas is consumed or decomposed under conditions not in the least like the usual manner of burning gas in private houses, and though the apparatus does not estimate the whole of the sulphur in the gas, nevertheless in these experiments a part of it is taken cognisance of which practically is not of the least interest, as it generally escapes into the air in a form that is not injurious. Furthermore, the apparatus is complicated; it requires difficult manipulation, a complicated fitting up and taking down, a continued superintendence during the whole time of the experiment, in order to supply an adequate quantity of air to the tube; and, as the experiment lasts only a few hours, the average quality of the gas made during the day cannot be ascertained. In short, I believe that this very ingenious apparatus of Mr. Valentin's is adapted exclusively to the laboratory, as it can only be used by an experimental chemist, and that it does not possess that practical working character required in a testing apparatus that is to be used rather by intelligent workmen than by men specially devoted to scientific pursuits.

"In conclusion, long consideration of the subject of the sulphur contained in purified gas has induced me to regard the simple apparatus of Dr. Letheby as the one which best fulfils the practical conditions of an instrument for testing the quantity of sulphur which, unfortunately, the progress of science has not yet succeeded in completely extracting during the manufacture of gas.

"Paris, April 15, 1868."

The Editor of the *Journal of Gas Lighting* appends the following note to this communication:—

"We are informed that Dr. Letheby made a large number of experiments, in 1854, with an arrangement on the same principle as Mr. Valentin's, the products of the burnt gas being passed with an excess of atmospheric air through a mass of very fine platinum wire, at a temperature a little below redness; and the results were so un-

satisfactory that the process was abandoned. Dr. Letheby also endeavoured to estimate the sulphur by passing the gas through a glass tube filled with small pieces of caustic baryta (obtained from the nitrate by ignition), and kept at a red heat; but as the process could not be continued for 24 hours—the time necessary for a fair analysis—this also was abandoned."

ARSENIC IN SUBNITRATE OF BISMUTH.

Dr. Gunning calls attention to the fact, that the metallic bismuth of commerce nearly always contains arsenic. He found on testing six different samples of the subnitrate of bismuth, as sold by respectable chemists at Amsterdam, that each of these samples contained arsenic; he instituted some experiments to find a ready mode of getting rid of the arsenic, and states that if even the metallic bismuth applied for the making of the subnitrate were contaminated with arsenic, the latter can be eliminated if to the nitric acid solution of the metal, just so much water is added as will suffice to produce a slight precipitate; this will contain all the arseniate of bismuth with a comparatively small proportion of subnitrate; the fluid has to be left until the precipitate has fully subsided, and the clear supernatant liquid may then be decanted, and on further addition of water the subnitrate of bismuth precipitated free from all arsenic. The *rationale* of this process is that the arseniate of bismuth is far less soluble in somewhat dilute acid, while still more than sufficient acid is present to prevent the nitrate of bismuth becoming converted into sub- or tris-nitrate, as it is called. It is true that by following this mode of manufacturing subnitrate of bismuth some loss is experienced of subnitrate; but the first sediments, if accumulated, may be boiled with a solution of caustic soda, whereby arseniate of soda is obtained, and oxide of bismuth, which latter of course can serve again for the preparation of the subnitrate with due care. In a medico-legal point of view, the fact that preparations of bismuth as applied in medicine may contain arsenic is of great importance.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, May 21.

Dr. Warren de la Rue, F.R.S., &c., President, in the Chair.

The minutes of the previous meeting were confirmed, and the names of candidates for admission into the Society were read:—For the first time, Mr. H. B. Riddell, late of the Indian Civil Service, 9, Stratton Street, Piccadilly; and Mr. Samuel Alexander Sadler (at Messrs. Chance's Chemical Works), Oldbury, Birmingham. For the second time, were read the following names:—Henry Chance, M.A., Glass Manufacturer, Birmingham; William B. Miller, assayer, Royal Mint, Sydney; and William Hustler, Mining Engineer, Rosemerryn, Falmouth.

Dr. W. J. Russell described "*Some Experiments on the Application of the Measurement of Gases to Quantitative Analysis.*" The author has made experiments for the purpose of testing the accuracy of a system of measurement, instead of weighing, in a variety of analytical operations wherein gases are evolved. The illustrations given in the paper have reference to the treatment of sodium and calcium carbonates with sulphuric or hydrochloric acid; manganic peroxide, decomposed by a mixture of oxalic and sulphuric acids; and, finally, zinc, magnesium, and other metals evolving hydrogen when undergoing solution in diluted acids. The apparatus employed consists of a eudiometer or measuring tube having attached to the top, at right angles, a flexible india-rubber tube, cemented on tightly with marine glue. This tube serves to connect it with a small flask, or bulb, in which the analytical operation is conducted, the acid being kept separate from the carbonate or metal in the usual manner (by a glass tube within), until all is ready for starting the decomposition; the gas and air measured before and after the experiment gives by difference the amount of hydrogen, carbonic acid, &c., evolved. India-rubber has been found to absorb notable quantities of the last-named gas by long contact, but the error from this and other sources proves to be very trifling, the following results having been obtained by Dr. Russell:—

CO_2 from carbonate of soda—		
Theoretical quantity	41·509	per cent.
Mean of eight experiments ..	41·533	,,
Mean of other five experiments	41·431	,,
CO_2 from calc spar—		
Theoretical quantity	44·000	per cent.
Mean of 13 experiments (HCl)	43·719	,,
,, 9 ,, (,,) ..	43·812	,,
,, 5 ,, (,,) ..	43·736	,,
,, 5 ,, (HNO_3)	43·924	,,
Manganese ore—		
I. MnO_2	58·156	per cent.
II. ,, ...	58·101	,,
Hydrogen from zinc foil—		
Theoretical amount..	3·0769	per cent.
Pure—mean of 3 experiments..	3·0757	,,
Ordinary ditto	3·0504	,,
Hydrogen from magnesium foil and ribbon—		
Theoretical amount	8·333	per cent.
Pure—one experiment	8·281	,,
Ordinary—mean of 4 experiments	8·265	,,

The President, in returning a vote of thanks to Dr. Russell, took occasion to remark that the experience of microscopists confirmed the statement relative to the permanent character of a junction made between glass and marine glue.

Dr. A. W. Williamson considered that the foregoing would be an exceedingly valuable addition to our methods of analysis, and no doubt capable of extension. As a rule, the process of measurement can be conducted with greater accuracy than by weighing; for whilst we could not weigh closer than one-tenth of a milligramme in our best balances, it was possible to measure volumes of gas within one-hundredth part of a cubic centimetre.

Mr. W. H. Perkin then read a paper, entitled "*Observations upon the Combining Powers of Carbon.*" The author addresses himself to the inquiry whether all four affinities, or combining units, of carbon are of equal value, and examines with this object the constitution of a great number of carbon compounds both simple and complex. Mr. Perkin concludes that the combining units of carbon are arranged in two pairs of very widely different chemical values, and proposes to indicate this difference in formulæ by drawing two thick lines for the stronger, and two thin lines for the weaker affinities. He deduces this conclusion from the fact that iodoform loses but two out of its three atoms of iodine when acted upon with bromine, or distilled with chloride of mercury, thus:—

$$C\begin{cases} I \\ I \\ I \\ H \end{cases} + BrBr = C\begin{cases} Br \\ Br \\ I \\ H \end{cases} + II.$$

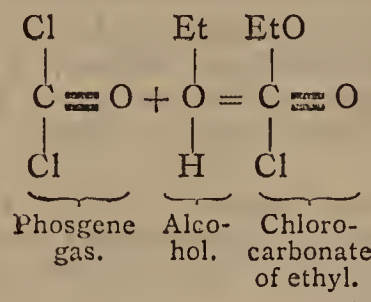
and

$$C\begin{cases} I \\ I \\ I \\ H \end{cases} + HgCl_2 = C\begin{cases} Cl \\ Cl \\ I \\ H \end{cases} + HgI_2.$$

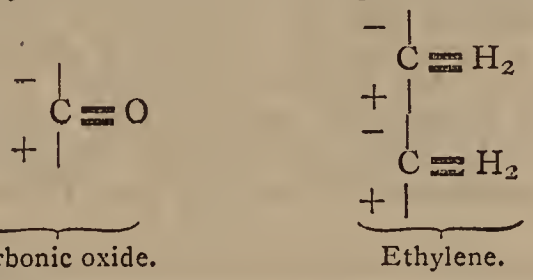
In a similar way disulphide of carbon, when attacked by chlorine, gives an intermediate product, thus :—

$$C\begin{cases}S\\S\end{cases} + Cl_6 = C\begin{cases}Cl_2\\S\end{cases} + SCl_4.$$

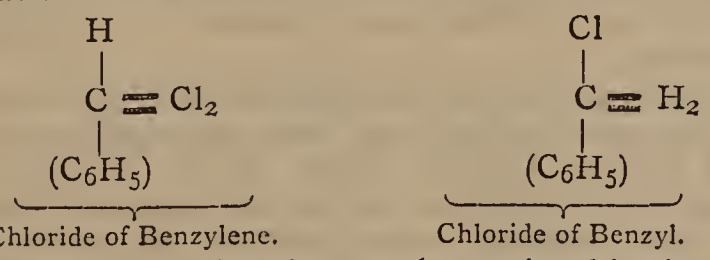
Mr. Perkin's views may be well illustrated by quoting another example, one of many described in his paper, thus :—



In the instances of carbonic oxide and ethylene, where the carbon affinities are not satisfied, the author suggests that in these bodies the stronger units are in combination with O and H_2 respectively, and the weaker pairs are neutralised by virtue of a kind of polarity, thus :—



The following experiments were shown by Mr. Perkin. Some dry oxalate of silver in a test tube was brought into contact with chloride of benzylene ; when the action had commenced, at first slowly, a delivery tube was adapted and the evolved gases collected over water were shown by appropriate tests (potash absorption and inflammation of the residue) to consist of a mixture in equal volumes of carbonic acid and carbonic oxide. A comparative experiment was likewise made for the purpose of showing the difference in the action of sodium alcohol upon chloride of benzylene, and chloride of benzyl in alcoholic solution, both being heated by immersion in boiling water ; the latter soon gave a copious deposit of chloride of sodium, whilst the contents of the other tube remained clear. The difference in the behaviour of the two bodies was explained by assuming that the chlorine was tied to the carbon by a weak bond in the chloride of benzyl, and by a strong double union, or affinity, in the case of the chloride of benzylene. Their constitution was thus sketched :—



The author concludes his paper by stating his views in the form of three propositions :—

1st. That the units of combination possessed by an atom of carbon consist of two pairs of greatly different values.

2nd. That the units of each pair are chemically of slightly different values ; and that they appear to possess some kind of polarity, thus rendering it intelligible that two units of combination in one atom may neutralise each other.

3rd. That in poly-carbon compounds the weak units of combination of one atom of carbon may unite with the strong units of a second atom.

The PRESIDENT moved a vote of thanks to Mr. Perkin, and invited an expression of opinion from Sir Robert Kane, Dr. Williamson, and others whom he saw in the room.

D. A. W. WILLIAMSON demurred in accepting the view put forward by Mr. Perkin, and considered that this was an example of the grave errors sometimes committed by chemists, when attempting to follow in the path of Kekulé without heeding the pitfalls against which that author had specially warned them at the outset. It was a mistake to assume that when one atom of hydrogen in marsh gas was replaced by chlorine, and the new body, CH_3Cl, came in turn to be attacked by chlorine, that because a greater difficulty was experienced in the second replacement a different degree of affinity must necessarily be exerted between the carbon and one or the rest of the original hydrogen atoms. The fact was that at the first stage the properties of the body were entirely changed, and we have afterwards to deal with a new system, and the effects produced in the two cases have, therefore, no assignable relation one to the other. Taking a simpler case, that of water, H O H. When attacked by sodium a hydrate was produced, H O Na, which exhibited new properties ; and when we proceeded further to replace the remaining atom of hydrogen in this hydrate it could not be said that we were at that moment dealing with water, nor could it be inferred that because one H left before the other it was originally held in weaker union with the oxygen. Professor Williamson instanced other examples of hydrogen replacement, So_2 for H_2 in the case of hydrated sulphuric acid being formed from two atoms of water ; and, then, whether one or both of the remaining hydrogen atoms were replaced by potassium, it was impossible to observe any difference, or imagine any distinction, between the two atoms of hydroxyl, or of hydrogen, in these compounds. The speaker objected to the use of the words, "atomicity" and "bond," and said that although Kekulé protested at first against a wrong use being made of these terms, his mind seemed afterwards to have become warped by constant misapplication of them on the part of others, for he had come at last to use them himself in the sense against which he formerly objected.

Dr. ODLING agreed heartily with all that Dr Williamson had said, but still felt thankful to Mr. Perkin for having brought the subject forward for discussion. The hypothesis which asserts that an atom of carbon is provided with bonds of which each has a specific function is not new, having been already described in "Watts's Dictionary of Chemistry" and discussed by Erlenmeyer and others. It seemed to be necessary to wait for facts : Dr. Williamson had alluded to the two chlorides of methyl formerly supposed to exist, and some chemists believed there were two bromides of ethyl. Referring to Mr. Perkin's illustrations, the four bonds of carbon instead of being extended at right angles would, if written in a straight line, be shown thus = C ≡ , but in attempting to explain the constitution of carbonic oxide there was nothing to show whether the oxygen atom stood on the right hand or left ; in either case there would be a hypothetical copulation of unsatisfied affinities.

Dr. HUGO MULLER disputed the inferences drawn from the asserted impossibility of converting the chloride of benzylene into a tri-chlorine compound, the fact being that this could be accomplished by the continued action of chlorine.

Mr. PERKIN, in reply, reminded his hearers that he had purposely abstained from the use of the word "bond," and employed throughout his paper the term "combining unit." Chloride of benzylene was produced by the action of pentachloride of phosphorus upon the oil of bitter almonds, and he was not aware that the chlorination of this product could be pushed further. He still wanted an explanation of certain facts ; for instance, when the vapour of bichloride of carbon is passed through a red-hot tube it gives up part of its chlorine ; but why does this happen if the combining units are of equal value ? and again, why does carbon rob carbonic acid of half its oxygen ?

Dr. ODLING said it would be easier to take a hundredweight out of the possession of a man who was loaded with two, than if he were holding but one.

Mr. JOHN PARNELL then read a short paper, and exhibited a few experiments "*On the Reducing Action of Peroxide of Hydrogen and Carbolic Acid.*" The leading

feature of this communication was a new colour-test for the detection of peroxide of hydrogen, which was shown to be much more delicate in its indications than the ordinary ethereal solution of chromic acid. The "ten-volumes preparation" of Messrs. Garden and Robbins was diluted with thirty times its bulk of distilled water, and 1-10th c.c. only taken for the experiment. This was added to a mixture previously made of aqueous carbolic acid and ferrous sulphate, when a permanent green colouration was produced in the cold. When, however, the ferrous sulphate and peroxide of hydrogen were previously mixed, only the usual violet colour, as with ferric salts, appeared on addition of the carbolic acid. The results obtained by varying the circumstances were shown at the meeting, and a somewhat remarkable reaction with the chloride of aluminium was described.

Dr. HUGO MULLER attributed the results observed to the production of oxyphenic acid, whlch is a powerful reducing agent; and the pink colour noticed in the case of chloride of aluminium may possibly have been due to the formation of rosolic acid.

A vote of thanks having been given to the author, the President adjourned the meeting until Thursday, 4th June, when Mr. Chapman will read papers "*On the Artificial formation of Pyridine;*" "*On a New Reaction for the formation of Isomeric Cyanides;*" and another short note. Dr. B. H. Paul will describe "*A mode of Testing Mineral Oils used in Lamps.*"

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, April 14th, 1868.

R. ANGUS SMITH, Ph.D., F.R.S., Vice-President, in the Chair.

Mr. WM. BROCKBANK and Mr. A. BROTHER were appointed Auditors of the Treasurers accounts.

Mr. S. BROUGHTON said that, on recently observing with high powers a group of fine spots on the sun, one of which was of considerable magnitude, it occurred to him to remove the dark glass, and by keeping the eye much beyond the focus of the heating rays, and at such a distance that the spot almost filled the apparent field of the eye-piece, to see if any phenomena could be observed different from those seen through the dark glass.

The spot was at once seen to be of a dark blood red; but thinking this perhaps might be from the strong contrast of colour, he attached a disc of plaster of Paris to the telescope, and projected the image of the spot on it. On looking at this with a common pocket magnifier, the image was observed to be a dark blood red, although the observatory was not darkened, and the disc merely shielded from the direct rays by an intervening opaque substance.

If these observations are confirmed, it will corroborate the opinion long held that the spots are not black, but appear so by contrast, and, as it would seem, from the intervention of the coloured glass.

BAVARIAN ACADEMY OF SCIENCES.

AT a meeting of the Bavarian Academy of Sciences, held on the 10th of May, the President, Baron von Liebig, delivered a lecture "*On Fermentation and the Source of Muscular Power.*" It was shown that Pasteur's celebrated discovery of the increase and propagation of the yeast fungi in a mixture of tartrate of ammonia, sugar, and yeast ashes, rested on a palpable error.

Liebig explained that, according to his analysis, the chief constituent of the yeast was a substance similar to the caseine of milk, containing nearly 1 per cent of sulphur, and recognisable when in putrefaction, even by the unprofessional, through the odour of rotten eggs.

As the materials which Pasteur employed for the growth of the yeast fungi, contained no sulphur, it follows that his assertion of the increase of the yeast fungi in the mixture of the said substances is simply an impossibility.

The proof adduced by Pasteur, viz., that the ammonia contained in his mixture disappeared and was used for the nourishment of the fungi, is characterised by Liebig as a superficial observation.

Pasteur overlooked the fact that his mixture contained soluble and insoluble phosphates, due to the yeast ash, and that on expelling the ammonia with caustic magnesia the well-known phosphate of ammonia and magnesia must be formed, and that, hence, the very means he employed to ascertain the amount of ammonia rendered the solution of this question impossible. The ammonia, then, which disappeared, had not been employed in the growth of the fungi, but simply had entered into a chemical combination whose formation Pasteur had overlooked.

It has been observed that fresh pure beer-yeast left to itself, in the presence of water, disengages carbonic acid and produces alcohol. Liebig found that the power of yeast to excite fermentation is retained as long as this process is going on; at its close putrefaction sets in.

Liebig regards this process as a vital act in the interior of the cell, and as the immediate cause of the action of yeast in the fermentation. When a solution of sugar comes into contact with the yeast cell, the inner decomposition of the latter is retarded, and the molecules of sugar in contact with the cell are decomposed.

One hundred parts by weight of yeast left to themselves furnished 9·18 per cent of alcohol. Pasteur has assumed that this alcohol is produced from the cellulose of the yeast, which had changed itself into sugar. If this assumption were true, the cellulose ought to disappear entirely: it remains, however, unaltered behind.

During the formation of alcohol no trace of ammonia is generated. As some of the most remarkable products of this vital process, Liebig mentioned leucine and tyrosine, and a nitrogenous substance containing a certain amount of sulphur.

With regard to the investigations of Fick, Wislicenus, and Frankland, which have been regarded by many as a proof against Liebig's theory of the mode in which muscular power is generated, Liebig remarked that they rest upon imperfect conceptions of the nature of the organic process involved. It was just as impossible by the combustion of dried muscle to calculate its efficiency in the living body (the assumption of these physicists) as it was by the combustion of a dried bee to estimate the work which it accomplishes in the flight of many hours, carrying the weight of its own body several miles. The muscle in the living body acts like the apparatus in a watch, which gradually expends the power stored up in it. A freshly-severed frog's leg represents an apparatus of this kind with an escapement, while the newly-removed heart of the same animal corresponds to one without escapement; the frog's heart beating for hours together, just as in the living body, while the frog's leg moves as soon as an irritant sets it for a moment free from the escapement; and if small weights are hung on them, it is possible to obtain work from a pair of severed frog's legs, that is, the weight will again and again be alternately raised to a certain height, without blood or the supply of any kind of nourishment.

FOREIGN SCIENCE.

PARIS, MAY 27, 1868.

A case in medico-legal chemistry.—Employment of sulphocyanides in toning and fixing photographs.—Derivatives of the radical silico-allyl.

A CASE in medico-legal chemistry, tried at Versailles, is worthy of some attention. Lucifer matches, it was said, had been used in poisoning the victim. The chemist stated that after a scrupulous examination of the exhumed matter (interred two years) he had failed to detect phosphorus, probably volatilised or oxidised long ago, but he had separated several pieces of melted sulphur, which he exhibited. From these facts he concluded that chemical matches must have been present, for these traces of sulphur, though very small, could not occur in culinary or pharmaceutical preparations. The question was then put—did he not know that sulphur similar to that which he had exhibited was found in deposits of fecal matter which had undergone a certain fermentation in the air? and upon this point, the finding of sulphur perfectly crystallised or in concreted masses in the old deposits in the sewer of Montfaucon, was cited; the specimens of sulphur here referred to are preserved in one of the public museums. Great doubt was thus thrown upon the source of the sulphur; indeed, judging from the chemist's evidence, he would appear to have argued farther than the experimental data justified him in doing. The prisoner was acquitted.

At a meeting of the Société de Photographie, M. Civiale made some observations upon the employment of sulphocyanides in toning and fixing. He stated that in the summer of 1867 he fixed about 700 positive proofs by means of potassium and ammonium sulphocyanides. A print, one half of which had been protected from the light, the other unprotected, and which had been exposed for three months, showed only an uniform tint.

MM. Friedel and Ladenburg communicated recently a paper, "on some derivatives of the radical silico-allyl," to the Academy of Sciences. The formation upon heating 3 molecules of silicic ether and 1 molecule of chloride of silicium, to 150°, during an hour, of a product named by MM. Friedel and Crafts, monochlorhydrine silicic-ethyl, and containing $SiCl(C_2H_5O)_3$, has already been pointed out. The same body can be obtained easily, in adding, drop by drop, 3 molecules of absolute alcohol to 1 of chloride of silicium, submitting the product to fractional distillation, and collecting that which distils at about 156°. The reaction taking place is expressed by the following equation:—

$$SiCl_4 + 3C_2H_5O,H = SiCl(C_2H_5O)_3 + HCl.$$

A very advantageous yield is thus obtained. Monochlorhydrine silicic-ethyl does not react upon zinc-ethyl, even upon boiling; but the addition to the mixture of a few fragments of sodium, with the aid of a gentle heat, commences a reaction which, if not moderated, may become very energetic. There is produced an abundant disengagement of gas; at the commencement of the reaction this gas is principally composed of chloride of ethyl, afterwards the chlorine disappears, and the gases are simply hydrocarbons (ethyl and hydride of ethyl). The sodium becomes covered with zinc in powder, and finally disappears; the liquid then contains zinc and chloride of sodium. The disengagement of gas having ceased, the operation is arrested, and the product distilled. The major part passes over, after several fractionatings, between 159 and 162° when only 1 molecule of zinc-ethyl has been used for 2 molecules of monochlorhydrine. Analysis assigns to this body the formula $SiC_2H_5(C_2H_5O)_3$, which is confirmed by the number found for the vapour density; experiment gave 6·92, while theory requires 6·65. The density has been found to be 0·9207 at 0°. Evidently in monochlorhydrine, 1 atom of chlorine has been replaced by the group C_2H_5. The body thus produced is an ethereal liquid, possessing an agreeable odour, resembling that of silicic ether. It is insoluble in water, but soluble in alcohol and ether in all proportions. Moisture transforms it gradually into alcohol, and products boiling at a higher temperature, which are doubtless, say the authors, polysilicates analogous to those which silicic ether gives. Ammonia and even alcoholic potash do not effect complete decomposition; the body partakes of the stable nature of silicium-ethyl, and is only oxidised by nitric acid above 200°. Concentrated sulphuric acid decomposes it instantaneously. With very concentrated hot potash a lively reaction is produced, and the ether is decomposed rapidly, two layers being formed, both soluble in water, which only separates some oily globules. When the solution is neutralised by hydrochloric acid, or better with chloride of ammonium, a white flocculent precipitate is produced, resembling silica. This precipitate collected on a filter and dried over sulphuric acid, burns and blackens when heated on platinum foil; it is soluble in potash and re-precipitated by hydrochloric acid. The solution feebly alkaline gives with nitrate of silver a precipitate white or yellowish soluble in ammonia, and containing a silico-carbonated acid with oxide of silver. By making a combustion of the acid in a current of oxygen, numbers were found leading to the formula $SiC_2H_5O_2H$ with a small quantity of silica; the body has not yet been obtained in a state of absolute purity. Potash has then split up the ether afresh according to the equation $SiC_2H_5(C_2H_5O)_3 + 2\ H_2O = SiC_2H_5O_2H + 3\ C_2H_6O$. The new ether has received the name of tribasic silicopropionic ether.

CORRESPONDENCE.

NAMES *VERSUS* SYMBOLS IN TEXT-BOOKS.

To the Editor of the Chemical News.

SIR,—As you have opened your columns to letters upon the difficult subject of teaching science, I beg to be allowed space for a few remarks upon the Names *versus* Symbols used in the text-books on chemistry recommended to beginners.

It may be taken for granted that what is generally understood as the binary theory of salts is the one adopted in this country to express the composition of chemical compounds. That this theory is imperfect we are all doubtless well aware, but at the same time its imperfection is of no consequence in practical teaching, since it is consented to by chemists because it is as good and useful a theory as any with which we are at present acquainted. I take the chief point in this theory to be, that the symbolic representation of all chemical compounds shall be analogous to that of common salt—NaCl.

If the above be true it is the duty of every teacher of chemistry, to endeavour by all the means at command, to suit his nomenclature and method to the theory he is about to communicate.

Are the text-books, or rather is the nomenclature they contain, in thorough accord with the theory they profess to teach?—I think not.

The reason why they are not, is, I believe, in great measure due to the fact that writers of text-books and teachers of the science have never worked in unison; have never come to any kind of agreement as to the terms best adapted to express certain facts. Each has taken his own view of the subject, and written what seemed sufficient to suit his own purpose. They have never, I think, duly considered the influence of old ideas upon persons who do not make chemistry their chief study, nor have they clearly stated the entire change which has taken place. Thus we have a mixed nomenclature, giving rise to conflicting notions upon what, whether right or wrong, should be distinctly stated.

I do not propose to lay before you anything new, neither do I pretend to unfold any panacea for our evils; I am fully alive to the difficulties of the problem I wish to solve, and am aware that many great and scientific men are, and for a long time have been, occupied with the revision of our nomenclature, and it is only my long and sad experience in teaching, which induces me to venture in bringing the subject before you.

There can of course be no objection to a chemist using any system of nomenclature in a scientific paper intended for chemists, or to the employment of any formulæ, not excepting the graphic, or even to his mixing different names and formulæ for the same substance; but in a text-book intended for beginners, we ought to confine ourselves rigidly to one, and heaven knows the labour of teaching only that one, to the generality of students.

The particular terms in our nomenclature to which I wish to draw attention, are those of acid, base, salt and radical.

Our idea of acid is borrowed from the taste of vinegar and unripe fruit; it has grown up with us from the nursery, and is an inheritance carrying with it its own lesson and associations. In chemistry, do we teach boys that all acids are sour? Certainly not. Do we mean to state that an acid is a body capable of uniting with a base to form a salt?—Certainly not! Do we mean to state that an acid is a body possessed of diametrically opposite properties to a base?—No. But many boys cling most pertinaciously to the idea, that lump sugar is the great representative of the class of bodies called bases. Do we imagine that mere words are sufficient to make a boy believe that flint is an acid, or that water is a most potent and active dibasic acid?.—We may try, but we don't succeed. The only way to convince him is, in the laboratory, to melt a carbonate, and when it is calmly fusing to add dry flint, and show him the escape of carbonic acid, and even then while admitting the fact he does not believe you, but says "you call it an acid." Again, water, which far more deserves the name of acid than flint, does not receive that appellation. It is only hinted at occasionally and indirectly in our text-books. But if H_2SO_4 is a bibasic acid, because it has two proportions of replaceable H; then H_2O is a dibasic acid for the same reason; and if H_2SO_4 is to be called dihydric sulphate on account of the two proportions of replaceable H, then H_2O is dihydric oxide for the same reason.

But who, in a laboratory, would ever dream of asking his fellow for the dihydric sulphate; or tell his pupil to add an additional proportion of distilled dihydric oxide to his solution, &c., &c.? If, then, from old association, we never apply these new names in practice, ought we not to do our utmost to save beginners from the danger of receiving wrong ideas? Now, it is granted on all hands, that to give exact principles to beginners you must use clear and definite language, thus supplying them, as far as possible, with correct means from which to form those ideas; and further, that until you get a youth to think chemistry in correct terms, you cannot expect him to produce exact answers; and even when correct terms are given, I am afraid, from past experience, it is hopeless to expect a boy to use them unless he finds others doing so too.

In our class-books we find such a term as dihydric sulphate given to the body H_2SO_4; but in the same page it is called sulphuric acid, and in a third place, oil of vitriol. So, also, authors who adopt hydric chloride as the proper name for HCl, almost invariably in the text write about hydrochloric acid. Is it fair to ask a boy in his reading to think in such terms as hydric chloride, and to be examined in them, while in his text-book, and in the laboratory, he has practically never heard it called anything but hydrochloric, or even muriatic acid. These names matter nothing to the chemist, neither do the formulæ, whichever, say of the 20 recognised ones, you choose to use for common vinegar. The chemist would most likely know what you meant, without caring much for your names or symbols; but not so the boy, or the beginner. To such a one, a name means something, and a formula is supposed by him to be a sort of representation of the composition of a body. He requires therefore, first, a formula for vinegar to be given without comment that it is wrong, or that there are 19 others, all of which, according to their authors, are more correct than the one he is learning. Further, all the recognised text-books should give the same recognised formula for the same body, leaving it to more advanced books to deal with the differences of opinion respecting them. If boys are to believe in chemistry, they must find the same names and the same facts recorded in all their text-books; and this at present is not the case. It is therefore proposed, for the consideration of all persons who have the school-teaching of chemistry at heart, whether it would not be better to exclude altogether the terms acid and base from elementary text-books.

Should you, Sir, deem these remarks worthy of a place in your valuable Journal, I shall be happy, at some future time, to consider the other terms, namely, salt and radical.

The present letter is, I fear, already too long to discuss them now.—I am, &c.,

Thomas Wood, Ph.D., F.C.S.

Laboratory, 23, Grove Street,
Lisson Grove, N.W.

SCIENCE TEACHING IN SCHOOLS.

To the Editor of the Chemical News.

Sir,—The excellent letter of Mr. Tomlinson in your Journal of May 22nd, must be my excuse for troubling you with a few lines upon the subject of Science Teaching in Schools.

I have, on the average, 150 pupils going through the courses of chemistry and physics necessary for the various competitive examinations during each six months, and as each member attends two lectures in the week, I may perhaps be allowed, after nearly seven years experience, to add my tribute to Mr. Tomlinson's assertions.

It is, in most cases, next to impossible to impart the theories for the production of heat or electricity, for example, to any except the senior divisions; and I most cordially agree with Mr. Tomlinson, "to say nothing of theory," except in special cases where the mind is well prepared by age and previous scientific training in lower classes to receive such impressions. I believe the true system to be, (1), gradually to rear the mind of the young student, commencing, for example, with the most elementary considerations of pneumatics and general properties of matters. (2). He should, as Mr. Tomlinson justly observes, be thoroughly instructed in that most difficult process of taking notes, and particularly of each experiment, and in some cases made to draw the essentials of the apparatus used. (3). A *viva voce* examination should be held upon the subject of the previous lecture; this I believe of great good, for by it you find out the weak points of which nine out of ten pupils are for some reason or other ashamed to tell you; and in junior classes it is well to invite questions after every experiment. (4). Scientific terms should be avoided as far as possible at the commencement, and even when they are used should be most thoroughly explained.

A course of elementary technology, such as our leading manufactures, illustrated by specimens and diagrams, and experiments when possible, I find of much value and greatly appreciated.—I am, &c.,

T. Bloxam.

The College, Cheltenham.

MISCELLANEOUS.

British Association for the Advancement of Science.—The meeting for the present year will commence in Norwich, on Wednesday, the 19th of August next, under the presidency of Joseph Dalton Hooker, Esq., F.R.S., D.C.L., &c., Curator of the Royal Gardens at Kew. The fact that Norwich has never before been visited by the British Association, the character of its manufactures, the highly interesting geological features and archæological remains in the surrounding district, with a hearty desire worthily to receive the Association, will combine, we trust, to make the meeting thoroughly interesting and successful. Through the liberality of various public bodies and private individuals, the Committee have obtained excellent accommodation for the various meetings of the Association; a subscription has been raised to defray local expenses; the offers of private hospitality have been numerous; special invitations have been given to the corresponding members, and a large number of distinguished foreigners; and every effort will be made to receive and heartily entertain visitors to the meeting. The Reception Room, at the Masonic Hall, Theatre Street, Norwich, will be open on Monday, August 17, at 12 o'clock, for the sale of tickets, and for supplying information to visitors. The local secretaries are—Donald Dalrymple, Esq., Rev. Hinds Howell, and the Rev. Joseph Crompton. Communications intended for presentation to the sections are expected to be forwarded before August 15th, addressed either to G. Griffith, Esq., Assistant General Secretary, 1, Woodside, Harrow, or to one of the local secretaries at Norwich, and to be accompanied by a statement whether the author will be present, and on what day, so that the business of the Association may be satisfactorily arranged. The *Opening Address* will be delivered in the Drill Hall, on Wednesday evening, the 19th of August, at eight o'clock, by Joseph Hooker, Esq., F.R.S., D.C.L., &c., President Elect. *Soirées* will be held in St. Andrew's Hall, on the evenings of Thursday, the 20th, and Tuesday, the 25th of August. *Evening Lectures* will be delivered in the Drill Hall, on Friday, the 21st, and Monday, the 24th of August, at half-past eight o'clock. Various *Excursions* (geological, archæological, and ethnological) have been arranged to take place on Thursday, the 27th of August, to Cromer and its district, to Hunstanton, to Holkham, Castle Acre, Diss, Hoxne, Thetford, &c., full details of which, with times of trains, will be published in due course. Minor excursions, within a short distance of Norwich, are in course of arrangement, and will be notified at the time of meeting. A post office will be opened at the Reception Room, Masonic Hall, Theatre Street, for the convenience of members, and to which members may have their letters directed. The Great Eastern, Great Northern, Great Western, North Western, and Midland Railway Companies will convey members, on the production of a pass ticket, at a fare and a half for the return journey. Trains between Norwich, Yarmouth, and Lowestoft will run frequently during each day.

Increase in the Quantity of Carbonic Acid met with in Air, narrow streets, and lanes.—Professor Dr. Gunning records a series of experiments concerning the quantity of carbonic acid gas contained in the atmosphere of the City of Amsterdam. While the average of carbonic acid gas found in a most densely populous part of that city was only 4·13 vol. in 10,000 vols. of air, it appeared that the air in the thoroughfare called *Halsteeg*, and taken at 3 meters height above the pavement, contained from 4·9 to 5·4 vol. of carbonic acid gas in 10,000 vol. of air. The width of the thoroughfare alluded to is about the same as that of Birchin Lane, Lombard Street, E. C. Dr. Gunning advocates the proposed widening of the above-named thoroughfare as necessary, also, on sanitary grounds.

Production of the Precious Metals.—Mr. J. Ross Brown, in his report to the Secretary of the United States' Treasury on the mineral resources of the States and territories west of the Rocky Mountains, says that a great increase in the production of both gold and silver is probable. In California, Australia, and Siberia gold mining is now conducted under many disadvantages. In the two former, wages and interest are exceptionally high, and in all there is a lack of that thorough knowledge and of those economical modes of working which can only be adopted by a generation educated to the business and devoted to it as a life-long occupation. In Spain and Brazil, which were once very rich in gold, and would probably pay for hydraulic washing, there must be numerous quartz veins that are now untouched. These will be made productive. The Andes and the Altai will be explored with care, and hundreds of veins as rich and large as those of Potosi and Guanajuato will be found. Machinery will be improved, so that tunnels or adits large enough for wagons can be bored five, ten, or twenty miles long, through high mountains, so as to pay for purposes of travel, and at the same time any lodes that may exist in the chain will be opened to a depth far below anything known in mining. The great lodes of the future will not be discovered by such antecedents as those which revealed Potosi, Cerro Pasco, Sombrette, Chanarcillo, and the best mines of Catorce. If veins like those could be found by chance, what will not the well-directed explorations of the future find? It is scarcely to be doubted that a large tunnel, commenced 1,500 feet above the sea level, on the western slope of the Sierra Nevada, at any point between 36° and 40°, would in the course of ten miles run through a multitude of rich lodes. We have reason to believe that when the great mountains were formed numerous large fissures, running in some places for hundreds of miles, were filled with auriferous and argentiferous quartz; and we fail to find them not because they are not there, but because they are covered with earth, and because the clambering hunter, the benighted wanderer, or the charcoal-burner, does not pull up the bush or does not light the fire in the right spot. A tunnel running through the Andes, commencing near Lima or Santiago, would reveal wonders; and the progress of mechanical industry is so marvellous that we are justified in hoping, if not in expecting, to see immense tunnels, fifteen or twenty miles long, cut through high mountain ranges.

Chlorous Anhydride and Benzol.—L. Carius. The formation of trichlorphenomalic acid is accompanied by the production of chlorbenzol and dichlorquinone, more especially if an excess of chlorous acid has been used (*Ann. Chem. Pharm.*, cxlii., 129). The author thinks that this is due to a reaction between chlorous anhydride and benzol, which he represents in the following two equations—

$$(1.)\ \mathrm{C_6H_6} + \mathrm{Cl_2O_3} = \mathrm{C_6H_4Cl_2O_2} + \mathrm{OH_2}.$$

$$(2.)\ (\mathrm{C_6H_6})_2 + (\mathrm{C_6H_4Cl_2O_2})_2 + \mathrm{Cl_2O_3} = (\mathrm{C_6H_5Cl})_2 + (\mathrm{C_6H_2Cl_2O_2})_2 + (\mathrm{OH_2})_3.$$

As regards the properties of dichlorquinone the author confirms most of Städeler's statements, but finds that the compounds obtained from it by the action of potassic or baric hydrate are not the salts of an acid similar to dichlorquinonic acid, but compounds of dichlorhydroquinone, $\mathrm{C_6H_4Cl_2O_2}$.—(*Ann. Chem. Pharm.*, cxliii., 315).

Obituary.—Professor Page.—Professor Charles G. Page, of the United States Patent Office, died on the 5th inst., after a lingering illness. He was the author of many important discoveries in electro-magnetism, and the inventor of an electro-magnetic motive-power engine, which attracted some attention from fifteen to twenty years ago. Prof. Page had for many years been chief examiner in the U. S. Patent Office, of the class termed "philosophical instruments," embracing all inventions in mathematics, mensuration and electricity.

PATENTS.

Communicated by Mr. VAUGHAN, F.C.S., Patent Agent, 54, Chancery Lane, W.C.

GRANTS OF PROVISIONAL PROTECTION FOR SIX MONTHS.

830. C. Attwood, Wolsingham, Durham, "Improvements in the production or manufacture of steel and iron of a steely character."—March 10, 1868.

835. F. Winser, Manchester, and J. Swindells, Kegworth, Leicestershire, "Improvements in the utilisation of the waste products arising from Esparto grass and other fibrous materials used in the manufacture of paper and other purposes.

836. F. Winser, Manchester, and J. Swindells, Kegworth, Leicestershire, "Improvements in the manufacture of sulphate of magnesia."—March 21, 1868.

855. B. Britten, Red Hill, Surrey, "Improvements in the manufacture of manure."—March 12, 1868.

861. M. Rowand, Lewisham, Kent, "Improvements in the treatment or preparation of the ingredients employed in the manufacture of fermented liquors."—March 13, 1868.

884. H. F. Griffiths, and A. Beard, jun., "Improvements in apparatus for puddling iron and steel."

891. W. E. Newton, Chancery Lane, "Improvements in the manufacture of illuminating gas in the distillation of hydrocarbons, and the making of gaseous fuel for heating purposes."—A communication from L. Stevens, Washington, Columbia, U.S.A.—March 16th, 1868.

901. W. E. Gedge, Wellington Street, Strand, Middlesex, "Improved fuel, by the use of which the kindling of fires will be greatly facilitated."—A communication from R. N. Legendre, Passage des Petites Ecuries, Paris.

909. W. E. Newton, Chancery Lane, "Improvements in producing steel and cast steel, and in furnaces or apparatus used therein."—A communication from A. Thoma, New York, U.S.A.

910. W. E. Newton, Chancery Lane, "Improvements in the process of preparing iron ore for smelting, and in furnaces therefor."—A communication from A. Thoma, New York, U.S.A.—March 17, 1868.

923. B. E. R. Newlands, F.C.S., Malvern Terrace, Charlton, Kent, "Improvements in treating and obtaining products from spent oxide of iron which has been used in gas works or otherwise, to separate sulphur from sulphuretted hydrogen, and in the purification of sulphur obtained from such oxide of iron."—March 18, 1868.

942. L. Encausse, Madrid, Spain, "Improvements in the application of remedial agents to the human frame, and in the apparatus employed therein."—March 19, 1868.

946. J. G. Tatters, W. Keeble, and B. Newbery, Plymouth, Devonshire, "Improvements in the preparation of cigars, and mode of lighting the same."—March 20, 1868.

NOTES AND QUERIES.

Extract of Madder.—In answer to the query of your correspondent, "W.B.," I beg to state that the extract of madder most in use now among the calico printers is that of M. Pernod, of Paris. Perhaps if he were to insert his query as an advertisement in a number or two of your Journal, he might be able to find some one who would be willing to communicate the process for a trifle.—R. S. DALE, B.A., Cornbrook Chemical Works, Manchester.

Aniline Green.—Cathartic Acid.—I make bold to trouble you with these queries:—1st. Iodine green for dyeing: what is it, and how applied? 2nd. Cathartic acid. It has been stated that senna owes its purgative properties to cathartic acid; if there have been any confirmatory experiments on the subject, might I trouble you for the process for the extraction of this acid?—TEST TUBE.

Vessel for Holding Nitric Acid.—Could any of your correspondents tell me of anything in which 1,200 gallons or so of dilute nitric acid (1 of strong acid, by measure, to 20 of water), can be boiled, the heat being applied by steam inside?—R. C. M.

Nitrate of Iron.—Your correspondent, "Constant Reader," desires a cheap mode of preparing nitrate of iron; he does not state, however, whether he requires the proto-nitrate or the per-nitrate, the former and latter both are applied in dyeing and calico printing. The proto-nitrate is best procured by so-called double decomposition, *i.e.*, by treating a solution of sulphate of protoxide of iron with the nitrate either of baryta, lime, or lead, the first-named salt being preferred on account of the great insolubility of the resulting sulphate of baryta; this salt, viz., nitrate of protoxide of iron, can be made also by treating iron filings with very dilute nitric acid, but the application of this process always gives rise also to the formation of ammonia, and consequently to nitrate of ammonia; moreover, it is almost impossible to evaporate the solution without causing the proto-nitrate of iron to become converted into per-nitrate of that metal. The per-nitrate of iron may be prepared—1st, by pouring over clean iron filings gradually nitric acid of commerce, previously diluted with its own weight of water, to keep the whole well cooled down by placing the vessel wherein the solution is taking place into very cold water, since otherwise the peroxide of iron which results of the action is precipitated, instead of being dissolved and combined with nitric acid, the affinity between nitric acid and oxide of iron being very feeble indeed; another plan is to dissolve hydrated peroxide of iron in nitric acid; another yet is to decompose 1 eq. of persulphate of iron by 3 eq. of nitrate of lead or nitrate of baryta. This latter mode is sure to give excellent and constantly good results, but is rather more costly; if, however, nitrate of lead is used sulphate of lead is obtained as a by-product which is of some value, while the precipitated sulphate of baryta, in case nitrate of baryta were used, is applicable in various ways as permanent white, which daily finds a more and more extended sphere of useful applications. For the preparation of persulphate of iron, which requires peculiar caution and manipulation, I must refer your reader to works on chemistry, or better, perhaps, on printing and calico dyeing.—Dr. A. A.

MEETINGS FOR THE WEEK.

MONDAY.—Royal Institution, 2. General Monthly Meeting.
TUESDAY.—Royal Institution, 3. Dr. M. Foster, "On the Development of Animals."
WEDNESDAY.—Geological, 8. 1. James Thomson, Esq., "On Some Carboniferous Corals." Communicated by Dr. P. Martin Duncan, Sec. G.S. 2. S. V. Wood, jun., Esq., F.G.S., "On the Pebble-beds of Middlesex, Essex, and Herts." 3. W. Topley, Esq., F.G.S., "On the Cretaceous Rocks of the Bas Boulonnais." 4. C. H. Weston, Esq., F.G.S., "Note on the Mendip Anticlinal.
THURSDAY.—Royal Institution, 3. Sir John Lubbock, Bart., "On Savages."
—— Chemical, 8. Messrs. E. T. Chapman and M. Smith "On Isomerism in the Organic Cyanides;" and "On the Artificial Formation of Pyridine." Dr. B. H. Paul, "On Testing Mineral Oils used for Lamps."
—— Royal, 8½.
—— Royal Society Club, 6.
FRIDAY.—Royal Institution, 8. Sir S. W. Baker, "On Abyssinia."
—— Geologist's Association, 8.
SATURDAY.—Royal Institution, 3. Sir John Lubbock, Bart., "On Savages."

TO CORRESPONDENTS.

Mech.—Oil of Tartar is the name sometimes given to carbonate of potash which has deliquesced to an oily-looking liquid.

A Constant Reader, whose query on Nitrate of Iron was inserted a few numbers ago, is informed that a letter is awaiting him at our office.

Dr. T. R. Fraser, Halifax, Nova Scotia.—We regret that the article forwarded is unsuitable for our columns.

A Subscriber.—Charcoal from the ignition of white sugar would be the purest, but probably ivory black would answer your purpose best.

Chemicus Senior's letter has created much diversion. We shall probably print it. Will our correspondent favour us with his name and address, to add to it?

C. Hunter.—Careful addition of lime water, with free exposure to the air, would probably be found the best means of removing iron from the water.

P. C.—Bauxite is found at Baux, near Arles, in the south of France. It contains about 55 per cent of alumina, the rest being oxide of iron, water, &c. For price, &c., apply to M. Morin, Salyndres, France.

Test Tube.—Two queries are inserted. The third was answered a short time back.

B. B. C.—Baillières, of Regent-street, have a collection of books on the subject. Consult these, and also the articles in Ure's and Watts's Dictionaries.

N. B. Rynard.—To give even a superficial outline of the subjects mentioned in your query would occupy several pages of the CHEMICAL NEWS. See the article in Watts's Dictionary.

B. W. Gibsone.—Communication received with thanks. We will endeavour to make use of it.

Errata.—In No. 442, "On Science Teaching in Schools," on page 243, line 43 from top, for "faults" read "faulty." On page 244, line 43 from top, for "heathers" read "breathes;" line 23 from bottom, for "trace" read "brace."

We are indebted to correspondents for the following periodicals containing reports and articles of Chemical or Scientific interest:—"The Chemist and Druggist;" "American Artisan;" "Mining and Scientific Press;" "Mining and Petroleum Standard and American Gas-Light Journal;" "Darlington and Stockton Times," May 16; "Le Moniteur Scientifique;" "Bulletin de la Société D'Encouragement;" "American Journal of Mining."

Communications have been received from The Council of the Society of Arts (with enclosure); G. F. Rodwell (with enclosure); C. Greville Williams, F.R.S. (with enclosure); J. Cliff; J. Smith, M.D., University of Sydney, N. S. Wales (with enclosure); Rev. B. W. Gibsone, M.A. (with enclosure); Dr. Lyon Playfair, F.R.S (with enclosure); Baron Von Liebig; C. J. Kaufmann; N. B. Rynard (U.S.A.); T. D. Reed, Canada; C. Hunter; H. J. Davies; W. Smallcome; A. Bird; J. W. Slater; H. McNaught; R. T. Dale, B.A.; R. Menzies; T. B. Fraser, M. D., Halifax, Nova Scotia, (with enclosure); McDougall, Bros. (with enclosure); Dr. Wood (with enclosure); H. Edwards; J. Parnell; Colonel W. H. Sykes, M.P.: E. F. Teschemacher; J. Denham Smith; J. Y. Buchanan; U. J. Kay Shuttleworth; Mawson and Swan; Albright and Wilson; J. Slessor; H. Jones; Townsend and Adams; J. Martin; Nicholson and Maule Dunn and Co.

THE CHEMICAL NEWS.

Vol. XVII. No. 444.

ON THE

ESTIMATION OF CARBON IN CAST IRON, WROUGHT IRON, AND STEEL.

ANALYSIS OF A CHROMIFEROUS WROUGHT IRON.

By M. BOUSSINGAULT.

M. Breche, an engineer, residing at Medellin, in Central America, sent me a specimen of cast iron derived from an oxidised iron mineral, reduced in a blast furnace fed with charcoal. It is white iron, in small plates, sp. gr. 7·45. Its hardness is great, and has been attributed to the presence of nickel. Upon dissolving the iron in hydrochloric acid a solution of a beautiful green colour is obtained. I soon saw that this colour was not due to nickel but to chromium.

Analysis gave :—

Combined carbon	.. ..	4·40 per cent.
Graphite	.. ..	—
Silicium	.. ..	0·75 ,,
Phosphorus	.. ..	0·07 ,,
Sulphur	.. ..	traces
Arsenic	.. ..	—
Nitrogen	.. ..	0·01 ,,
Manganese	.. ..	0·84 ,,
Chromium	.. ..	1·95 ,,
Vanadium	.. ..	traces
Iron	.. ..	92·50 ,,
		100·52

The estimation of the carbon having presented serious difficulties, I was induced to make a special study of this. I have been led to a process based upon the transformation of the iron into protochloride, without there being the least evolution of a gas capable of carrying off or burning the carbon. The agent which I use is bichloride of mercury. At first I worked in the dry way, and then with more success by the wet way.

The powdered iron is mixed with fifteen parts of bichloride. Add rapidly enough water to form a thin paste, which triturate for about half an hour in an agate mortar (when there is no objection to the introduction of a little silica, a glass mortar may be used). The diluted paste is introduced into a German flask, and kept for an hour at a temperature of 80° or 100°. Then throw it on a filter and wash with warm water. The protochloride of mercury after being well dried in the oven is put into a platinum boat and introduced into a glass tube communicating with a generator of *dry* hydrogen. Heat gradually up to a red heat in the current of gas. The protochloride is volatilised without decomposition; at least only very little mercury is reduced.

The volatilisation of the protochloride may equally well be effected in a current of nitrogen; but independent of the fact that it is not easy to keep up a sustained current of this gas, there will always be a suspicion of the presence of a little oxygen. In this respect hydrogen offers more security, especially if the device adopted at the *Ecole Normale au Conservatoire des Arts et Metiers* is employed, which consists in passing the dry hydrogen over a column of spongy platinum before it arrives at the tube containing the boat. The sponge retains the arsenic, and determines the disappearance of the oxygen which the hydrogen gas might contain.

In proportion as the protochloride of mercury disappears, the presence of carbon becomes manifest. Allow the boat to cool in a current of hydrogen, and then weigh it with the usual precautions. The carbon is voluminous, of a fine black; it lights and burns like tinder if the boat is heated a little. This is generally the case with carbon extracted from white irons, wrought iron, and steel. The graphite coming from grey cast irons only burns with the assistance of pure oxygen.

The carbon leaves an ash after its combustion. Before weighing this residue it must be heated red hot in a current of hydrogen.

From one gramme of white cast iron in large plates, I obtained—

Carbon .. .. 0·042 gramme.

After combustion there remained a crystalline residue having the appearance of silica. This residue heated in a current of hydrogen gas weighed 0·005 gramme, showing that the combined carbon weighed 0·037.

From one gramme of iron from a cementation furnace I obtained—

Carbon .. .. 0·009

After combustion—

Grey siliceous residue	.. .. .. 0·0015
Carbon	.. .. 0·0075

The carbon extracted from cast irons, steels, and even from the better qualities of iron always leaves small quantities of ash. What is its origin? The silica of this ash when it comes from steel or wrought iron, in which it cannot be supposed to come from scoria, comes from silicide; but it does not represent the totality of this because the silicium in combination with the iron, being first transformed into chloride by the bichloride of mercury, passes by the action of water into the state of silica, of which one part, being soluble, is carried away in the washings, while another part, insoluble, is left with the protochloride of mercury. It is this insoluble silica which is found after the combustion of the carbon. I have proved this by experiment.

The metallic substances submittted to the action of bichloride of mercury should be reduced to powder. There is no difficulty in this in the case of white cast irons, for they pulverise easily. But when grey cast irons, steel, and especially wrought irons, are operated on, recourse must be had to the file to divide them, and this is an inconvenience on which I do not need to insist. A skilful analyst, M. Damour, has tried whether it is not possible to chlorinise the iron without this preliminary division. He placed, in a spiral formed of platinum wire, a small steel cylinder weighing 1·06 grammes, and then suspended it in water containing 15 grammes of bichloride of mercury. It was put in a warm place. Two days afterwards the steel cylinder had disappeared: the protochloride of mercury, &c., was collected on a filter, washed, dried, and weighed. It yielded—

Carbon, &c.	.. .. 0·012
Residue of silica	.. .. 0·003
Carbon	.. .. .. 0·009

If the silicium combined with the iron is attacked in the cold by the chlorine of the bichloride of mercury, this is not the case with crystallised silicium. Upon triturating this with bichloride and water, so as to form a thin paste, no reaction is remarked. For the silicium to be attacked the operation must be conducted at an elevated temperature. 0·5 gramme of crystallised silicium, mixed with bichloride of mercury, was placed in a platinum boat, introduced into a glass tube, and raised to a red heat. The vapour of bichloride of mercury was then passed into the tube: all the silicium had disappeared in the form of chloride of silicium, and only a trace of silica remained behind in the boat.

ON THE

ADULTERATIONS AND FALSIFICATIONS OF BREAD.

By Prof. H. DUSSAUCE, Chemist.

BREAD kept for a few days experiences spontaneous alterations which render it unfit for use. Under the influence of heat, water, and acid yeasts it often developes in that food cryptogamic vegetations of a dark green colour and a special odour, belonging to the family of mushrooms. Among these cryptogamics the most common species is that known by the name of *mucor mucedo*. When that vegetation is examined with the microscope, it is observed that the large tufts it forms are composed of simple pedicles, carrying at the top a globulous and membranous body, which is the receptacle. This organ is filled with grains by maturity, it is broken with elasticity in water, and the sporules which go out form a kind of beard. That plant presents different colourations, according to the degree of maturity of the sporules.

It has been observed that common rye bread, from one day to the next, was covered with a kind of red efflorescence, and emitted a very bad smell. MM. Payen and Mirbel ascertained with the microscope that this reddish substance was composed of round corpuscules, which were the sporules of a mushroom, the *oidium curantiacum*. These seeds, as those of the *mucor mucedo*, were developed on the loaves with a great rapidity, and would bear a temperature of 212° without losing their germinative property. This alteration can be prevented by diminishing the quantity of water in the bread, increasing the dose of salt, and using it twelve hours after being withdrawn from the oven.

The amylaceous matter of the bread is destroyed by these mushrooms, it is transformed into water and carbonic acid, while the mineral, nitrogenous, and fatty substances are assimilated, and feed the vegetal.

A bread which has a blackish-blue colour was prepared with flours from hard African wheat, and wheat of lower qualities from Smyrna and Salamque. Poggiale has ascertained that this bread does not contain inorganic substances, such as iron, copper, &c., and with the microscope he has found an innumerable quantity of infusoria of the genus *Bacterium* of Dryardin. These infusoria have not been observed in biscuits made with the same flour; the blue colouration of these infusoria was manifested only after the fermentation, the baking, and the cooling. The gluten of the flours which have produced this phenomenon was soft, coloured, disaggregated, and had a very bad odour.

The use of bread containing moulds ought to be rejected; indeed, several cases of poisoning have been observed by the use of moulded bread. Johier has signalised the poisoning of three animals which had eaten moulded bread. Westerhoff has made known the case of poisoning of two children who had taken rye bread containing the *mucor mucedo*.

Bread prepared with flours altered by caries, darnels, &c., has a brown colour, and bitter taste, and a disagreeable odour. Wheat damaged by weevil, sea-water, or any other cause, gives a brown, bitter bread, not very nutritive, and consequently improper for domestic use. When bread containing darnels, &c., is treated by alcohol, and the filtered liquid is evaporated, an extract is obtained which has an acrid and bitter taste. Sometimes rice, probably boiled, is added to the paste. Bread thus prepared contains from 7 to 8 per cent more water than ordinary bread. It is then important to exactly determine the proportion of water.

Experience has demonstrated in bread the presence of legumin, by diffusing in a very small quantity of a solution of potash, containing 12 per cent of alkali, a little crumb; by a microscopical observation the tissue proper to the legumin is seen, while all the grains of starch have disappeared.

When bread containing flour of horse-beans or vetch is successively treated by nitric acid and by ammoniacal emanations, lines coloured in rose are to be seen. That colouration often appears only after fifteen minutes.

To ascertain potato starch in bread, examine by the microscope a little crumb, and add to it two or three drops of a solution of potash containing 1·75 per cent of alkali; the characteristic plates of the fecula are perceived if the bread has been adulterated with that starch.—*American Artizan.*

CARBONYLIC SULPHIDE.

THIS body, intermediate between $\overset{\ominus}{C}O_2$ and $\overset{\ominus}{C}S_2$, has been long foreseen. THAN has recently succeeded in forming it and in determining its properties. He first attempted its direct synthesis by passing carbonous oxide $\overset{\ominus}{C}O$, and sulphur vapour through a red-hot porcelain tube. A considerable quantity was thus produced, but it could not readily be separated from the excess of carbonous oxide. Moreover it was decomposed again by the heat into its constituents. Recalling then the fact that cyanic acid, by taking up the elements of water, was decomposed into carbonic dioxide and ammonia according to the equation

$$(\overset{\ominus}{C}O)''\,HN + H_2\overset{\ominus}{O} = H_3N + \overset{\ominus}{C}O_2$$

(in which cyanic acid is viewed as the imid of carbonic acid), Than saw that analogy required a similar decomposition for sulpho-cyanic acid thus:—

$$(\overset{\ominus}{C}S)''\,HN + H_2\overset{\ominus}{O} = H_3N + \overset{\ominus}{C}O\overset{\ominus}{S}$$

Experiment confirmed this theoretic view. By the action of strong sulphuric acid, the sulphocyanates take up $H_2\overset{\ominus}{O}$ and yield the gas with effervescence. The method of its preparation is as follows:—

Into a cooled mixture of five volumes concentrated sulphuric acid and four volumes of water, is placed in as much potassic sulphocyanate as will allow the mass to remain fluid. The evolution of gas commences spontaneously; should it become too violent the flask may be cooled by placing it in water; and if toward the close of the action it is too slow, a gas-flame may be put under it for a moment. Thus regulated, a constant stream of gas is obtained, which contains the vapour of water and of carbonic disulphide, as well as a trace of cyanhydric acid, and probably of formic acid. It is allowed to pass through three U-tubes, the first of which contains cotton covered with moist mercuric oxide to absorb the acids, the second, pieces of unvulcanised caoutchouc to remove the $\overset{\ominus}{C}S_2$,* and the third, calcic chloride to dry the gas. It is then collected over mercury, upon which when dry it has no action, even after many days. The moist gas, however, covers the mercury in a few hours with a thin coating of mercuric sulphide.

As thus prepared, carbonylic sulphide is a colourless gas, with an odour not dissimilar to that of $\overset{\ominus}{C}O_2$, but at the same time aromatic, recalling perhaps that of H_2S, though not at all disagreeable. It is soluble in its own volume of water, to which it communicates its odour. The taste of this solution is distinctly sweet, followed immediately by a peculiar prickly sulphur taste resembling that of H_2S and of $S\overset{\ominus}{O}_2$ together. After a few hours, however, the solution exhales a strong odour of H_2S. It is twice as heavy as air, and may be poured from one vessel to another. It reddens litmus feebly, much more so than $\overset{\ominus}{C}O_2$. When ignited it burns with a beautiful blue slightly luminous flame, yielding $\overset{\ominus}{C}O_2$ and $S\overset{\ominus}{O}_2$. It takes fire very readily, even by a spark on a match. If the jar be held with the mouth downward, the gas burns completely; but if it be reversed, and a taper be introduced, the gas takes

* The success of this device is so great that Than recommends its use in all similar cases. The CS_2 is completely removed.

fire, while the taper is extinguished, and again relighted at the mouth of the jar. Under these conditions the combustion is incomplete, and sulphur is deposited. A cold and dry beaker-glass held over a jet of the burning gas, shows no trace of moisture. With 1½ vols. of oxygen it forms an explosive mixture. Potassic hydrate absorbs the gas completely, though more slowly than CO_2. Dilute acids evolve H_2S and CO_2 from this solution, so that the absorption appears to take place thus:

$$COS + (HKO)_4 = K_2CO_3 + K_2S + (H_2O)_2.$$

The potash solution gives a copious black precipitate with ammonio-argentic nitrate; no trace of cyanogen was found in the filtrate.

Neither chlorine nor fuming nitric acid has any action upon the gas at ordinary temperatures. Passed over heated mercury in a bulb-tube, no change is observed, though if long boiled in the gas a trace of mercuric sulphide is formed. When sodium is treated in the same way, a white crust is produced at the common temperature, which by heating easily melts and becomes darker. At a low red heat the sodium ignites, burning with a brilliant light and leaving a black easily fusible mass, containing no trace of sodic cyanide, thus proving the absence of nitrogen in the gas.

When passed through mercuric-ethyl, at near its boiling point, a violent action takes place, pure metallic mercury without a trace of sulphide is separated, and a yellow liquid is produced, having a strong garlic odour. The author supposes it to be ethylic sulphopropionate.

If heated to low redness the gas is decomposed. This fact may be used to determine its composition as follows:—

Near the closed end of a U-tube, a fine platinum wire is fused, stretching across the tube. This arm being filled with the gas over mercury, and its volume ascertained, the wire is maintained at a bright red heat by means of the battery. About the wire the gas is decomposed; thick heavy clouds of yellow sulphur-vapour fall down the tube till the decomposition is complete. After cooling, the residual gas possesses the original volume. It is odourless, does not render baryta-water turbid, and burns with a pale blue flame; the product of the combustion renders baryta-water at once milky. It is hence carbonous oxide.

	Milligrams.
Since, therefore, 1 vol. (22·33 c.c.) carbonylic sulphide weighs	60
and 1 vol. (22·33 c. c.) carbonous oxide weighs..	28
The weight of the sulphur in the gas is	32

The gas contains therefore one atom of C, one of O, and one of S, and its formula is COS.

Two determinations of its density were made by Bunsen's method. The first, however, was on a portion of the gas before the use of the tube containing caoutchouc; it retains therefore a trace of CS_2 vapour. The observed density is 2·1152 and 2·1046 in the two experiments; the calculated is 2·0833. 60 is therefore its molecular weight. These results were confirmed by direct analyses of the gas, made according to Bunsen's methods.

In the opinion of the author, this gas is widely distributed in nature. Because it is so easily decomposed by water into CO_2 and H_2S, however, it has generally been confounded with these substances. From some experiments of his own, Than thinks he can assert almost positively that this compound is contained in the new and remarkable thermal spring of Harkány; and also in the cold sulphur spring of Parád, both in Hungary. He sustains this view by the fact that the water freshly drawn has precisely the odour of the gas, which recalls, but cannot at all be mistaken for that of H_2S; though on standing a few hours, the sulphydric acid odour appears. For these reasons it is probable that it occurs in many other sulphur springs, and it can scarcely be doubted that it could be found among the sulphur gases of volcanic action, and perhaps in those arising from organic decomposition.

It is recognised by the following reactions: 1st. Potassic hydrate deprives the gas, or its watery solution, at once, of its peculiar odour. The alkaline solution effervesces with dilute sulphuric acid, evolving H_2S (distinction from C_2S). 2nd. In acid solutions of silver or cadmium it produces no precipitate; but upon the addition of ammonia, the respective sulphides appear (distinction from H_2S). 3rd. Sodic nitroferricyanide has no action in neutral or acid solutions; but an intense blue-violet colour is developed by potash or ammonia. 4th. Iodide of starch is decolourised in a short time by this gas.—*Ann. Ch. Pharm., Suppl.*, Band v., 236, *Oct.*, 1867.

ON THE PROPOSED WATER SUPPLY FOR THE METROPOLIS.*

By EDWARD FRANKLAND, Ph.D., F.R.S.
Professor of Chemistry, Royal Institution.

(Concluded from page 258.)

Having thus discussed the organic portion of the solid impurity of these waters, let us now turn to the inorganic or mineral portion, which may be conveniently divided, as regards its most important constituents, into three subdivisions, viz.:—

1. Soap destroying substances.
2. Mineral compounds, constituting chiefly the skeleton of decomposed sewage or manure.
3. Poisonous substances, such as arsenic, copper, and lead.

The first or soap destroying category of substances communicate to water the quality called hardness. These substances are the salts of lime and magnesia; and the quantity of them contained in the proposed, as compared with the present, metropolitan water supply will be seen on reference to the analytical table. The hardening effect of these substances is also given in a separate line of the same table, from which it will be seen that the proposed is only about 1-10th as hard as the present water supply.

Tastes differ as regards hard or soft water for drinking purposes, and medical arguments have from time to time been advanced, now in favour of and now against each. It has been asserted in this country, for instance, that hard water is necessary for the formation of bone, and that the finger of Providence points to the advantage of hard water by the profusion of calcareous strata occurring in the earth's crust, whilst M. Belgrand states that the inhabitants of the hard-water districts of France notoriously suffer from carious teeth. It would probably be extremely difficult to prove either of these assertions. As regards the enormous advantages of soft water for washing, cleansing, and manufacturing purposes, there is, however, no difference of opinion. In Glasgow alone the annual saving of soap only, by the introduction of Loch Katrine water, for a previous supply of very moderately hard water, has been estimated at 36,000*l*. Having had the opportunity of comparing a six years' experience of the soft water supplied to Manchester, with a subsequent ten years' experience of the hard water of London, I can state that the soft water was for all purposes preferred by every member of my family. On removing from Manchester to London, the repugnance to drink the hard water of the latter city was at least as marked as that which I have sometimes noticed in persons making the transition in the opposite direction.

The hardness of the London waters is chiefly what is termed *temporary* hardness; that is, it is caused by the

* Read before the Royal Institution of Great Britain, Friday, April 3, 1868.

carbonates of lime and magnesia, the greater portion of which is gradually deposited on boiling the water for half-an-hour. By reason of this softening of such water by boiling, temporarily hard water is considered to be less objectionable than water of the same degree of permanent hardness. My own experience leads me to the conclusion that the advantages of temporary over permanent hardness have been considerably over-rated. In reality, water used for domestic purposes is, even when used hot, either not heated to the boiling point, or is boiled for too short a time to remove more than a small proportion of its temporary hardness. Thus, water drawn from the kitchen boilers of a dwelling-house and of the Athenæum Club was usually almost as hard as the cold water with which they were supplied, as is seen from the following table:—

Date and Hour.	Hardness of Cold Water.	Hardness of Hot Water.
	°	°
Sept. 30th, 1867, 8 P.M.	14·6	13·6
Oct. 1st ,, 8 P.M.	14·4	13·9
,, 2nd ,, 8 A.M.	14·4	13·4
,, 3rd ,, 9 P.M.	14·6	11·6
,, 4th ,, 8 A.M.	14·6	7·6
,, 7th ,, 8 P.M.	14·4	11·7
,, 8th ,, 8 A.M.	14·4	12·1
,, 9th ,, 8 P.M.	15·4	14·3
,, 10th ,, 8 P.M.	15·9	11·9
,, 11th ,, 8 A.M.	15·9	8·4
,, 12th ,, 8 A.M.	16·1	11·9
Nov. 8th ,, 5 P.M.	18·7	18·4
,, 11th ,, 5 P.M.	18·7	18·6
,, 12th ,, 6 P.M.	18·7	18·4

The amount of soap destroyed by the use of various waters for washing purposes is seen from the following table, in which certain Welsh and Cumberland waters are also introduced for the purpose of comparison :—

Soap destroyed by 100,000 *lbs. of various Waters.*

	lbs. of Soap destroyed.
METROPOLITAN WATERS.	
Thames Water	212
River Lea	204
Kent Company's Water	265
OTHER WATERS.	
South Essex Company's Water	253
Caterham Company's Water	84
Water supply of Worthing	285
,, ,, Leicester	161
,, ,, Manchester	32
,, ,, Preston	80
,, ,, Glasgow (Loch Katrine)	4
,, ,, Lancaster	1
Bala Lake	5
Thirlmere	8
Haweswater	16
Ullswater	23

In the recent supply of water to Paris from new sources, the importance of soft water attracted the attention of the eminent engineer M. Belgrand; a close investigation of the available sources, however, soon showed that he had unfortunately but little choice, as the really soft streams of the Fontainebleau sands (the minimum hardness is however 6°) and of the granite of Morvan (minimum hardness 2·2°) were mere dribblets. Of the latter M. Belgrand says :—"Sources qui donnent les eaux les plus pures du bassin de la Seine; déviation vers Paris impossible, en raison du peu d'importance des sources." Hence the river Vanne (17°-20°), somewhat softer than the Thames, was the softest available source, and having first conclusively demonstrated this, he consoles the Parisians by saying—"Les eaux du granite, du greensand et des sables de Fontainebleau, qui sont chimiquement plus pures, sont beaucoup moins agréables à boire."

The second category of inorganic substances contained amongst the solid impurities of waters, consists of *the mineral compounds constituting chiefly the skeleton of decomposed sewage or manure*. The putrescible nitrogenous organic matters present in water, or in the soil through which water percolates, undergo gradual oxidation and decomposition, by which their carbon and hydrogen are converted into carbonic acid and water, and their nitrogen into ammonia, nitrous and nitric acids. The last three remain in the water, constituting a record of previous contamination with putrescible nitrogenous organic matter. But rain-water always contains ammonia, and, as Dr. Bence Jones has shown, also nitrous and nitric acids. The nitrogen in these forms in rain-water, as it finds its way into rivers and springs, amounts in the aggregate to ·032 part in 100,000 parts of water, therefore this amount must be deducted from that found on analysis, as nitrogen derived from aërial sources. The remainder, if any, represents the nitrogen derived from putrefied nitrogenous organic matters with which the water has been in contact. To express this in terms of some known standard, I employ average filtered London sewage, which contains 10 parts of nitrogen in the form of putrescible organic matter in 100,000 parts. Thus, a water which contained one part of nitrogen in 100,000, as nitrous acid, nitric acid and ammonia would contain in 100,000 parts, the nitrogenous remains or skeleton of an amount of putrescible organic matter equal to that contained in 10,000 parts of average filtered London sewage. Such a water therefore is said to have a previous sewage contamination of 10,000 parts in every 100,000 parts. But it may be asked, Is this a true record of the previous history of the water in this respect? I believe it to be so, as far as it goes. *I believe that this nitrogen as truly represents a quantity of previously existing putrescible organic nitrogenous matter, as that the bones of a megatherium demonstrate the previous existence of an individual of that species*; but as the geological record of previously existing organisms is imperfect, so is the nitrogenous record; just as chemical and mechanical agencies have broken up and dissipated the remains of millions of animals during long geological periods, so does the action of growing plants, and perhaps also of living animals, remove from water, in a few hours or days, some portion of this skeleton of previous putrescible organic matter. Thus by storage in large reservoirs, the East London Company reduced the previous sewage contamination of the river Lea last summer from about 2,000 down to 230 parts in 100,000. The previous sewage contamination of a water as determined by analysis is therefore a minimum quantity.

But in addition to the aërial, for which due allowance is made, can there not be some other source of this skeleton than putrefied sewage or manure matter? Can it not be derived from putrefied vegetable matter—from peaty matter for instance? Without utterly denying the possibility of this, I venture to assert that nowhere, in this country at least, nor probably on the continent of Europe, is there such a quantity of nitrates, nitrites, or ammonia produced from vegetable sources as to appreciably affect the truth of my proposition that the nitrogen in these forms obtained by waters from terrestrial sources is substantially due to the putrefaction and oxidation of sewage and manure matters.

It has been objected to this view of the origin and significance of these forms of combined nitrogen, that waters derived from comparatively deep wells, in the chalk for instance, contain them in large quantities; thus the Kent Company's water exhibits a previous sewage or manure contamination of from 3,000 to 5,000 parts in 100,000. It is difficult to understand how such an objection could have originated, and it certainly disappears on examination; for instance in the above case, it is well

known that a very large proportion of the water collected in the London chalk basin consists of the drainage from manured land, and it is doubtless from this source that the large proportion of nitrates existing in this water is derived.

According to Mr. Way's analysis, the drainage water from cultivated land contains an amount of nitrates corresponding to the following proportions of previous sewage contamination in 100,000 parts :—

	Max.	Min.	Mean.
Previous sewage contamination of drainage-water from manured land	54,490	7,040	20,370
Ditto from pasture-land, unmanured ..	2,100	180	830

The results of the examination of various well-waters contained in the following table, further illustrate this point :—

Previous Sewage or Manure Contamination in 100,000 *parts of various Well-waters.*

Names of Waters.	Ammonia.	Nitrogen as Nitrates and Nitrites.	Previous Sewage Contamination.
Artesian Well at Grenelle	—	·006	0
Chalk Well at Caterham	·009	·000	0
Water delivered by Kent Company ..	·001	·408	3770
Water supplied to Worthing	·000	·426	3940
Water delivered by the South Essex Company	·006	·848	8205
Shallow Well at Leyland, near Preston	·003	2·466	24360
" at Ledbury	·001	1·575	15440
" at Redhill	·002	1·446	14160
" in Aldgate	—	3·840	38080
" in Minories	—	5·738	57060
" in Leadenhall Market ..	—	5·769	57370
" in St. Nicholas Olave Churchyard	—	7·596	75640
Well in the Rue Traversine, Paris ..	—	30·029	299780
Royal Institution Well-water	·001	4·355	43240

With two remarkable exceptions the above results show the greatest previous sewage contamination precisely in those places where it would be predicted; thus the shallow well-water of Leyland, near Preston, consists almost entirely of the drainage from cesspools and market gardens, through a sandy soil, the latter being heavily manured with night-soil, stable manure, and guano. It need therefore excite no surprise that nearly 25 per cent of this water has been in a condition equivalent to average London sewage. The quality of the waters taken from four of the city pumps and from the well in the Royal Institution* needs no comment; these shallow wells are now recognised as being fed by oxidised and somewhat diluted sewage. It is, however, in the well of the Rue Traversine, in Paris, that this kind of contamination reaches perhaps its maximum. The cesspool system is still in full activity in Paris, and the soil of that city is saturated with liquid manure of such a strength that one gallon of it is equivalent to three gallons of average London sewage.

As already mentioned there are in the above table two remarkable exceptions to the general previous sewage contamination of well-waters. These are the artesian well at Grenelle and the chalk well of the Caterham Water Company. With regard to the first, it is evident that the pressure of water which supports a column of 122 feet above the surface at Grenelle, precludes the possibility of admixture with the drainage of Paris, still there can be little doubt that the water supplying the chalk of the Paris basin is, to some extent at least, contaminated by manure, although the land through which it drains is far less generally cultivated than that through which the water supply of the London chalk percolates. The water from the Caterham Company's well, comes, I believe, from a greater depth than that of the Kent and South Essex Companies', and this circumstance, coupled with the observation of Mr. Dugald Campbell that the water of the deep chalk wells, unlike that of the shallower chalk-wells, is free from nitrates, and taken in connection with the fact that there is free water-communication between the upper and lower chalk, points to the conclusion that chalk possesses the property of abstracting nitrates from water. If this be the case, it would also account for the circumstance that the water of the shallow chalk-wells exhibits much less previous sewage contamination than might be expected; the average amount of nitrates found by Mr. Way in drainage water would indicate a previous sewage contamination in the chalk-water, equal to about 20,000 parts in 100,000, whilst the contamination actually exhibited in the case of the Kent, Worthing, and South Essex Companies' waters is only: Kent, 3,770, Worthing, 3,940, and South Essex 8,205 in 100,000 parts.

I have extended this investigation to various river and lake waters, as well as to spring waters, and have been here much indebted, as regards the non-British waters, to M. Boussingault's researches on the presence of nitrates in waters. The following tables exhibit the results of this investigation :—

Previous Sewage, or Manure Contamination, in 100,000 *parts, of various River and Lake Waters.*

Names of Waters.	Ammonia.	Nitrogen as Nitrates and Nitrites.	Previous Sewage Contamination.
RIVER WATERS.			
Nile	—	·102	700
Rhine, at Bâle	—	·026	0
Seine, at Notre Dâme ..	—	·152	1200
Ourcq	—	·223	1910
Thames	·005	·234	2062
Lea	·002	·220	1901
Severn (near source) ..	·003	·007	0
Lower Clywedog	·004	·006	0
Tarannon	·008	·024	0
Ceryst	·001	·052	210
Carno	·003	·049	190
Banw and Eira	·004	·023	0
Vyrnwy	·003	·011	0
Tylwch	·003	·004	0
Upper Rothay	·003	·002	0
Lowther	·002	·003	0
Kent	·001	·045	140
Sprint	·000	·021	0
Fourteen other Cumberland Streams	—	—	0
LAKE WATERS.			
Bala Lake	·001	·000	0
Thirlmere	·003	·002	0
Haweswater	·004	·000	0
Ullswater	·003	·005	0
Watendlath Tarn	·002	·006	0
Loch Katrine	·002	·031	0
Five Lakes and Tarns examined by Boussingault	—	—	0

* As this water enjoyed for a long time a very high reputation in the domestic department of this Institution, and as I have been frequently and very earnestly requested to withdraw a prohibition which I placed upon its use in the cholera year, 1866, I append, for my own justification, a more complete analysis.

	In 100,000 parts.
Total solid impurity	93·7
Organic carbon	·440
Organic nitrogen	·085
Nitrogen as nitrates and nitrites	4·355
Ammonia	·001
Total combined nitrogen	4·441
Previous sewage contamination	43240
Actual contamination with unoxidised sewage	4250
Hardness	32·5

The gases dissolved in this water contained scarcely a trace of oxygen. A half-pint glass of it contains nearly a quarter of a pint of water which has previously been in the condition of average London sewage, besides a dessert-spoonful of actual or unoxidised sewage. *It seems, therefore, highly probable that filtered and tolerably well-oxidised sewage, in its undiluted condition, would furnish the most popular water supply for London.* Such is the reliability of instinct in these matters.

Previous Sewage, or Manure Contamination, in 100,000 *parts, of various Spring-waters.*

Name of Waters.	Ammonia.	Nitrogen as Nitrates and Nitrites.	Previous Sewage Contamination.
Mother Ludlaw's Cave	·001	·034	30
Water supplied to Ferette (Haut Rhin)	—	·039	70
Spring near Dürmenach (Haut Rhin)	—	·114	820
Source of the Roppensviller (Haut Rhin)	—	·168	1360
Source of the Arcueil	—	1·111	10790
„ „ But at Montmartre	—	8·563	85310
„ „ Martinet	—	·557	5250
„ „ Trois Meules, St. Etienne	—	·210	1780
Spring at Nimes	—	·129	970
Ebersbronn (Bas Rhin)	—	·447	4150
Water supplied to Woerth-sur-Saüer (Bas Rhin)	—	·259	2270
Source of the Ill, near Winckel (Haut Rhin)	—	·104	720
Liebfrauenberg Spring (Bas Rhin)	—	·005	0
Seltz (Bas Rhin)	—	·008	0
Mineral Spring of Bussang (Vosges)	—	·003	0
Water supplied to Thann (Haut Rhin)	—	·010	0
Source of the Boelacker (Haut Rhin)	—	·018	0
Spring at Castle Fleckenstein (Bas Rhin)	—	Traces.	0
Thermal Spring at Baden	—	·016	0
„ „ Dax	—	·013	0
Source of the Presle (East Pyrenees)	—	·013	0

The results embodied in the above tables throw considerable additional light upon this form of water contamination. They show in the first place that waters which have not been in suspicious company exhibit little if any previous sewage contamination, thus in the whole of the Cumberland and Westmoreland district it only occurs in one instance (the Upper Kent which, as every tourist knows, has a little cultivated land on its banks), and that to a small extent only. In the Welsh waters again there are only three instances. The spring-water, which issues from the Greensand beneath an uncultivated but heather-covered surface at Mother Ludlaw's Cave, near Farnham, exhibits a mere trace of this contamination, whilst the waters of nine springs on the Rhine, in the Vosges, and in the Pyrenees, examined by M. Boussingault, exhibit no indications of previous sewage contamination. On the other hand, the spring forming the source of the But and issuing not far from the cemetery at Montmartre at once discloses its antecedents, and exhibits a previous sewage contamination of 85,310 parts in 100,000. It will be seen that the water of the Ourcq, which is now used only for watering the streets of Paris, exhibits a previous sewage contamination somewhat less than Thames water.

But what is the import of this previous sewage contamination? These skeleton compounds are innocuous, why trouble ourselves about them? True, they are innocuous, or nearly so; but inasmuch as they show that the water has been in contact with animal refuse they bring a heavy charge of suspicion against it. These refuse animal matters are known to contain that which is hurtful to human life. This hurtful matter is believed, on very strong evidence, to consist of spores, or germs of organisms, which are capable, under favourable circumstances, of producing in man such diseases as cholera, typhoid fever, and dysentery. Now such spores or germs, endowed as they are with vitality, will be likely to resist the oxidising agencies which convert the rest of the animal refuse into carbonic acid, water, nitric acid, nitrous acid, and ammonia. For instance, if the contents of an egg were beaten up with water and poured into the Thames at Oxford, the organic matter would probably be entirely oxidised and converted into mineral compounds before it reached Teddington; but if the egg were thrown whole into the Thames at Oxford, it would, if it retained its vitality, be carried down to Teddington without any decomposition of its organic matter. There can be no doubt that the spores or germs of many organisms are in like manner capable of resisting for a long time the decomposing action of water. Now no practicable process is known by which these spores, once introduced into water, can be again removed or can have their vitality destroyed. Filtration will not do it; in fact it is well known to engineers that water is often contaminated with visible suspended matter which cannot be separated by filtration; thus M. Belgrand says, "Lorseque l'eau est troublée dans le fleuve, elle sort louche de nos filtres." And again, speaking specially of the London water supply, "Le mode de dégrossissage employé par les grandes compagnies anglaises, *très convenable à Londres, ou l'on ne boit pas d'eau, ne vaut rien à Paris, ou les femmes, les enfants, les vieillards de la classe ouvriére n'ont pas d'autre boisson.* J'ai constaté par moi même, et les ingénieurs anglais n'en disconviennent pas, que l'eau sort des filtres *très chargée de matiere organique.*" Again, in the account of his highly remarkable researches on vaccine and small-pox poisons, recently communicated to the Academy of Sciences, M. Chauveau says regarding the organic germs contained in these poisons, that they "ne se déposent jamais complètement dans les couches profundes du milieu ambient, et passent à travers tous les filtres."

Boiling even for several hours cannot be relied upon for the destruction of such germs, some of which have recently been shown to retain their vitality after four hours boiling; in fact there can now no longer be any doubt that as contended by M. Pasteur, the cases of so-called spontaneous generation have all had their origin in ignorance of the excessive tenacity of life in the germs of the lowest organisms.

Nothing short of distillation therefore, as it is carried on in nature, can be relied upon to free, completely, sewage-contaminated water from its noxious constituents. Excessive filtration is doubtless to some extent a safeguard, and hence previous sewage contamination in chalk-water, if we could be certain that the water had been fairly filtered through some 100 feet of chalk, and that none of it gained access to the wells through fissures or swallow-holes, would have far less significance than it has in the case of a river water where the fine-suspended and noxious matters of sewage have but a comparatively slender chance of removal before the water reaches the consumer. We must also not forget that mere dilution fails, in the case of these suspended germs, to destroy their noxious quality, differing as they do in this respect remarkably from soluble poisons. The daily casting of a thousand fatal doses of strychnine into the Thames at Oxford ought not to occasion so much alarm amongst the London water-drinkers as the present flow of the Oxford sewage into the river, because the excessive dilution of the soluble strychnine would effectually prevent its producing any physiological effect. Each noxious living germ, on the other hand, contains within itself the power of indefinite multiplication and mischief. One such germ may be present in a wineglass-full of water, whereas it would be necessary to drink many thousand gallons of water to imbibe a noxious amount of strychnine, under the conditions just alluded to. I am therefore of opinion that water once contaminated with sewage or manure matter ought never again to be used for domestic purposes, if any other supply can be obtained; and I endorse the advice of M. Belgrand and the principle which guided him in the selection of the new water supply for Paris:—"On a dit des eaux potables, qu'elles étaient comme la femme de César, qu'elles ne devaient pas meme être soupçonnées, et c'est mon avis."

A few words will now suffice regarding the third class of mineral matters that may be present in the solid impurities of waters, viz., *poisonous substances, such as arsenic, copper, and lead.* These substances are only likely to occur in waters connected with mineral workings—one of them only, lead, has been detected in the proposed supplies, and that only in two streams in Cumberland, and in quantity far two minute to require any further notice.

The following table exhibits a comparative view of the amount and quality of the gases contained in the proposed and present supplies:

Gases expelled on boiling 100 *volumes of various Waters.*

Names of Gases.	Welsh Waters. Mean.	Cumberland Waters. Mean.	Thames Water. Mean.	Distilled Water.
Nitrogen ..	1·323	1·424	1·656	1·133
Oxygen	·612	·726	0·801	·617
Carbonic acid	·227	·281	3·652	·105
	2·162	2·431	6·109	1·855

You will find that the gases contained in the Welsh and Cumberland waters are very similar in quantity and proportion to those found in recently distilled water. Collected as these waters are near the mountain ranges which constitute the great condensers of natural distillation, this is exactly the result we should expect. The London waters differ mainly in containing more carbonic acid in solution, which makes them more sparkling in appearance. Sparkling waters are generally preferred by the public, although they commonly owe their briskness to extensive contact with decaying organic matter. It is thus that the highly-sparkling pump-waters of London are still preferred by many. On the other hand, soft waters are not necessarily vapid; no draught of water could be more delicious than that which is obtained from the public drinking-fountains of Glasgow, supplied from Loch Katrine, although I find that 100 volumes of this water contain only the following gaseous constituents:—

Nitrogen	1·731	vols.
Oxygen	·704	,,
Carbonic Acid	·113	,,
	2·548	

Action upon Lead.

The conditions which determine the action or non-action of water upon lead, have hitherto been involved in much obscurity. Messrs. Graham, Miller, and Hofmann, the Government Commission of 1851, on the supply of water to the metropolis, established the fact that the presence of dissolved oxygen and the absence of more than three volumes of carbonic acid in 100 volumes of water, are amongst the conditions necessary for the attack of lead.

The whole of the present water supply of the metropolis is perfectly protected from acting upon lead by the large quantity of carbonic acid which it contains. Still there are obviously other conditions involved in the problem; for all the samples of water from the Welsh and Cumberland districts contain, as shown in the above analytical results, dissolved oxygen, whilst not one of them possesses an amount of carbonic acid even remotely approaching that which is necessary to protect it, and yet some of these waters act violently upon lead, whilst others are entirely without action upon the metal. Having recently had occasion to observe that a sample of distilled water, which acted powerfully upon lead, completely lost this quality by momentary contact with animal charcoal, I found on further investigation, that a minute quantity of the chief constituent of bone-black, viz., phosphate of lime, completely protects water from action upon lead. I then carefully examined for phosphate of lime, the water of the River Kent (Upper Kent) which is eminently distinguished for its violent action upon lead, and the water of the River Vyrnwy, which, although nearly as soft as distilled water, has not the slightest action upon lead, even when placed in contact with a bright and freshly-cut surface of the metal for 24 hours. This examination established the fact that the water of the Vyrnwy contained an appreciable amount of phosphate of lime, whilst not the slightest trace of this substance could be detected in that of the Upper Kent. The waters from both the proposed districts have been carefully examined as regards their behaviour towards lead, and, as the result of this examination, it may be safely affirmed that no danger on this score need be apprehended from the introduction of water from either districts into the metropolis. I here exhibit to you samples of lead in various waters exemplifying the points upon which I have just spoken.

Lastly, I place before you in these large cylinders, holding more than a gallon each, samples of the Welsh and Cumberland waters, side by side with a similar sample of Thames water, which fairly represents the condition of more than 3-5ths of this water as it has been delivered in London since 21st of January last. But it may be asked, will these waters from Wales or Cumberland reach the metropolis in this colourless, transparent, and soft condition after passing through a conduit from 180 to 280 miles in length? Let me tell you what I conceive will be the effect of such a conduit upon the water. For the first two or three years a certain amount of lime from the surface of the cement in the conduit will dissolve in the water, communicating at first an amount of hardness probably not exceeding 5°; this effect will gradually subside, and after two or three years the water will be delivered in London in a slightly better condition than that in which it leaves the storage reservoirs—a slightly better condition because it will be somewhat better aërated than when it starts on its journey. At the present moment the water of Loch Katrine passes through a conduit 26 miles long, and I have lately carefully taken its hardness as it leaves the lake and as it is delivered to consumers in Glasgow. Its hardness on delivery in Glasgow is only 0·3°, exactly the same as in the lake, and its transit through 26 miles of conduit has therefore added no hardening constituent to the water. Now, if 26 miles of conduit fail to alter the hardness, I can only conclude that 180 or even 280 miles of conduit, if properly constructed, will also, after the lapse of a few years, be equally incompetent to produce any substantial increase in the hardness. At the time the above experiments were made, Loch Katrine water had flowed through the conduit for seven years.

These are the principal points I have to bring before you in connection with the proposed supply, and as a summary of the chemical investigation of the present water supply on the one hand, and of the samples furnished by the Welsh and Cumberland districts on the other, I may state the following conclusions to which these investigations have led me:—

1. The present water supply of the metropolis is largely contaminated with sewage. Both analysis and statistics concur in the statement that each glass of Thames water taken from the river by the companies, contains one teaspoonful of sewage.

2. Although this sewage is generally to a great extent oxidised before the delivery of the water in London, yet there is no guarantee whatsoever that all its noxious qualities are removed, because these noxious qualities are, in all probability, contained in the mechanically suspended and least oxidisable portion of the sewage.

3. The river water supplied in London is often very imperfectly filtered; and thus even the visible suspended matters of sewage are not wholly excluded from the water supply. Only on one occasion during the whole year 1867, have I obtained a transparent sample of water from the Southwark Company's mains. The Grand Junction Company's water was turbid four times out of twelve, the Chelsea thrice, the West Middlesex, Lambeth, and East London each twice, out of the twelve occasions when the samples were drawn for analysis. The New River Company alone delivered perfectly filtered water during the whole year.

4. The quality of the water supplied to London is greatly inferior to that of any other town in the United Kingdom, whose supply I have examined.

5. The distribution of water in the metropolis still continues, with but slight exceptions, on the intermittent

system, a system which has been abolished in almost every town of importance in the United Kingdom.

6. The water which it is proposed to supply either from the Welsh or the Cumberland districts is of excellent quality. It is equal or superior to that supplied to any town in Great Britain.

7. The water from each of the proposed districts is extremely soft, pleasant to drink, and of good aëration.

8. These waters have never been contaminated with sewage, and are therefore above all suspicion.

9. They can be distributed in the present system of supply pipes without any danger of lead contamination.

The choice between the present and proposed supply rests virtually with the intelligent inhabitants of the metropolis. Will you go to a source of pure water uncontaminated with sewage, or will you continue the existing supply? I can anticipate your verdict, but you must not delay to record it. These splendid sources now available will not remain much longer within your reach.

In conclusion, I beg to quote the opinion of one of our highest medical authorities on the dangers of sewage-contaminated water. Unpleasant as the theme may be, this opinion is in the highest degree deserving the earnest attention of every individual who has progressed beyond the state of savagery. In his Report on the Cholera Visitation of 1866, Mr. Simon, the medical officer of the Privy Council, says :—"It cannot be too distinctly understood that the person who contracts cholera in this country is *ipso facto* demonstrated with almost absolute certainty to have been exposed to excremental pollution; that what gave him cholera was (mediately or immediately) cholera-contagium discharged from another's bowels; that, in short, the diffusion of cholera among us depends entirely upon the numberless filthy facilities which are let exist, and especially in our larger towns, for the fouling of earth and air and water, and thus secondarily for the infection of man, with whatever contagium may be contained in the miscellaneous out-flowings of the population. Excrement-sodden earth, excrement-reeking air, excrement-tainted water, these are for us the causes of cholera. That they respectively act only in so far as the excrement is cholera-excrement, and that cholera-excrement again only acts in so far as it contains certain microscopical fungi, may be the truest of all true propositions; but whatever be their abstract truth, their separate application is impossible. Nowhere out of Laputa could there be serious thought of differentiating excremental performances into groups of diarrhœal and healthy, or of using the highest powers of the microscope to identify the cylindro-taenium for extermination. It is excrement, indiscriminately, which must be kept from fouling us with its decay.

"And thus it is that my practical advice remains substantially what it has been for years. The local conditions of safety are, above all, these two:—(1) that, by appropriate structural works, all the excremental produce of the population shall be so promptly and so thoroughly removed, that the inhabited place, in its air and soil, shall be absolutely without fœcal impurities; and (2) that the water supply of the population shall be derived from such sources, and conveyed in such channels, that its contamination by excrement is impossible.

"What good results are got even by rough approximation to those sanitary standards has already been abundantly shown here. The way in which the southern districts of London, with their three-fourths of a million of population, have gradually gained *comparative* immunity from cholera *in proportion* as their two water companies have ceased to distribute sewage-tainted water among them, is a matter of familiar history.

"That cholera is still a terror to Europe shows how scantily such illustrations are yet understood. Even here in England the objects which I have named as essential are at best but rarely fulfilled; indeed for vast numbers of our population scarcely rudimentary endeavours have been made to attain them. Town after town might be named, with myriad on myriad of population, where there is little more structural arrangement for the removal of refuse than if the inhabitants were but tented there for a night. The case of the water supply is no better: my reports are incessantly showing the too frequent foulness of private supplies, while as regards public water supplies, *such as generally are in the hands of commercial companies*, it has again and again been shown (and seldom more pointedly than in the present volume), that their conveniences and advantages are countervailed by dangers to life on a scale of gigantic magnitude, unless those who administer the supplies act under a very deep sense of responsibility.

"Cholera, ravaging here at long intervals, is not Nature's only retribution for our neglect in such matters as are in question. Typhoid fever and much endemic diarrhœa are, as I have often reported, incessant witnesses to the same deleterious influence; typhoid fever which annually kills some 15,000 to 20,000 of our population, and diarrhœa which kills many thousands besides. The mere quantity of this wasted life is something horrible to contemplate, and the mode in which the waste is caused is surely nothing less than shameful. It is to be hoped that, as the education of the country advances, this sort of thing will come to an end; that so much preventible death will not always be accepted as a fate; that for a population to be thus poisoned by its own excrement, will some day be deemed ignominious and intolerable."

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

Weekly Evening Meeting, Friday, April 24, 1868.

ON SOME NEW EXPERIMENTS ON LIGHT.

By J. H. GLADSTONE, Ph.D., F.R.S.

The speaker commenced by referring to the fact that we are constantly making new experiments or observations on light: in fact, all seeing is but a comparison of different degrees of light and shade, and the contrast of colours. Most of the rays that meet our eyes from surrounding objects are reflected rays, but some of the commonest things, such as the water-bottles and tumblers of cut-glass on our dining tables, exhibit beautifully the bending, the magnifying, the diminishing, and the production of coloured fringes, due to refraction. The purpose of this discourse was to rise from the simplest phenomena of this kind to a consideration of refraction-equivalents, and to describe the state of our present knowledge in regard to them.

By means of the electric lamp it was shown that a piece of glass, or other transparent body, will throw a perfectly black shadow if the two surfaces through which the ray passes be not parallel; that the light is then bent on one side, and at the same time spread out into its component colours; that this bending (refraction) varies with the amount of inclination of the two surfaces to one another, but in such a way that the sine of the angle of refraction bears a constant ratio to the sine of the angle of incidence; that this constant number, termed the index of refraction, or μ, belongs only to the one substance, each solid liquid, or gas, having its own index; that there is no necessary connection between the amount of refraction and the length of the spectrum (dispersion) caused by different substances, whether gaseous, liquid, or solid—for instance, a solution of an iodide always disperses more

than a solution of the chloride of the same metal, even though it be diluted to the same amount of refraction.

This index of refraction is affected by change of temperature. In liquids, and probably in all gases, the bending decreases as the thermometer rises; in solids, on the contrary, as lately shown by Fizeau, the change is in the opposite direction, crown glass always remaining the same, and fluor spar being the only case where he observed a diminution. This was experimentally demonstrated in regard to liquids. Thus a yellow sodium ray, which had passed through a hollow prism filled with oil of nutmeg, and thence through another filled with bisulphide of carbon, moved some inches along the screen, when the nutmeg oil was warmed a few degrees by stirring it with heated iron wire. This index of refraction is still more materially affected when a body passes from the solid to the liquid, or from the liquid to the gaseous condition; a fact that was illustrated by the visibility of the water melted in crystalline spaces in the middle of a block of ice.

The index of refraction of a mixture is moreover not always the mean of the indices of its constituents. Thus a ray passed successively through two hollow prisms filled with equal quantities of alcohol and water respectively, fell on the screen in a certain position; but when the two liquids were mixed together, and divided between the two prisms, the ray was visibly refracted to a greater distance.

These changes depend on the alterations of volume which the substances undergo; and the speaker, in conjunction with the Rev. T. Pelham Dale, had observed in liquids that the index of refraction, *minus* unity, divided by the density (in symbolic language $\frac{\mu-1}{d}$) is constant for all temperatures, and for all mixtures, or rather that the coincidence is very close but not quite perfect on account of some other law not yet understood. This conclusion has been abundantly verified by Landolt of Bonn, Ketteler, and Wullner, and the former experimenter has founded upon it a method of analysing mixtures of liquids.

This unchangeable number was termed the "specific refractive energy" of the substance, and it seemed to hold good notwithstanding a change from the solid to the liquid or the gaseous condition. It was early observed that the specific refractive energy of a compound bore a close resemblance to the mean of the specific refractive energies of its components. Landolt, by multiplying this number by the chemical equivalent, facilitated the calculation greatly. He termed this new number the "refraction-equivalent," $P\frac{\mu-1}{d}$, and proofs have rapidly accumulated that the number is little affected, not only by temperature, change of aggregate condition, mixture, or solution, but even by strong chemical combination.

Thus diamond, which is crystallised carbon, has the refraction-equivalent 5·0; sulphur has 16·0. Bisulphide of carbon, CS_2, which is nearly the most refractive liquid known, should therefore be represented by $5 + 2 \times 16$, that is 37·0. The experimental number is 37·3. But the diamond will burn in oxygen, and is thus converted into carbonic anhydride, while it is possible to reduce this gas into another containing only half the amount of oxygen, namely, carbonic oxide. The refraction-equivalents of these gases, as deduced from Dulong's observations, are respectively 10·03 and 7·53; but the difference between CO_2 and CO is one equivalent of oxygen, and the difference between the above numbers is 2·5. This then may be taken as the refraction-equivalent of oxygen, and subtracting it from CO = 7·53 we have remaining C = 5·03, practically the same number as that obtained directly from crystallised carbon. Similarly, but generally by more indirect methods, it has been determined that this element, whether pure as diamond or combined with other elements to form gases as the above-mentioned, coal-gas, or cyanogen, or liquids as chloride of carbon, benzol, oil of turpentine, alcohol, or ether, or solids as paraffin, sugar, or camphor, is still exerting the same influence on the rays of light that set its particles in motion, an influence that we can express by the number 5·0. Again, to revert to sulphur, the two salts sulpho-cyanide and cyanide of potassium—K S Cy and K Cy—differ by one equivalent of this element, and their refraction-equivalents as determined from their aqueous solutions are respectively 33·4 and 17·1, numbers differing by 16·3, a number almost identical with that reckoned from molten sulphur. In this way the refraction-equivalents of a large number of the elements have been determined; and the following table comprises what seem the most probable numbers among those that have been hitherto published by Landolt, Haagen, and Schrauf, as well as the speaker:—

	Atomic-weight.	Refraction-equivalent.
Hydrogen	1·0	1·3
Chlorine	35·5	9·8
Bromine	80·0	15·7
Iodine	127·0	24·4
Oxygen	16·0	3·0
Sulphur	32·0	16·0
Carbon	12·0	5·0
Silicium	28·0	6·2
Nitrogen	14·0	4·1
Phosphorus	31·0	18·5
Arsenic	75·0	16·0
Antimony	122·0	25·7
Vanadium	51·4	25·4
Sodium	23·0	4·9
Tin	118·0	19·2
Copper	63·4	11·2
Mercury	200·0	21·6

The above numbers are reckoned for the red ray. Most of them as yet claim to be considered only as approximative; and it seems certain that some elements, as oxygen and sulphur, have more than one refraction-equivalent.

Vanadium, though included in the above table, has only just been determined, and that from the oxytrichloride which Professor Roscoe exhibited a few weeks before. It is interesting, as it supports his theory of the close analogy of phosphorus and vanadium, for these two bodies, with sulphur, exceed all others in refraction and especially in dispersion.

The speaker stated that he was now engaged in examining the effect of salts in solution on the rays of light, and that he hoped to determine in this way the refraction-equivalents not only of a multitude of salts, but of the metallic elements themselves.

But the question may be asked, "If a substance has a refraction compounded of the refraction of its constituents, how can bodies such as Iceland spar have two refractive indices?" Now these are crystalline bodies, or if uncrystallised they have become doubly refracting by being unequally heated or compressed. In either case we may suppose a different amount of tension in different directions; and the fact of the two rays being oppositely polarised points to some such difference of molecular arrangement. It is easy to understand that the change of tension or internal structure may act in the same way as a change of density in modifying the velocity of transmitted light, and therefore the amount of its refraction. But if we take the crystal to pieces by dissolving it, there can then no longer be unequal tension or unsymmetrical arrangement of particles, and it must have one refraction-equivalent. And this is always the case. The numbers deduced from Brewster's observations of the two rays of crystallised nitre are 16·3 and 25·0, while the equivalent of nitre dissolved in water is the intermediate number 21·8.

FOREIGN SCIENCE.

Paris, June 3, 1868.

A plea for unapplied science.—The congress at Sorbonne.—New acetate of chromium.——Academy of Sciences:—Manufacture of phosphate and fluoride of sodium.—New process of refining sugar.—Vegetable fibres employed in industry.—18th of May: Award of the long-deferred prizes.

At a Congress of the Learned Societies, at Sorbonne, M. Nicklès delivered a lecture "On the new fluosalts and their uses." An account of the ferric fluosalts made by M. Nicklès has already appeared under the heading Foreign Science. He showed the disappearance of colour from the sulphocyanide of iron upon the addition of an alkaline fluoride, and numerous experiments of a similar kind, showing how the ordinary coloured precipitates of various metallic oxides were modified. Fluoride of potassium offers no impediment to the production of ferrocyanide of copper, but many polycyanides are decomposed. Platino-cyanide of magnesium is precipitated white, at the same time that it loses its dichroïsm; the beautiful blue crystals of mangano-cyanide of potassium are also destroyed by fluoride of potassium. After describing numerous coloured substances that were interfered with, M. Nicklès remarked—"What is the good of all these operations, having for their purpose the destruction of beautiful colours, will one ask? New facts are made known, errors are rectified, the history of a simple body becomes complete, but what matters this to those who cry *cui bono* where these products of human labour, where these conquests over nature are not immediately applicable to our physical wants. We might reply, that a new fact being acquired, no one can predict all the consequences which may result." M. Nicklès justified in eloquent terms purely scientific research, and alluded to the vast number of substances, largely employed in industrial operations, which had remained unused for great lengths of time, in the majority of cases, too, these substances having been first made only for scientific interest.

A paper "On a new acetate of chromium," has been published by M. Schützenberger. By mixing four or five equivalents of neutral acetate with one equivalent of neutral nitrate, and concentrating by heat, small green crystals were obtained; this salt, dried at 110°, possessed the formula—

$$\left.\begin{matrix}(Cr_2)''''''\\ 4(C_2H_3O)\\ NO_2\\ H\end{matrix}\right\} O_6 + H_2O$$

The salt is soluble in warm glacial acetic acid, the solution upon cooling yielding beautiful fern-like masses of crystals, which, after desiccation, possess the formula—

$$\left.\begin{matrix}(Cr_2)''''''\\ 5(C_2H_3O)\\ NO_2\end{matrix}\right\} O_6 + 4H_2O$$

From these experiments chromium, like iron, is evidently capable of forming a series of salts of two acids. Interesting as this may be, M. Schutzenberger has investigated the action of heat upon various acetonitrates, and the account of their behaviour when heated occupies the larger portion of the paper referred to. The final result and progressive states, induced by heat, are the same in all of these salts. Towards 200° water and acetic acid are disengaged; above this temperature nitrous vapours appear, at the same time the pulverulent residue assumes a deep yellowish brown tint. At this stage the compound is still soluble in water, and reagents detect the presence of chromic acid. Finally, at about 350° (in mercurial vapour) a brisk action is established, gas being disengaged, and the light powder thereby thrown into little conical heaps. In a few moments this stage is passed, and there remains a light powder of a clear green tint, pyrophoric while hot. The ultimate product contains no nitrogen; prepared with an acetonitrate purified several times by crystallisation, and heated by mercurial vapour in a current of carbonic acid to remove acid vapours and protect the heated residue from the atmosphere, the residue yielded upon analysis the following numbers:—Cr 34·1, C 22·43, H 3·14, numbers agreeing pretty closely with those required by the formula—

$$\left.\begin{matrix}(Cr_2)''''''\\ 3(C_2H_3O)\\ H\end{matrix}\right\} O_5$$

which is that of a first, triacetochromic anhydride. In presence of water this powder hydrates immediately, with elevation of temperature, and becomes a homogeneous green paste. Dried, this paste shows a fine deep and peculiar green tint, possessing transparency. As more and more water is added, the paste becomes proportionately thinner, till at last a veritable solution is apparently produced. This solution is colloidal: the addition of a neutral salt of an alkali precipitates the whole of the acetate. The anhydrous powder, heated to about 400°, loses more acetic acid, while retaining the property of hydrating with water; probably there is formed a second anhydride, diacetochromic anhydride—

$$\left.\begin{matrix}(Cr_2)''''''\\ 2(C_2H_4O)\end{matrix}\right\} O_4.$$

Finally, in the vapour of boiling sulphur the decomposition is complete, and there remains only the anhydrous oxide mixed with carbon.

The following memoirs were brought before the Academy at the meeting held on the 11th of May:—"Analysis of a chromiferous pig iron," by M. Boussingault (translated in full); "on a new process of refining coarse sugars and molasses, yielding colourless and pure sugar, without the employment of animal black or albuminous substances," by M. Woestyn; "vegetable filaments employed in industry, characters permitting of distinguishing between them;" M. Jean addressed a rectification to his note on the manufacture of phosphate and fluoride of sodium; M. Zaliwski-Mikorski made a communication relative to the weight of the air.

The process of sugar-refining made known in M. Woetsyn's memoir is simple. The sugars being dissolved to a certain degree Beaumé, varying according to the practice of the refinery, milk of lime is introduced, the amount added for fine coloured sugars being reckoned by thousandths, progressing to hundredths for the yellow tints. After having intimately mixed the lime and the syrup, a current of carbonic acid is made to pass into the mixture, until test paper shows no trace of alkalinity. M. Woestyn operates at from 20° to 30° C. The operation is terminated by ebullition, to decompose the bicarbonates, and the deposit is separated by filtration, sometimes decantation may be substituted. The resulting sprup is deprived of colour, and yields sugar of great fineness, both in regard to taste and brilliancy.

M. Vétillard's memoir was a very minute description of the microscopical characters, when treated by reagents, and in some cases dissected, of linseed, hemp, jute, New Zealand flax, China grass, and cotton fibres.

M. Jean informed the Academy that by an error in calculation, he had come to the conclusion that in fusing tribasic phosphate of lime with an excess of sulphate of soda and carbon, nearly the whole of the phosphoric acid was obtained in the state of neutral phosphate. Recent experiments have proved to him, that under these conditions, the phosphate of lime only cedes two-thirds of the phosphoric acid in the soluble state.

At the meeting held on the 18th, M. Chevreul presided, and the prizes in various branches of sciences were awarded to the candidates of 1867. The prize subjects ranged through many sciences, physiology, medicine, astronomy, mathematics, chemistry, &c. The works worthy of a prize in connection with noxious trades

(founded by M. De Montyon) were considered by MM. Combes, Dumas, Payen, Balard, and Chevreul, reporter. The report states that long since the law of 1810 has shown itself insufficient, notwithstanding various modifications made, indeed for twenty years the necessity has been felt of placing the class of manufactures referred to in this law more in harmony with the progress of science, in the double interest of health and the manufactures themselves. The law of 1810 was created especially to prevent the dangers of acid vapours such as arise in the roasting of pyrites and the hydrochloric acid of the alkali factories recently established. Doubtless at this epoch there existed many works operating upon organic matters, and the inconvenience they caused was known; but these works were then restricted in number, and confined to localities where from their long existence, the inconveniences to the neighbourhood were tolerated. It was then in the double cause of the public health and the progress of industry that the administration of agriculture, commerce, and works, directed the consulting committee of arts and manufactures to revise the law of 1810. The committee deemed it desirable that they should possess a knowledge of the state of the factories and works in the foreign countries most advanced in point of industry, and to this end, M de Freycinet, a mining engineer, was commissioned to visit England. The subject of the questions proposed to M de Freycinet were—(1). The examination of factories or works considered dangerous or inconvenient from three points of view: the pollution of the atmosphere, the pollution of water, and the influence of the process upon the workmen. (2). The indication of the means, or processes of abatement employed in each noxious manufacture. M. de Freycinet's mission resulted in a report of 118 pages.

Finally, in 1866, M. de Freycinet visited England again by authority, this time to examine the employment of the sewage water of London. The first report was deemed so valuable that M. de Freycinet was charged to make a similar work for France; this matter was embodied in a report of 247 pages. The prize commission recognising the value of his services, proposedto award to M. de Freycinet 2500 francs. The proposition received the approbation of the Academy.

The chemical section unanimously voted the Jecker prize to M. Berthelot for his late researches on the carbides of hydrogen.

LABORATORY NOTES.

PREPARATION OF LITMUS PAPER.

I have had much trouble in obtaining a thoroughly satisfactory litmus paper. When used with blotting paper it is not as delicate as could be wished, and on one occasion when attempting to make it with sized paper, the blue tincture persistently turned red when it touched the paper. The latter reaction seemed to be due to the sizing material, and it occurred to me that if I sized some paper myself with pure gelatin, my object would be obtained.

I can recommend the following receipt:—

"Digest 20 grm. litmus with 100 c.c. water for some time, shaking occasionally; then filter. To the filtrate add a slight excess of nitric acid, and boil; then neutralise exactly with potash. Now make a weak solution of gelatin by boiling 1 part of isinglass with 50 parts of water; draw white blotting paper through this and hang it up to dry. When dry paint one side with the above solution of litmus."

A. Vacher.

20, Great Marlborough Street, May 30.

MISCELLANEOUS.

Adulteration of Wines.—It is much to be regretted that cheap wines advertised and offered to the public as pure and wholesome, are adulterated with alum, and other ingredients of a most deleterious nature. Dr Barbier in the *Gazette Medicale de Lyon* states that he has treated a whole family, in vain, for acute pains in the stomach and indigestion. At last he had their wine analysed and found no less than two drachms of alum in the bottle. As soon as their wine was changed, their gastralgia ceased. We have ourselves obtained samples of so called *pure* claret, which we have reason to believe, contained a considerable quantity of alum; it is therefore evidently necessary for those who are accustomed to drink these wines, to have some guarantee of their purity.

Lighting the Street Lamps by Electricity.—The street Gas-Lighting Machines by Electricity, to be seen at No. 7 Duane street, are really wonderfnl, and we see no reason why they should not be generally adopted by all the cities using gas for lighting, on the points of economy and convenience. It is a simple, small machine placed in each lamp-post and connected by insulated wires with a central point, where the operator can, by simply starting the clock work attached to the batteries, at once open the cocks is each lamp and light up a whole city in the twinkle of an eye, or put out the lights at his pleasure. We notice by our Mayor's late inaugural address, that 38,000 dollars is the estimate for labour and lighting of the city street lights. The labour and the amount of gas that would be saved in the time allowed for lighting and putting out, and the amount that is now used on bright moonlight nights, constitute an aggregate and no doubt would more than pay for the whole expense of introducing the improvement for the first year. The experiments at Duane are worth witnessing by all.—*N. Y. Times.*

The Secrets of the Ocean.—Mr. Green, the famous diver, gives the following sketch of what he saw at the "Silver Banks," near Hayti:—"The banks of coral on which my divings were made are about forty miles in length, and from ten to twenty in breadth. On this bank of coral is presented to the diver one of the most beautiful and sublime scenes the eye ever beheld. The water varies from ten to one hundred feet in depth, and so clear that the diver can see from two to three hundred feet when submerged, with but little obstruction to the sight. The bottom of the ocean, in many places, is as smooth as a marble floor; in others it is studded with coral columns, from ten to one hundred feet in height, and from one to eighty feet in diameter. The tops of those more lofty support a myriad of pyramidal pendants, each forming a myriad more, giving reality to the imaginary abode of some water nymph. In other places the pendants form arch over arch; and, as the diver stands on the bottom of the ocean, and gazes through in the deep winding avenues, he finds that they fill him with as sacred an awe as if he were in some old cathedral which had long been buried beneath old ocean's wave. Here and there the coral extends to the surface of the water, as if the loftier columns were towers belonging to those stately temples that are now in ruins. There were countless varieties of diminutive trees, shrubs, and plants, in every crevice of the corals where water had deposited the earth. They were all of a faint hue, owing to the pale light they received, although of every shade, and entirely different from plants that I am familiar with that vegetate upon dry land. One in particular attracted my attention; it resembled a sea fan of immense size, variegated colours, and the most brilliant hue. The fish which inhabit these 'Silver Banks' I found as different in kind as the scenery was varied. They were of all forms, colours, and sizes—from the symmetrical goby to the globe-like sunfish, from the dullest hue to the changeable dolphin."—*Journal of the Telegraph.*

PATENTS.

Communicated by Mr. Vaughan, F.C.S., Patent Agent, 54, Chancery Lane, W.C.

GRANTS OF PROVISIONAL PROTECTION FOR SIX MONTHS.

965. H. Bessemer, Queen Street Place, Cannon Street, London, "Improvements in the manufacture of refined iron and of malleable iron and steel."

967. H. Bessemer, Queen Street Place, Cannon Street, London, "Improvements in the manufacture of malleable iron and steel, and in apparatus used in such manufacture."—March 21, 1868.

978. G. F. Guy, Bury St. Edmunds, Suffolk, "Improvements in the manufacture of sugar and other alimentary substances from beet-root."

983. E. Vignier, Fowkes Buildings, London, "Improvements in distilling and rectifying spirits, and in apparatus employed therein."—March 23, 1868.

1020. T. Whitehouse, Wednesbury, Staffordshire, "Improvements in blast furnaces."

1023. J. Jameson, Catherine Terrace, Gateshead, Durham, "Improvements in the preparation of safety paper."—March 25, 1868.

1044. T. Routledge, Ford Works, near Sunderland, and W. H. Richardson, Jarrow-on-Tyne, "Improvements in bleaching Esparto and other fibrous substances used in paper making."—March 26, 1868.

1045. A. Warner, Laurence Pountney Lane, London, "Improvements in the manufacture of cement."

1048. A. Scott, Paddington, Middlesex, "The production of an alcoholic fermented dry, sweet, or effervescent drink, of which tea or coffee, or theine or caffeine, is an essential ingredient."

1050. F. Bauman, Wellington Street, Strand, Middlesex, "The preparation of a certain combination of chemical substances, and its novel application to the treatment of wood or other fibrous material in the preparation of paper pulp."—March 27, 1868.

1063. T. C. Currie, Kensington, Middlesex, "An improved process for the preservation of milk."

1067. J. C. Coombe, Holloway, Middlesex, and J. Poole, Chelsea, Middlesex, "Improvements in coating iron, steel, and such like surfaces, and protecting and preserving them from the corrosive action of sea or salt water, and the oxidising influence of damp air, wet, and moisure."—March 28, 1868.

1071. H. Armstrong, Tow Law, Durham, "Improvements in the manufacture of steel and homogeneous iron."

1073. C. F. Claus, Middlesbrough-on-Tees, Yorkshire, "Improvements in the manufacture of iron."

1074. C. F. Claus, Middlesbrough-on-Tees, Yorkshire, "Improvements in the manufacture of malleable iron, and in the process of re-heating the same for the purpose of rolling or hammering it."

1076. J. H. Johnson, Lincoln's Inn Fields, Middlesex, "Improvements in the manufacture of cast steel and malleable iron from cast iron, and in the apparatus or means employed therein."—A communication from F. Ellershausen, Ottawa, Canada.

1077. J. H. Johnson, Lincoln's Inn Fields, Middlesex, "Improvements in the treatment of cork, and in the manufacture of a new compound or material therefrom, applicable to various useful purposes."—A communication from H. Hauer and W. Howell, Philadelphia, Penn., U.S.A.

1078. J. H. Johnson, Lincoln's Inn Fields, Middlesex, "Improvements in the manufacture of cast steel, and in furnaces to be employed therein, which furnaces are also applicable to the remelting of metals generally."—A communication from F. Ellerhausen, Ottawa, Canada."

NOTES AND QUERIES.

Phosphate of Iron.—Can any of your correspondents give me the chemical composition of the precipitate resulting from the addition of a small quantity of dil. sulphuric acid to a solution of pyrophosphate of iron; and also the reason of its formation?—Pharmacist.

Solvent for Essential Oils.—Your correspondent desires to be informed whether it is possible to find a solvent for essential oils which would render these soluble in water, without producing therein a milky appearance. To a certain extent all essential oils are more or less soluble in water, but only very slightly so; still orange-flower water, for instance, shows that the fragrance due to the Oleum Neroli, as contained in the orange flowers, can be imparted to water in great measure. If "Subscriber" does not desire or want a perfectly limpid and clear solution, he may make use of the property possessed by essential, as well as fixed oils, of becoming more largely soluble in water by the intermediate of emulsions, or rather mixtures fit to make emulsions, with either essential or fixed oils. Another mode of rendering the essential oils soluble in water, without becoming milky, is to rub the essential oils in small quantity with some sugar and the ordinary magnesia of the chemists' shops in a mortar, to add water gradually, and to filter; gum arabic may be substituted for sugar, and by addition of a few drops of strong acetic acid or pure ether, or better yet acetic ether, the solvent power of water for essential oils may be somewhat increased. It is clear that a dilute alcohol will keep in solution a larger quantity of essential oils than water alone. It is, at least on the Continent, and also in Mexico, a well-known fact that the peculiarly delicious fragrance of the vanilla may be thoroughly imparted even to cold water or milk by first rubbing the vanilla to powder along with lump sugar, and gradually adding the water or milk afterwards, and straining through a silk sieve, or filtering; of course an infusion may be made, but its fragrance is less pleasant. "Subscriber" is, I suppose, aware that many essential oils have a boiling-point higher than that of water, and that if it is desirable to make a strong aqueous solution it is often desirable to use hot, but best distilled water, to shake a quantity of the essential oil with the hot water, and allow to cool in a closed or corked bottle, and decant the clear solution.—Dr. A. A.

Aniline Green.—In reply to "Test Tube," I can inform him that aniline green has been produced in the preparation of Hofmann's violet (salts of triethylrosaniline) in the following way:—The mixture which produces the violet colour is heated to 110°—115° C., not as in the production of violet, for four to six hours, but for a much longer time. After opening the apparatus and distilling off the excess of iodide and alcohol there is obtained a mixture of Hofmann's violet and a more or less bluish green colour, which may become even a greenish blue when the operation has been continued for a sufficiently long time. If there is too much violet in the mass, and the green is too blue, it is best to treat the whole once more with the iodide. Some manufacturers treat their product with iodide until the whole or almost the whole is transformed into a green colour, to which they give the yellower shades by adding picric acid.—Beta.

TO CORRESPONDENTS.

X. A. P.—Phosphorus is now generally purified by treating the crude product with sulphuric acid and bichromate of potash. The red phosphorus, it is said, becomes oxidised first, and rises to the surface in the form of a scum, while the pure phosphorus remains perfectly colourless and translucent at the bottom of the vessel.

W. L.—The work shall be sent by post.

Alfred K.—The term antimony-vermillion has been applied to sulphide of antimony, which, under certain conditions of preparation, exhibits a pure red colour.

Hardware.—Iron vessels must be used; they are not attacked by strong nitric acid.

Communications have been received from H. Saunders (with enclosure); W. Wallace; J. R. Atkinson; J. Richardson; W. Rose (with enclosure); T. Geeves (with enclosure); Kuhlmann & Co.; A. Losh; W. de Castro; Mr. Young (with enclosure); J. Owens; T. Hunt; F. Maxwell Lyte (with enclosure); J. A. Wanklyn; W. F. Barrett (with enclosure); J. S. Bouck; W. Huggon (with enclosure); C. Cochrane (with enclosure); W. Browning; Castle & Lamb; W. Blythe (with enclosure); R. Crook; E. Wheeler, jun.; Dunn & Co.; C. J. De Winton; F. C. Calvert & Co.; R. A. Macfie.

MEETINGS FOR THE WEEK.

Monday.—Royal Geographical, 8½.
Tuesday.—Royal Institution, 3. Dr. M. Foster, "On the Development of Animals."
—— Photographic, 8.
Wednesday.—Microscopical, 8.
Thursday.—Royal Institution, 3. Sir John Lubbock, Bart., "On Savages."
—— Royal Society Club, 6.
—— Royal, 8½.
—— Zoological, 8½.
Friday.—Royal Institution, 8. Prof. Frankland, "On the Source of Light in Luminous Flames."
—— Astronomical, 8,
Saturday.—Royal Institution, 3. Sir John Lubbock, Bart., "On Savages."

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THE CHEMICAL NEWS.

VOL. XVII. No. 445.

ON THE ARTIFICAL FORMATION OF ORGANIC SUBSTANCES.*

By C. GREVILLE WILLIAMS, F.R.S.

CHEMICAL researches are liable at various epochs to take special directions. Before 1830, organic chemistry was comparatively little studied. The simplification of the methods of organic analysis by Liebig, took place at a most opportune moment, and gave an extraordinary impetus to the study of carbon compounds. So great was this influence that proximate and ultimate analysis made a progress the rapidity of which was unexampled in the history of science.

But chemists soon became dissatisfied with merely determining the composition of substances, and they very soon began eagerly to study their products of decomposition, and in this manner get a clue to the way in which nature had put them together.

The successful attack on this problem led to a much grander one suggesting itself. This was to utilise the insight analysis had given them into the constitution of substances, and to endeavour to build them up without the assistance of life. The lecturer showed that we thus arrive at the two great engines of chemical research, *analysis* and *synthesis*.

He then proceeded to define and illustrate experimentally these terms.

In inorganic chemistry the information supplied by the analysis of a substance often renders its synthesis easy. Water was decomposed by a battery, and the properties and quantitative relations shown. The mixed gases were then introduced into a soap-bubble, so prepared as to last a considerable time. It was by a simple contrivance attached to a thread, and the lightness of the enclosed gases was shown by the fact that the bubble was able to raise the thread and a disc of paper into the air. The energy with which the mixed gases combine to form water, was then shown by applying a light to the bubble, when it burst with a loud report. This simple but delicate experiment was twice repeated successfully. The *quantitative* synthesis of water was experimentally shown by passing hydrogen over cupric oxide in an apparatus which allowed of the collection of the water. It was then shown that in organic chemistry the molecules are generally too complex to be put together so easily; and this statement was proved by reference to the constitution of methylamine, the simplest of the organic alkaloids.

The lecturer than went somewhat fully into the question of the propriety of the use of the terms "organic" and "inorganic." He showed also that all the attempts hitherto made at separating chemistry into two distinct branches had failed; Liebig's definition of organic chemistry as the "chemistry of compound radicles" being obviously inadequate, inasmuch as some compound radicles (such as sulphuryl and phosphoryl), are certainly inorganic.

Laurent's definition "chemistry of carbon," is equally insufficient, inasmuch as carbonic anhydride, and carbonic tetrachloride, are as clearly inorganic as sulphuric anhydride or sodic chloride. He then proceeded to argue that chemistry was "one and indivisible," and stated that one of the chief aims of his discourse was to prove that assertion.

It was shown that until within the last few years all the specific attempts made to break the apparently natural barriers between organic and inorganic chemistry had proved failures.

It was true that in the course of the innumerable researches and experiments made by chemists, one or two of the simple organic bodies had presented themselves; but like urea and cyanogen, they were substances which, as it were, hovered on the confines of inorganic chemistry, and would have been called inorganic had they not contained carbon.

The grand problem, which consisted in taking the elements themselves, and building them up *gradatim* into the proximate principles existing in the tissues of plants and animals, until lately appeared almost hopeless. This apparent difficulty was shown to arise from the mistake of supposing the proximate principles of animals and vegetables to result from an occult power vaguely termed the "vital force." It was at one time supposed that the laws which regulate combination, were either suspended or modified in the tissues of living creatures, but it was argued that, whenever the proper reagents were made to act upon each other under the proper conditions, the same substances were produced which at one time were supposed to require the aid of vitality for their formation.

The problem of the "synthesis," or building up of the so-called organic substances, was then shown to present itself (in the present state of chemistry) under two aspects:—1st. Where they are prepared by the aid of reagents, which have themselves been produced directly or indirectly from animals or vegetables. 2nd. Where the synthesis was effected from the free elements themselves, from hydrogen and pure carbon.

The lecturer then proceeded to enumerate some of the principal instances where substances originally derived from animals or vegetables had been formed synthetically. Wöhler's synthesis of urea was shown to be one of the earliest in point of date, and his method was described, and also Kolbe's new process by the mere heating of ammonic carbonate to a point just below that at which urea is decomposed.

One of the next most important steps in the history of synthesis was shown to be the conversion of carbonic disulphide into carbonic tetrachloride or perchlorinated marsh gas. Inasmuch as carbonic disulphide is a purely inorganic body, it is evident that the formation of any substance from it is a case of true synthesis.

The following equations represent the steps by which acetic acid may be produced from carbonic disulphide:—

$$\underbrace{CS_2}_{\text{Carbonic disulphide.}} + 6Cl_2 = \underbrace{CCl_4}_{\text{Carbonic tetrachloride.}} + \underbrace{2(SCl_3)}_{\text{Sulphuric tetrachloride.}}$$

$$\underbrace{2(CCl_4)}_{\text{Carbonic tetrachloride.}} = \underbrace{C_2Cl_4}_{\text{Carbonic dichloride.}} + 2Cl_2$$

$$\underbrace{C_2Cl_4}_{\text{Carbonic dichloride.}} + Cl_2 = \underbrace{C_2Cl_6}_{\text{Carbonic trichloride.}}$$

$$\underbrace{C_2Cl_6}_{\text{Carbonic trichloride.}} + 2(H_2O) = \underbrace{C_2HCl_3O_2}_{\text{Trichloracetic acid.}} + 3(HCl)$$

$$\underbrace{C_2HCl_3O_2}_{\text{Trichloracetic acid.}} + 3H_2 = \underbrace{C_2H_4O_2}_{\text{Acetic acid.}} + 3HCl$$

This important series of reactions, then, result in the production of acetic acid, one of the most marked of the so-called organic acids, from purely inorganic materials.

The synthesis of oxalic acid by the direct union of carbonic anhydride with sodium, as recently accomplished by Dr. Drechsel, was next described, and it was shown that as oxalic acid, by mere distillation, yields formic acid, the synthesis of the first acid led directly to a new synthesis of the second.

* A lecture delivered before the Royal Institution of Great Britain, Friday, May 8th, 1868.

The other modes of effecting the synthesis of formic acid were then pointed out, viz.:—Berthelot's process, which consists in heating potassic hydrate in an atmosphere of carbonic oxide; and Kolbe and Schmidt's method, by exposing potassium to a warm moist atmosphere of carbonic anhydride.

The lecturer, in the course of his remarks on the constitution of formic acid, showed that the quantity of oxygen in it was so large that it only required one atom more to convert it into carbonic acid and water. Its easy oxidation was illustrated by letting it fall on plumbic dioxide in an apparatus which caused the evolved gas to pass into a solution of baric hydrate, the result being a copious precipitation of baric carbonate.

Having shown that acetic acid can be formed from carbonic disulphide and the chlorides of carbon, and oxalic and formic acids from the oxides of carbon, the lecturer proceeded to indicate the modes in which complex bodies, hitherto obtained from animal and vegetable sources, can be built up from elemental carbon and hydrogen.

If carbon can only be made to combine directly with hydrogen, no matter how simple the resulting compound may be, it becomes possible to effect the synthesis of a vast number of the most characteristic substances found in animals and vegetables.

This brilliant result has been accomplished through the agency of acetylene, a most remarkable hydrocarbon which was first noticed by Edmund Davy as long ago as 1836.

There are two methods by which acetylene can be formed from inorganic materials—one devised by Berthelot, and the other by the lecturer. The first consists in passing a stream of hydrogen through a globe in which the voltaic arc (from 70 or 80 cells of a Grove's battery) is produced between carbon points. At this tremendous temperature, the carbon unites directly with the hydrogen. The experiment was made, and the production of acetylene shown, by the formation of a precipitate in a solution of ammoniacal cuprous chloride.

The speaker then showed, experimentally, that much larger quantities of acetylene can be formed by the decomposition by the induction spark of carbonic tetrachloride in presence of hydrogen, in accordance with the equation—

$$2(CCl_4) + 5H_2 = C_2H_2 + 8(HCl).$$

The experiment succeeded perfectly, and a large quantity of the cuprous acetylide was rapidly produced.

But the most simple and ready means of preparing acetylene was shown to be by drawing air through the flame of a common glass spirit lamp, by means of an aspirator. So readily is the cuprous precipitate obtained by this means, that it suffices to draw a few cubic centimetres through the solution of ammoniacal cuprous chloride to obtain evidence of the presence of acetylene in the flame. In a few seconds the solution became quite thick with the suspended precipitate.

All the homologues of olefiant gas give acetylene in abundance when subjected to the induction spark. Amylene does it readily in accordance with the annexed equation:—

$$\underbrace{2(C_5H_{10})}_{\text{Amylene.}} = \underbrace{5(C_2H_2)}_{\text{Acetylene.}} + 5H_2.$$

That the spark acted only in consequence of its high temperature, the speaker said, is rendered probable by the fact that if hydrogen be passed through carbonic tetrachloride, and then into a globe containing a platinum spiral, when the latter was heated to dull redness by three cells of a Grove's battery, no acetylene was produced; but when five cells were used, and the spiral became white hot, the cuprous precipitate was obtained readily. The same result was stated to occur with amylene.

Simple as the formula of acetylene is, almost all the animal and vegetable substances which have been formed by pure synthesis may be obtained from it.

The following equations represent the steps by which alcohol may be "synthesised." It is proper to premise that the conversion of acetylene into olefiant gas is accomplished by treating the cuprous acetylide with zinc and ammonia, so as to obtain nascent hydrogen:—

$$\underbrace{C_2H_2}_{\text{Acetylene.}} + H_2 = \underbrace{C_2H_4}_{\text{Ethylene.}}$$

$$\underbrace{C_2H_4}_{\text{Ethylene.}} + H_2SO_4 = \underbrace{H, C_2H_5SO_4}_{\text{Ethyl-sulphuric acid.}}$$

$$\underbrace{H, C_2H_5SO_4}_{\text{Ethyl-sulphuric acid.}} + H_2O = \underbrace{C_2H_6O}_{\text{Alcohol.}} + H_2SO_4.$$

The synthesis of succinic acid from acetylene was next shown in accordance with the annexed equations, omitting the synthesis of ethylene, which has been already given:—

$$\underbrace{C_2H_4}_{\text{Ethylene.}} + Br_2 = \underbrace{C_2H_4Br_2}_{\text{Ethylenic dibromide.}}$$

$$\underbrace{C_2H_4Br_2}_{\text{Ethylenic dibromide.}} + \underbrace{2(KCN)}_{\text{Potassic cyanide.}} = \underbrace{C_2H_4(CN)_2}_{\text{Ethylenic dicyanide.}} + 2(KBr)$$

$$\underbrace{C_2H_4(CN)_2}_{\text{Ethylenic dicyanide.}} + 2(KHO) + 2(H_2O) = \underbrace{C_4H_4K_2O_4}_{\text{Potassic succinate.}} + 2(NH_3).$$

This mode of effecting the synthesis of succinic acid is due to the researches of Maxwell Simpson.

The beautiful appearance of succinic acid under the influence of polarised light was shown by the aid of the electric lamp. The specimen used having been artificially prepared.

The speaker then proceeded to show that the synthesis of succinic acid was a direct step to that of tartaric acid. This latter reaction is due to the researches of Perkin and Duppa.

The artificial formation of succinic acid, starting with acetylene, having been proved, it is only necessary to start with that acid to prove the synthesis of tartaric acid from acetylene:—

$$\underbrace{C_4H_6O_4}_{\text{Succinic acid.}} + 2Br_2 = \underbrace{C_4H_4Br_2O_4}_{\text{Dibromosuccinic acid.}} + 2HBr$$

$$\underbrace{C_4H_2Ag_2Br_2O_4}_{\text{Argentic dibromosuccinate.}} + 2(H_2O) = \underbrace{C_4H_6O_6}_{\text{Tartaric acid.}} + 2(AgBr).$$

The lecturer next proceeded to show how the synthesis of the organic alkaloids could be effected from inorganic materials.

In the first place the fact that cyanides can be produced by heating carbon in presence of nitrogen and an alkali, is well known. The next step is to procure prussic acid by distilling cyanides with acids. From pure prussic acid methylamine, the simplest of the primary monamines, can easily be obtained, either by the aid of nascent hydrogen, or free hydrogen in the presence of spongy platinum.

$$\underbrace{CHN}_{\text{Prussic acid.}} + 2H_2 = \underbrace{CH_5N}_{\text{Methylamine.}}$$

This equation has been realised by both the methods given above, the first by Mendius, the second by Debus.

It is also evident that as alcohol can be obtained from acetylene, so all the ethylic, primary, secondary, and tertiary monamines of Hofmann can now be synthetically formed. The steps being (1), conversion of acetylene into olefiant gas. (2). Passage of olefiant gas into alcohol. (3). Alcohol into iodide of ethyl. (4). Action of iodide of ethyl on ammonia.

Again, acetic acid, it has been shown, can be prepared from carbonic disulphide. Now acetic acid, by the action of a red heat, can be made to yield a number of the homologues of olefiant gas. The latter, by treatment with excessively strong hydriodic acid, become converted into the iodides of the alcohol radicals (Berthelot). By following up this last reaction with pentylene, heptylene, octylene, and nonylene, the lecturer succeeded in obtaining pentylamine, heptylamine, octylamine, and nonylamine.

The direct ascent from acetylene to the coal tar colours was then shown according to the following equations :—

$$\underbrace{3(C_2H_2)}_{\text{Acetylene.}} = \underbrace{C_6H_6}_{\text{Benzol.}}$$

$$\underbrace{C_6H_6}_{\text{Benzol.}} + HNO_3 = \underbrace{C_6H_5NO_2}_{\text{Nitrobenzol.}} + H_2O$$

$$\underbrace{C_6H_5NO_2}_{\text{Nitrobenzol.}} + 6(FeO) + H_2O = \underbrace{C_6H_7N}_{\text{Aniline.}} + 3(Fe_2O_3)$$

$$\underbrace{C_6H_5Br}_{\text{Bromobenzol.}} + \underbrace{CH_3Br}_{\text{Methylic Bromide.}} + Na_2 = \underbrace{C_7H_8}_{\text{Toluol.}} + 2\,NaBr$$

$$\underbrace{C_6H_7N}_{\text{Aniline.}} + \underbrace{2(C_7H_9N)}_{\text{Toluidine.}} = \underbrace{C_{20}H_{19}N_3}_{\text{Rosaniline.}} + 3H_2$$

These transformations were all described at length. In effect acetylene passed through a red hot tube becomes polymerised into benzol.

The passage of toluol into nitro-toluol and toluidine is omitted in the above equations, because the reactions are identical in kind with those of benzol into aniline.

In describing benzol, experiments were shown illustrating the density of its vapour as compared with air. In one of these, benzol was poured into a large beaker containing a hot iron; at first the benzol assumed the spheroidal form, but, as the temperature fell, it became converted into vapour, which filled the beaker. A glass syphon was then introduced into the beaker, and the vapour drawn off as if it had been a liquid, and inflamed. The vapour descending through the syphon was then received into a warm beaker, from which it was decanted into another beaker in which it was inflamed.

The speaker then proceeded to show the way in which the synthesis of zinc ethyl could be effected; it is, however, unnecessary to follow the equations in detail, because, having already explained the manner in which alcohol can be synthesised from acetylene, it is obvious that zinc ethyl can be directly derived from that fluid.

Wanklyn's interesting synthesis of acetic acid from sodium methyl was then shown to take place in accordance with the expression—

$$\underbrace{CH_3Na}_{\text{Sodium methyl.}} + CO_2 = \underbrace{C_2H_3NaO_2}_{\text{Sodic acetate.}}.$$

The method appears to be general, inasmuch as the same chemist has effected the synthesis of propionic acid :—

$$\underbrace{C_2H_5N}_{\text{Sodium ethyl.}} + CO_2 = \underbrace{C_3H_5NaO_2}_{\text{Sodic propionate.}}$$

And, substituting carbonic oxide for carbonic anhydride, we have—

$$2(CH_3Na) + CO = \underbrace{C_3H_6O}_{\text{Acetone.}} + 2Na.$$

The speaker stated that one of the most interesting of the cases of synthesis recently accomplished was that in which Mr. W. H. Perkin had succeeded in producing artificially the odoriferous principle of new hay and the tonquin bean.

The delicious fragrance of new hay is entirely due to the presence of the sweet scented vernal grass, *anthoxanthum odoratum*. It is the same substance which is the cause of the sweet smell of the woodruff, *asperula odorata*; the melilot, *melilotus officinalis*. It is also the flavouring ingredient in the *mai wein* of the Germans, which is perfumed with woodruff.

Until lately, nothing was known about coumarin, except that it was a colourless crystalline body, having the formula—

$$C_9H_6O_2.$$

The crystals of coumarin appear very beautiful under the influence of polarised light. The image of some artificial coumarin, which had been fused and allowed to crystallise in a plate of glass, was then thrown upon the screen, and the light being polarised by the aid of Nicol's prisms, the crystals assumed the most gorgeous and varying colours as the prisms were rotated.

The clue to its constitution was shown to be the circumstance that when heated with potassic hydrate it yields salicylic and acetic acids. The production of salicylic acid from coumarin was then shown, experimentally, the presence of the acid being proved by its yielding a deep purple colouration with ferric chloride.

Artificial coumarin was obtained from the hydride of salicyl. By treatment with sodium it yielded hydride of sodium salicyl; this substance, heated with acetic anhydride, gave hydride of aceto-salicyl. This last substance was then distilled with acetic anhydride and sodic acetate, and when the temperature reached 290°, the distillate solidified to a mass of crystals of pure coumarin, having all the fragrance and beauty of that obtained from the tonquin bean.

The speaker submitted that the assertion he had made at an early period of his lecture that there was no natural barrier between organic and inorganic chemistry, had been amply proved by the instances he had brought forward. He said that they had studied together that evening several cases where, starting from inorganic matter, they had ascended step by step until they had reached some of the most complicated bodies secreted by animals and vegetables. What, he said, could be more distinctly inorganic than nitrogen, carbon, and oxygen? What more distinctly an animal secretion than urea? What more completely inorganic than acetylene? What more distinctly vegetable in origin than coumarin?

Chemists have then, so far, done what a very few years ago would have been regarded as possible only by the aid of the vital force. A true organic substance, he said, is so definite that we can almost invariably determine its molecular weight, and it is generally crystalline. But when we come to the tissues we are dealing no longer with organic substances, but with organised beings, and we feel that we are approaching the barriers which separate the study of life from the study of matter. The bonds which unite them are so close that we cannot imagine life *without* matter, and it is equally difficult to conceive the assumption of vitality *by* matter, but we must never cease to look anxiously for the solution of the problem. The impossible is a horizon which recedes as we advance, and the *terra incognita* of to-day will to-morrow be boldly mapped upon every schoolboy's chart!

Artificial Gems.—The base of these gems, as patented by the superintendent of the Royal Porcelain Works at Berlin, is a flux obtained by melting together 6 drachms of carbonate of soda, 2 drachms burnt borax, 1 drachm saltpetre, 3 drachms minium, and 1½ ounces of purest white sand. To imitate in colour, but of course not in composition, the following minerals, add to the flux the ingredients named in connection with each gem :—*Sapphire.*—10 grains carbonate of cobalt. *Opal.*—10 grains oxide of cobalt, 15 grains oxide of manganese, and from 20 to 30 grains protoxide of iron. *Amethyst.*—4 to 5 grains carbonate of peroxide of manganese. *Gold Topaz.*—30 grains oxide uranium. *Emerald.*—20 grains protoxide of iron, 10 grains carbonate of copper.

CHEMICAL CONSTITUTION AND ITS RELATION TO PHYSIOLOGICAL ACTION.*

By Dr. A. CRUM BROWN, F.R.C.P. Edin., &c.

CHEMICAL *composition* is an expression which has a perfectly definite meaning. By it we understand the portions of the elements (or hitherto undecomposed substances) which can be obtained from a substance, or from which it can be built up.

In investigating the composition of a substance it is a matter of secondary importance *how* it has been decomposed or *how* it may be reconstructed; what we wish to know is, what are its ultimate constituents, and how much of each it contains. The knowledge of the intermediate steps is only valuable so far as it is necessary to prove that we have lost nothing and added nothing. It is quite different when we come to consider chemical *constitution*. Here we have to deal not only with the nature and quantity of the constituent elements, but with their relations to each other in the compound. The difference between composition and constitution may be illustrated by very many analogies. I shall make use of a familiar one. The *composition* of a household is expressed by the number of persons contained in it, with the sex and age of each, but its *constitution* is not known, until we have the relations in which they stand to one another, as husband and wife, parents and children, master and servant, &c.

If we assume the existence of atoms, we may define constitution as the relation of the atoms to one another in the compound, just as the constitution of a family is the relation of the individuals composing it to one another. We have now to inquire, how these relations may be discovered, and how they may be expressed. We discover them by examining the way in which the compound is built up or decomposed, and we express them by means of "rational" or "constitutional" formulæ. In the case of the family alluded to above, we should know the constitution if we knew the history, that is, if we knew by what steps these particular individuals came to live together in one house; and in the same way, the history of a compound, or the steps by which it was produced from its elements, leads us to a knowledge of its constitution. As, however, the work of destruction is generally easier than that of construction, we are often obliged to take the course of gradually, so to speak, picking the compound to pieces, and learning its history backwards.

It would be out of place for me here, even did time permit, to give detailed examples, showing how the constitution of particular substances may be determined; a very clear account of the subject will be found in the article, "Formulæ, Rational," by Professor G. C. Foster, in Watts's "Dictionary of Chemistry." It is sufficient here to indicate some of the general results. We believe, then, that the atoms in a compound are related to one another in such a way that atom is united to atom, and that when two groups of atoms unite together, it is by individual atoms of the one uniting with individual atoms of the other. We find that the atoms of some elements can each enter into only one relation, those of some into two, those of some into three, &c; such atoms are called monatomic, dyatomic, triatomic, &c., respectively; if we prefer English to Greek names, we may call them singly-related, doubly-related, and so on. We find, further, that an atom which in one compound is singly-related, becomes in another trebly- or fivefold-related, and that a doubly-related atom may, in the same way, become fourfold- or sixfold-related; the change in the number of relations (what is called the change of "atomicity") taking place, in the vast majority of cases, by two at a time, so that an atom which has an odd number of relations in one compound has an odd number in all, and may, therefore be called *oddly-related* ("perissatomic"), and an atom which has an even number in one, has an even number in all, and may be called *evenly-related* ("artiatomic").

These relations have been expressed in a great variety of ways; most completely by the system of "graphic formulæ," first introduced by Professor Kekulé in 1859. These graphic formulæ have received many modifications; in one of these, which I proposed in 1861, each atom is represented by a circle containing the symbol of the atom, and the relations by lines proceeding outwards from the circumference and connecting the circle with others. Such graphic or pictorial representations have not unfrequently been objected to as expressing more than we know, and as leading readers to imagine that they show the relative *position* of the atoms, of which, of course, we are entirely ignorant. I think I shall be able to show you that this accusation is unfounded, and, at the same time, illustrate the meaning of chemical constitution, by applying the same graphic method to a case where the idea of position in space does not enter at all. Let us suppose that John is occupant of a house which belongs to Mr. Smith, and is situated in Edinburgh, and that Thomas occupies a house belonging to Mr. Brown in Glasgow; then representing John by J, his house by E, Smith by S, Thomas by T, his house by G, and Brown by B, we may express their relations thus:

```
S       B
|   and |   without meaning
E—J     G—T
```

that the landlord is situated above and the tenant at the side of the house. Now if it is convenient for John to go to Glasgow and Thomas to Edinburgh, they may change houses, and we have

```
S       B             S
|   and |   becoming  |
E—J     G—T           E—T

B
|
G—J.
```

Here we have two groups, one in Edinburgh and one in Glasgow, before the change and after it. The only difference is, that we have John and Thomas each in the position previously occupied by the other. These two men may be totally unlike in every other respect, but they are capable of replacing each other in this particular relation. So in chemistry; when chloride of sodium and nitrate of silver are brought together, the silver and the sodium change places, each taking up the relations previously held by the other. This is an example of what is called double decomposition.

Now let us suppose a slightly more complicated case. Let John be proprietor as well as occupant of his house.

```
    B
    |
E = J
    |
    G—T.
```

Here we have E = J and In this case John is doubly-related and united by both bonds to the house. If he *now* change with Thomas, the state of matters will be represented thus:

```
    B
    |
G—J
|
E—T.
```

Before the change *two* groups, but only *one* after. The corresponding chemical reaction is termed addition, and groups to which addition can be made in this way are said to be *condensed*. The condensation obviously consists in the atoms being united together by more bonds than are necessary to ensure the unity of the group. Thus J and E are united by two bonds, while one would be sufficient. We have an instance of this kind of addition in the union of chlorine and olefiant gas. The two carbon atoms of olefiant gas are doubly-related to one another; by the action of chlorine one of these relations is broken, and each carbon atom becomes related to a chlorine atom.

Addition may take place in another way, namely, by an increase in the number of relations of an atom; thus in ammonia, nitrogen is trebly-related, being united to three atoms of hydrogen, but when ammonia is brought into contact with hydrochloric acid, chloride of ammonium is

* Read before the Pharmaceutical Meeting, Edinburgh.

formed, the nitrogen becoming fivefold-related, being united to four atoms of hydrogen and one of chlorine. We may illustrate this by supposing that John, who is already doubly-related, borrows £1000 from Robinson, and buys another house, thus entering into *two* new relations, one to his creditor and one to his investment. Now if we offer Robinson a better investment, he will call up his money from John, who must either borrow from some one else or sell one of his houses. Both cases occur in chemistry; if we mix caustic potash with chloride of ammonium, we offer, so to speak, the better investment of potassium to the chlorine, which accordingly leaves the nitrogen, but the nitrogen, at the same time, loses one atom of hydrogen, just as John parted with his creditor and one of his houses together; but if we try a similar experiment with the chloride of a complex ammonium (such as chloride of tetramethylammonium), mixing it, say, with moist oxide of silver, and thus tempting the chlorine to leave the nitrogen and unite with silver, the nitrogen does not lose a methyl atom long with the chlorine, but, if I may use the expression, pays off the chlorine by borrowing from oxygen. This analogy might be carried much further, but it is unnecessary to do so. My object is attained if I have shown that *any* kind of relation which can be single, double, treble, &c., may be used to illustrate chemical constitution, and that we are not necessarily restricted to geometrical relations.

Our time does not allow me to dwell longer on this part of the subject; I shall, therefore, proceed to the consideration of the relation between chemical constitution and physiological action.

The difficulty of determining this relation depends mainly on our ignorance of the constitution and action of the great majority of substances, which makes it impossible for us to make much progress by direct comparison of constitution with action. All that has been made out in this way is, that the soluble compounds of what we may call a poisonous element, such as lead, have all nearly the same action, and similarly of a poisonous radical, such as cyanogen. Here, however, we meet some remarkable exceptions; thus, kakodylic acid, although readily soluble, and containing a large amount of arsenic, is inert, and a number of the double cyanides, such as ferrocyanide of potassium, show no trace of the action of hydrocyanic acid.

In this difficulty it occurred to Dr. Fraser and myself, that another method of inquiry might lead to better results. The method which we have adopted consists first, in introducing a known change into the constitution of an active substance; and, second, in comparing the action of the new product so formed with that of the original substance.

The first set of substances to which we applied this method (in an investigation communicated to the Royal Society of Edinburgh, January 6, 1868) was the group of vegetable alkaloids, and we have treated in this way strychnia, brucia, thebaia, morphia, codeia, and nicotia. Each of these alkaloids contains a trebly-related atom of nitrogen, which can easily be made fivefold-related. We do not know what its original three relations are, but we know what the two new ones are which we introduce. Thus, confining our attention to strychnia (and what is said of it applies with slight modification to the others), we find that a molecule of that alkaloid contains two atoms of nitrogen; of the connections of one of these we know nothing, of the other we know that it is trebly-related, and that we can make it enter into two new relations. We can do this by uniting it to an acid, hydrochloric acid for instance, in which case the new relations are, one to hydrogen and one to chlorine; to use the old metaphor, it borrows from chlorine and invests in hydrogen. We cannot, however, compare the action of strychnia with that of its hydrochlorate, for a very slight inducement is all that is necessary to make chlorine call up its investment, and retire with an atom of hydrogen. To try the effect of addition we must add two new relations of a more stable kind. This is done by acting on strychnia, not with an acid, that is a compound of hydrogen, but with an ether or compound of methyl or ethyl, such as iodide of methyl or iodide of ethyl. Here the nitrogen borrows from iodine and invests in methyl or ethyl, and the resulting compound is so stable that it has no tendency to recur to its former state. These peculiar compounds were first studied chemically by How, and afterwards by Stahlschmidt. The latter made some experiments with methyl-strychnium salts on rabbits, and came to the conclusion that they were quite inert. A more complete examination has shown that this is not exactly true, but that a still more remarkable change has been produced. Large doses (thirty grains) of the iodide of methyl-strychnium (the compound of strychnia with iodine and methyl) produce no effect whatever when administered to a rabbit by the stomach; fifteen grains, however, kill a rabbit when injected under the skin. In this case the symptoms are quite unlike those of strychnia poisoning; instead of violent tetanic convulsions, we observe a condition of general paralysis. Now it is important that we should know *how* this paralysis is produced, and for this purpose we had recourse to a method which has been very successfully applied to the study of various poisons, especially "urali" or "curare," the South American arrow poison. To make this method of experimenting intelligible, I shall compare the nervous system to a set of telegraph-wires and offices. Let us suppose that every town in the country has two telegraph offices, and that each is connected by a wire with the head office in London, one of the wires is used only for sending messages up to London, the other only for sending messages down, and there is no means of telegraphic communication between one country town and another, except through London. Let us further suppose that whenever a message from London is received in any town the town bell is rung; now if we give in a message at the Manchester office, intending it to go through London to every town in the country, and find that no bells are rung, it is obvious that something is wrong; it may be, first, that the apparatus at Manchester is out of order, so that the message was never sent; or, second, that the up wire from Manchaster was broken or not insulated, so that the message never reached London; or, third, that there was something wrong at the head office; or, fourth, that all the down wires were broken or not insulated; or, fifth, that something was wrong at all the receiving offices throughout the country; or, sixth, that the bells could not be rung. When a healthy frog is pinched anywhere the animal jumps, a message is sent from the part of the skin pinched to the nerve-centres, and thence to all the muscles; but if we give the frog iodide of methyl strychnium a pinch produces no effect. As in the supposed telegraphic case this result may be due to one of six causes,—first, the ends of the sensory nerves may be injured, so that the message is never sent from the place pinched; or, second, the sensory nerve-trunks may be paralysed, so that the message does not reach the nerve-centres; or, third, the nerve-centres, may be so deranged that they do not send it on to the motor nerves; or, fourth, the motor nerve-trunks may be paralysed; or, fifth, the terminations of the motor nerves may be injured, so that they do not communicate the stimulus to the muscular fibres; or, sixth, the muscular fibres themselves may be incapable of contracting.

To return to the supposed telegraph case,—if we could ensure that the whole apparatus within some one county at a distance from London, say Cumberland, were in good order, and found that the bells rung in Cumberland, and nowhere else, when a message was sent from Manchester, we should know that the mischief was not in the apparatus for sending the up message, nor in the up wires, nor in the head office, nor in the down wires, for to reach Cumberland the message had to pass through districts where the injury existed; the fault must be either at the receiving offices or in the bells. If now we can go directly to the

bells throughout the country, and find that they can be rung, we prove the fault to lie at the receiving offices. In the case of the frog, we can, by trying the artery of one leg, prevent the poison from reaching that leg, which thus represents Cumberland in the case above; if the frog be now pinched *anywhere* he kicks with the unpoisoned leg, proving, as above, that the poison does not act on the terminations of the sensory nerves, nor on the trunks of the sensory nerves, nor on the nerve-centres, nor on the motor nerve-trunks, but that its action must be either on the terminations of the motor nerves, or on the muscular fibres. But we find that the muscles contract when directly stimulated, showing that they are not injured, and we thus prove that the paralysis is produced by a destruction of the power of the terminations of the motor nerves to receive the stimulus and transmit it to the muscles.

Strychnia produces tetanic convulsions, by exciting the nerve-centres in the spinal cord, but the substances formed by rendering the trebly-related nitrogen atom of strychnia permanently fivefold-related produce paralysis, and do so in the very remarkable way described above. Not only is this the case with strychnia, but we have found that the same change is produced in every alkaloid we have examined, which has an action like that of strychnia.

It will be seen that we have as yet made but little progress towards the solution of the very important question, What is the relation between chemical constitution and physiological action? We may, however, reasonably expect that a series of investigations, such as that sketched above, will throw a good deal of light on the subject.

ON THE RELATION BETWEEN

THE MAGNETISM OF SOME METALS,

AND THEIR

ATOMIC AND SPECIFIC WEIGHTS.*

By P. H. VAN DER WEYDE, M.D.

WHEN we divide the specific gravity of the different metals respectively into their atomic weights, we obtain quotients which indicate, not directly, but relatively, the distance of their atoms; (upon the supposition that the atomic weights indicate really the relative weights of their atoms, which is only probable, but not proved): comparing those quotients in the subjoined table, we find the following remarkable results:—

	Sp. Gr.	Atomic Weight.	Quotient.
Cobalt	8·5	30·	3·53†
Iron	7·8	28·	3·59‡
Chromium ..	6·8	26·	3·82‖
Nickel	8·	31·	3·90§
Manganese ..	7·	27·6	3·94¶
Palladium ..	11·8	53·	4·49
Platinum ..	21·5	99·	4·57
Zinc	6·8	32·5	4·78
Aluminium ..	2·56	13·7	5·35
Iridium	16·	99·	6·2
Cadmium ..	8·7	56·	6·4
Magnesium ..	1·74	12·	7·
Mercury	13·5	100·	7·47
Lead	11·4	104·	9·12
Osmium	10·	100·	10·
Gold	19·4	196·	10·005
Silver	10·47	108·	10·3
Lithium	0·593	6·4	10·9
Antimony ..	6·7	130·	19·4
Bismuth	9·8	208·	21·22

* *American Journal of Mining.*
† Remains paramagnetic at white heat.
‡ Is only magnetic below bright red heat.
‖ Is only magnetic below dark red heat.
§ Is only magnetic below 600° Fahr.
¶ Is only magnetic below 4° Fahr.

Observations.

1st. The five magnetic metals have all quotients below 4.

2nd. The so-called non-magnetic metals have all quotients above 4.

There is, however, one exception to this rule, in the case of copper, of which the respective specific and atomic weights are 8·8 and 31·7, of which the quotient is 3·602; but then it is probable that the atomic weight of copper needs correction, and should be doubled to 63·4, in which case the quotient would be 7·204, and it would then fall among the other non-magnetic metals.

3rd. The quotients are the smallest for those metals which are the most permanently magnetic, even at high temperature, and *vice versa*.

4th. As cooling increases by contraction the number expressing the specific gravity, it will consequently decrease the quotient obtained by using this increased specific gravity as a divisor, in perfect accordance with the fact that cooling increases the paramagnetic property.

5th. As, inversely, heating decreases by expansion the specific gravity, it will increase this quotient, in accordance with the fact that heat diminishes paramagnetism, and finally destroys it in all metals with the single exception of cobalt, which has the smallest quotient of all, and consequently can stand some increase.

6th. The experiments of Faraday on diamagnetism and paramagnetism, with very powerful electro-magnets, have proved that palladium and platinum are the strongest paramagnetic next to the first five in the above list; they have in my list, also, the smallest quotients connected with them.

7th. In the same way as diamagnetism is the opposite of paramagnetism, the larger quotients in the above table belong to diamagnetic bodies, as, for instance, mercury, antimony, and bismuth. The last is the strongest diamagnetic substance experimented upon, and possesses the greatest quotient in the above table.

8th. If we were able to cool the other metals so as to increase their specific gravities to such a degree as to have a decided effect on the amount of this quotient, we might perhaps succeed in discovering in several of them paramagnetic qualities, by means of Faraday's apparatus.

9th. Heating decreases the paramagnetic qualities, with the specific weight, and consequently increases the quotient. That it may do this to such a degree as to make the body diamagnetic, is proved in the case of oxygen gas, which, when cool is paramagnetic like iron, and when hot, diamagnetic like bismuth.

10th. That this relative distance of the atoms (upon which, of course, the specific gravity of bodies depends) is closely related to their magnetism, is again proved by their crystals, which are always less dense in the direction of their optical axis, and expand by heat more in one direction than in another; and Pluecker has demonstrated that they are diamagnetic in the direction of their optical axis, or of the longest axis of crystallisation. In some of these crystals, this action is so strong that they are influenced by the magnetism of the earth; as, for instance, a properly cut crystal of kyanite (a dense silicate of alumina) when suspended on an axis, will behave like a compass needle, and may be used as such, a fact little known, but worth knowing.

Hydrocarbons of the CH_{2n} Series.—A. Butlerow has compared the reactions of the propylene derived from allylic iodide and iodhydric acid with those of the propylene from amylic alcohol, and arrived at the conclusion that they are identical, their composition being represented by the formula $\begin{cases} CH_3 \\ CH \\ CH_2 \end{cases}$ —(*Zeitschr. Chem.*, N.F. iii., 682.)

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, June 4.

Dr. WARREN DE LA RUE, F.R.S., &c., President, in the Chair.

THE minutes of the previous meeting were read and confirmed, and the donations to the library announced. Messrs. G. W. Brown and James Richards were formally admitted Fellows of the Society; and the following gentlemen were balloted for and duly elected, viz.:—Henry Chance, M.A., glass manufacturer, Oldbury, near Birmingham; William Hustler, mining engineer, Rosemerryn, Falmouth; and William B. Miller, assayer, Royal Mint, Sydney.

There were no new candidates proposed. For the second time were read the names of the following gentlemen, viz.:—Samuel Alexander Sadler (at Messrs. Chance's Chemical Works), Oldbury, near Birmingham, and H. B. Riddel (late of the Indian Civil Service), 9, Stratton Street, Piccadilly.

Mr. E. T. CHAPMAN gave a short account of the reactions more fully described in three papers presented to the Society, of which Mr. Miles H. Smith and himself were joint authors.

I. "*Action of Chloride of Zinc on the Oxalic Ethers.*" Having sketched the chemical changes which were possible under these circumstances, the authors show that the action of the metallic dehydrating agent upon the oxalates of ethyl, amyl, and methyl, results in the formation of oxalate of zinc (left as a residue in the retort), but instead of obtaining the chlorides of these radicals as the direct result of a double decomposition, they are split up into hydrochloric acid and the olefines, or a mixture of bodies of the latter class; only in the last-named instance was a small quantity of chloride of methyl produced.

II. "*On the Artificial Production of Pyridine.*" Referring to Mr. Perkin's researches on this subject, and to a previous speculation as to the possibility of inducing the formation of artificial pyridine (*Chem. Soc. Jour.*, August, 1866), the authors now show that this body may be produced in small quantity by the action of anhydrous phosphoric acid upon the nitrate of amyl. The principal product of the reaction is a dark brown pitch-like substance, but a sufficient amount of pyridine was obtained upon distilling with potash, to permit of its identification, and of an analysis being made of its chloride, C_5H_5N,HCl.

III. "*Isomerism in the Organic Cyanides.*" On a review of Dr. A. W. Hofmann's researches on the isomeric cyanides, it appeared to the authors desirable to attempt the formation, and study the characters of compounds produced by the action of dehydrating agents, such as anhydrous phosphoric acid, and fused chloride of zinc, upon the formamides obtained by acting with formic ether upon ammonia, ethylamine, aniline, and other amines. The products of these reactions were the so-called pseudocyanides, and were at once recognised by their peculiar odour, and by the decompositions they underwent upon treatment with acids. This method of proceeding cannot be considered available for the preparation of these cyanides in large quantity, and an attempt to obtain analogous bodies from the compound acetamides did not prove successful.

Dr. B. H. PAUL then gave an account of "*The Modes of Testing Mineral Oils used in Lamps*," in which he showed that the results obtained as indicating the degree of inflammability of this oil were subject to considerable variation according to the mode in which the test was applied. It was also shown that there was a difference of opinion as to what should be regarded as the firing point, whether it was to be the temporary firing of the first small portion of oil vapour given off or the permanent firing of the oil itself. Between these two results there might be a difference of from ten to twenty degrees Fahrenheit, according as the oil was tested in a shallow open basin, or a partially closed vessel. In reference to the influence of the degree of inflammability of mineral lamp oil on the possibility of accidents occurring in the use of this material, it was shown that the mere volatility of the oil was not the only point to be considered, and that, apart from misuse and carelessness, the construction of the lamps in which it was burnt was of considerable importance. To illustrate this, a lamp filled with what is commonly known as spirit or naphtha (the most volatile portion of petroleum or paraffin oil), was kept burning during the meeting. This lamp was so constructed, that there was no communication between the flame and the oil reservoir, except through the tube containing the wick, and consequently there was no chance of oil vapour or an explosive mixture of it with air coming in contact with the flame, so as to cause accident. Other kinds of lamps in which there is free communication between the oil reservoir and the flame, afford less security, especially when the oil used in them vaporises at a low temperature.

The mode of testing oil, and the possible variation of the results obtained under different conditions were illustrated by experiments; and an apparatus devised by Mr. Rippingille, of the Albion Lamp Company, was shown. It consisted of a small copper vessel like a paraffin oil lamp fitted with a thermometer, and a valve, and two wires, by which an electric spark could be produced inside the vessel. By gradually heating the oil to be tested in this vessel, and passing the spark from time to time while the temperature was observed, a point was at length reached when there was a slight explosion, and this was taken as the firing point of the oil.

In conclusion, the author pointed out that whatever degree of inflammability was fixed upon as the proper minimum for safety, the mode of testing should be precisely defined, and the experiment should always be made under like conditions.

A short discussion followed, in which Messrs. Dugald Campbell, E. T. Chapman, and Dr. Attfield took part. The general conclusion arrived at was that the danger from explosion had been greatly exaggerated, and that when such events occurred they might sometimes be traced to faults of construction in the lamp. It was generally agreed that an oil which does not give off inflammable vapour below 100° Fahr., should be accounted safe, but no precise figure below this point on the thermometric scale was adopted at the meeting, although Mr. Dugald Campbell, amongst other speakers, pressed for a decision to support his own judgment in the matter.

The PRESIDENT, in moving a vote of thanks to Dr. Paul, took occasion to support his view of the case, namely, that the construction of the lamp was as important as the firing point of the petroleum, and that the dangers of explosion had been greatly overrated.

The meeting was then adjourned until Tursday, the 18th instant, which will be the last during the present session.

Carbolic Soap.—The beneficial effects experienced by the use of the medical carbolic soap, have led the patentees, Messrs. McDougall, Bros., to introduce it in a form suitable for the toilet. The antiseptic properties of carbolic acid are well known to our readers, and the form in which it enters into the composition of this soap renders its use pleasant, refreshing, and perfectly harmless. We have no doubt that as the soap becomes known it will be more appreciated, and command an extensive sale. Messrs. McDougall, Bros., are also manufacturing a common carbolic soap for household use, which is valuable for cleansing purposes, and also to prevent contagion.

FOREIGN SCIENCE.

PARIS, JUNE 10, 1868.

New polarising apparatus.—Testing colouring matters.—Ferric hydrate and ferric chloroxide. ACADEMY OF SCIENCES: New substance replacing magnesia in the oxyhydrogen light.—Estimation of phosphates by conversion into phosphides.

A NEW polarising apparatus has been constructed by M. Hempel, from a suggestion by M. Plucker, the eminent physicist and mathematician of Bonn. The apparatus, which, in the majority of its parts is similar to one already known, is made up as follows:—(1) a black glass, usually horizontal, but capable of being more or less inclined, and upon which the incident ray is polarised rectilineally by reflection; (2) a disc or ring destined to carry and maintain, in the path of the reflected polarised ray, the transparent plate which is to exhibit chromatic polarisation; (3) a convex lens which renders the ray leaving the transparent plate parallel or convergent; (4)—and this is the addition which gives to the instrument a perfection scarcely expected—a parallel glass silvered on the exterior surface, and fixed in a support at such an inclination that it leads to the vertical the doubly reflected and transmitted ray, which it directs to the Nicol's prism, serving as analyser.

Referring again to M. Nicklès' lecture "On the new fluosalts and their uses," noticed last week, your correspondent finds some points of interest that ought not to be omitted. M. Nicklès showed how the various colouring matters used in industry might be detected. For example, commercial substances often owe their colour to "artificial blood;" knowing that sulphocyanides are poisonous, one might wish to be certain whether sulphocyanide of iron had been used in this case. A drop of fluoride of potassium discharges the colour from the iron compound immediately. Similarly, looking at a fabric dyed blue, one might wish to ascertain whether the colour belonged to the anilines, indigo or prussian blue; the two first being unacted upon by the test solution, the fabric would receive a drop of this solution, and then be subjected to a jet of steam. Inks, of course, may be examined in the same way. Those having indigo-carmine for the base, become redder with the fluoride of potassium solution.

In a memoir presented a short time back to the Academy, M. Jeannel remarks "why certain varieties of hydrated sesquioxide of iron dissolve easily in acids, dissolve incompletely, or give unstable salts, is ignored; all that is known is that calcination is an absolute condition of insolubility." He then states that he believes to have found the cause, or at least the most frequent cause of these variations; the hydrated sesquioxide of iron is more or less insoluble in acids, and gives salts more or less unstable, when prepared from substances containing sulphates. Sesquioxide precipitated from the persulphate is always to a certain extent insoluble, or yields unstable salts; the same is the case with the sesquioxide precipitated from the perchloride, when this has been prepared with acids contaminated with sulphuric acid, or equally when the alkalies employed as precipitants have been so contaminated, or, finally, when the ferric hydrate precipitated from pure solutions by pure alkalies has been washed with common water, which chemists need not to be told nearly always contains a little earthy sulphate. Ferric hydrate prepared from materials rigorously free from sulphates, and in vessels rinsed with distilled water, is extremely soluble in the cold in very dilute acids: notably, it dissolves with surprising facility in the officinal solution of perchloride of iron. A new compound, indefinitely soluble, which might be named ferric chloroxide, is easily obtained in solution or in the solid state. This compound is represented by the perchloride of iron, Fe_2Cl_3, and an indeterminate quantity of sesquioxide of iron, Fe_2O_3. M. Jeannel has prepared, in the cold, a stable aqueous solution of ferric chloroxide, which may be represented by the formula $Fe_2Cl_3, 9Fe_2O_3$, and consequently contains nine times as much iron as the neutral or officinal solution. This solution of ferric chloroxide might perhaps be found specially advantageous in checking hæmorrhage. The solution possesses in the highest degree the property of coagulating albumen, and removing albuminoid and colouring matters. A few drops thrown into water produce a very voluminous brown precipitate. The solution of ferric chloroxide is decomposed and precipitated by very small quantities of sulphuric acid, or of sulphates soluble or insoluble. It is likewise decomposed by citric, or tartaric acid, and curiously, or rather surprisingly, it is decomposed by a few drops of concentrated hydrochloric or nitric acids.

The following memoirs of chemical interest were communicated to the Academy on the 21st ult.:—"On the composition of the gaseous mixture used in the oxyhydrogen light, and on a new matter replacing magnesia," by M. Caron; "Estimation of phosphoric acid by the transformation of the phosphates into phosphides of iron," by M. Schlœsing; "Researches on the combustion of oil," by M. Scheurer-Kestner; "Researches on the bleaching of tissues," by M. Kolb; "On the dilation of solid bodies by heat," by M. Fizeau.

The magnesia pencils, made either by compression or by the wet method, cannot, M. Caron says, resist indefinitely the intense heat produced by the combustion of coal-gas mixed with oxygen. It would likewise be difficult to use magnesia with pure hydrogen and oxygen, which give rise to a more intense heat, and consequently greater corrosion. Therefore, he examined other substances. Knowing the infusibility of silicate of zirconium, M. Caron tried this material, but found the light emitted by the pulverised and agglomerated zircon very small, the case with most silicates. There remained the earth zirconia yet. According to Berzelius, this earth is infusible, and emits a brilliant bluish light in the blow-pipe flame. This M. Caron has tried, and to him the earth does not seem volatile in the oxyhydrogen flame. The same pencil used daily for more than a month, heated upon a sharp angle, shows no trace of volatilisation, partial reduction, or loss of any kind. This fact is important, for with a lamp burning only a feeble jet of gas, the part of the flame which gives the light is very small, and the incandescent matter must of necessity always remain at the same distance from the point; as the pencil becomes used, this distance augments, and the light diminishes more and more. M. Caron says, the employment of zirconia seems to him to lead to a notable improvement in the oxyhydrogen light, for besides the valuable quality of remaining intact, zirconia possesses luminous properties even greater than those of magnesia, the proportion being nearly as 6 : 5. Zirconia, it is true, is infinitely rarer than magnesia, but it is found in many volcanic sands, and especially in great abundance in the zirconic rocks, near Miask in the vicinity of Ilmensec, at the foot of the Ural. This rare earth, too, can be easily economised, it need only be employed where the flame impinges, the remainder being magnesia or refractory clay; compression will fasten the two materials together, and baking add to the solidity of the pencils.

M. Schlœsing's memoir deserves a somewhat detailed account. This chemist described in 1864 a process for estimating phosphoric acid, based upon the reduction by carbonic oxide, at a high temperature, of phosphates placed in contact with silica; the phosphorus was collected upon copper or in a solution of nitrate of silver. When the phosphates contain oxide of iron, this process ceases to give exact results. Obliged to accept its presence in manures, vegetables ashes, &c., M. Schlœsing decided to make the iron perform a part in a process. Alkaline and earthy phosphates heated to whiteness in a carbon crucible, with silica and iron, cede to the metal the whole of their phosphorus. This transformation being effected, the phosphorus requires only to be separated from its combination with iron. The task is not as easy as it at first sight appears to be. Acids are not admissible. When

dry chlorine passes at a moderately elevated temperature over iron containing phosphorus and some other metalloids, arsenic, sulphur, and silicium, all the bodies are converted into chlorides; this fact is well known. The chloride of iron is less volatile than the others, but not sufficiently so to permit of accurate separation; with the phosphorus, the difficulty is augmented by the formation of a combination between its chloride and the chloride of iron. M. Schlœsing has been enabled to destroy this combination by the use of chloride of potassium, which at the same time greatly lessens the volatility of the chloride of iron. At the temperature at which the experiment is made, the whole of the chloride of phosphorus is disengaged absolutely free from the metallic chloride. The process is minutely described; it will be given in my next letter. Experiments showing the accuracy of the process are appended; in the following figures, the first in each case represents the number required by theory, the second that found—43·74 mg. 45·70; 47·9 mg. 47·5; 3·32 mg. 3·50. Results which testify to the value of the process.

NOTICES OF BOOKS.

The Stock-Feeder's Manual: the Chemistry of Food in relation to the Breeding and Feeding of Live Stock. By Charles A. Cameron, Ph.D., M.D. London and New York: Cassell, Petter and Galpin. 1868.

The basis of this work, we learn from the author's preface, consists of some papers on the Chemistry of Food, read before the Royal Agricultural Society of Ireland and the Athy Farmer's Club, and a few articles on the Management of Live Stock, published in the *Weekly Agricultural Review*. It describes the nature of the food used by the domesticated animals, explains the composition of the animal tissues, and treats generally upon the important function of nutrition. The most recent analyses of all the kinds of food usually consumed by animals of the farm are fully stated; and the nutritive values of those substances are in most instances given. Some information is afforded relative to the breeds and breeding of live stock; and a division of the work is wholly devoted to the consideration of the economic production of *meat*, *milk*, and *butter*. Our readers will see from this brief outline of the contents that Dr. Cameron has produced a book likely to interest a large and increasing class of the community, and a careful perusal of its pages will show that it is one which may be read with interest by the mere consumer of the three staple articles of food above named, as well as studied with profit by the producer.

The author is thoroughly practical in his treatment of the subject. He regards an animal simply as a mechanism by which meat is to be manufactured; and he draws the reader's attention to five economic points:—The first cost of the mechanism, the expense of keeping the mechanism in working order, the price of the raw materials intended for conversion into meat, the value of the meat, and the value of the manure. In proportion to the attention given to these points will be the feeder's profit.

The chemistry of food, and the value of different kinds of meat are gone into very fully. The evils resulting from over-fattened, over-driven, or positively diseased meat, are discussed at length; and here Dr. Cameron, in his capacity of analyst to the City of Dublin, is able to speak from his own experience to the large amount of meat, unfit for human food, which is brought to our large markets; 30,000 lbs. weight of bad meat being the probable quantity offered for sale in London alone every week, 2,000 lbs. of which are condemned by Dr. Letheby in the limits of his jurisdiction.

CORRESPONDENCE.

SCIENCE TEACHING IN SCHOOLS.

To the Editor of the Chemical News.

Sir,—The remarks that have lately appeared in the pages of the Chemical News, by Messrs. Rodwell, Johnson, Oscar Browning, Tomlinson, and Bloxam, on the subject of science teaching in schools, will have been read with interest by many who, like myself, are engaged in this important work. As I believe that few things are more likely to lead to the formation of definite opinions as to the possibilities and impossibilities of the various schemes that have been suggested, than a comparison of notes and interchange of ideas amongst those who are so engaged, I propose to say a few words on some of the statements advanced by the writers referred to, and then to give, very briefly, an outline of what we are doing at Rugby, stating some of the results at which we have arrived.

In the first place, then, I fully agree with Mr. Tomlinson's remark, that boys are good observers, but bad theorists. I believe that it is essential to success to realise this. And it is those alone who have had considerable experience with boys who seem able to do so. My own experience extends now over fourteen years, and the last few years have more than ever convinced me of the necessity of admitting this as an established fact. Holding this view, I cannot but think that the prominence Mr. Rodwell assigns to theory in chemistry would be fatal to success with the majority of boys. Of course with senior and advanced pupils we may go as far into theory as we like, and in a large school there will probably be some who are quite fit for reading of this kind: but I am speaking of lecturing on chemistry to classes composed of ordinary schoolboys, with no special aptitude for the subject. Mr. Rodwell asserts "that to thoroughly master the vibratory motion theory, as applied to the explanation of chemical phenomena, is better, as a mental exercise, than the knowledge of fifty of the applications of chemistry;" and he goes on to say that "science applied to the useful arts—to the making of dyes, the extraction of metals, the manufacture of coal gas—has ceased to be pure science." I fully and entirely agree with Mr. Rodwell that chemistry is not to be taught to boys *because* it admits of such applications, and so has a kind of money value; but, at the same time, I can only say that, in lecturing on aluminium, for instance, I should be sorry to lose the additional interest that I always find given to the subject by showing, and that experimentally, the application of the peculiar properties of alumina to the processes of dyeing and calico printing. And so again with regard to coal gas and the coal-tar colours, as a sequel to a lecture on carbon. I have not the slightest sympathy with so-called popular science. I believe science to be quite as hard as Latin and Greek, and to require at least as great an exercise of the mental powers to master it. But for all this, I do not see why the more important practical applications of science are to be thought unworthy of a place in the lecture-room.

Next, with regard to method of teaching. Mr. Johnson suggests that, as a lecturer on science can hardly be expected to undertake the drudgery of drilling his boys in elementary notions, meaning of terms, &c., his lectures should be supplemented by another master, who would be contented "to go over the ground again, and pick out the weeds that the lecturer had left behind." Without saying anything of the improbability of finding many men competent and willing to do this not over exciting work, surely it ought, in itself, to be unnecessary. No lecturer ought to get on faster than his pupils can follow him, and he can only ascertain this by constant oral questionings during the lecture, followed if need be, by repetition and

additional explanation of what has been said before. This, it seems to me, is just the difference between a popular lecture in a town-hall, and a scientific lesson in a school-room. Of course the rate of progress through the subject will be slow, but anyhow it will be sure. And I am convinced that, with regard to science, it is better to teach even a very small amount thoroughly well, than to get over a much larger extent of ground superficially. Mr. Johnson says, "we who live with boys, and become or remain like boys, know better than the lecturer knows their shoals and fogs of misconception, their power of forgetting, &c., &c."; but this is assuming that the lecturer is not one of the regular resident masters, and so long as this is the case, he certainly would be at a distinct disadvantage. Mr. Oscar Browning, indeed, proposes that scientific information, as distinguished from scientific training, should, as a rule, be taught by such itinerant lecturers. As a first step towards introducing the study of science into a school, this would be all very well, and no doubt it would give a kind of impulse, and awaken interest and aptitude in particular boys. But I should be sorry to see the scientific teaching of a school entrusted to any but regular resident masters.

There is another point in Mr. Browning's paper that, I think, calls for some observations. Mr. Browning urges the introduction of applied mathematics in the form of statics, dynamics, and hydrostatics into schools, and he "sees no reason why these subjects should not be obligatory upon all the scholars." Now I cannot believe that the mass of boys in any public school are sufficiently advanced in mathematics to be able to take up such subjects as these, treated *mathematically*. For those who are sufficiently advanced these studies would be excellent; but I understand Mr. Browning to advocate their general introduction as the staple of the scientific work of the school, chemistry and other branches of science being only taught in exceptional cases.

I can only say that my experience goes to prove that the majority of boys in a public school find quite sufficient difficulties in following a course of lectures on elementary mechanics, including little more than the mechanical powers and the simpler forms of mechanism, treated graphically and arithmetically, not *mathematically*.

I believe that mechanics, hydrostatics, and pneumatics, treated experimentally, with such explanations as involve little more than general reasoning, are exceedingly valuable subjects for school work, and might well form a part in any complete scheme of scientific instruction, but their mathematical treatment must, I feel sure, be reserved for the more advanced pupils.

I will now say a few words with regard to our work at Rugby. The school is divided into :—the lower school, containing 50 boys; the middle school, consisting of two divisions, upper and lower, with a total of about 270 boys; the upper school, with 135; and the highest, or sixth form, with about 45. The average age of the boys in the lower school may be said to be 14; in the two divisions of the middle school, 15 and 16; in the upper school, 17; and in the sixth form, 18.

In the lower school natural science is not taught at all. In the middle school every boy learns some branch of it: and in the upper school and sixth form the boys are allowed to choose between natural science and German, the result being a pretty equal division of the two subjects. In the lower middles the subjects are botany, physical geography, and very elementary mechanics. In the upper middles, the botanical work is continued, physical geography is supplemented by geology, and a higher course of mechanics and mechanism is commenced. In the upper school geology and chemistry are, at present, the only subjects taught, and in the sixth form chemistry alternates with some branch of experimental physics. The work is shared between five masters, who give two lessons a week (of one hour each) to each "set." Rough notes are taken by the boys during the lecture. These are afterwards elaborated into note-books which are shown up to the master at regular intervals; they are looked over and corrected by him, and then returned.

Of the success of the botanical work, as a commencing subject, we have not a doubt. It has proved itself to be quite within the grasp of the younger boys. They do it exceedingly well, and are greatly interested in it. They gain from it ideas of terminology, classification, and generalisation, that prove of real value to them in their subsequent progress.

The mechanical work is more difficult; but many boys have a natural bent in that direction, and those who are fair mathematicians frequently enter into it with ardour. Indeed, there are certain portions of the subject that I have always found to greatly interest the whole class. I refer to the application of mechanical principles to roofs, bridges, buttresses, and the theory of construction in general.

Geology, again, is a subject in which many boys take a real interest. But to study geology satisfactorily implies a larger stock of preliminary knowledge of chemistry, mineralogy, and natural history, than most boys can bring to the task. For this reason, geology is, perhaps, hardly quite satisfactory as a study for schoolboys.

With regard to chemistry I have one or two things to say. There is some uncertainty as to what is generally meant by *teaching* chemistry. In most cases, I believe, it means simply delivering lectures on the metallic or non-metallic elements, with more or less of experimental illustration.

Of course a good deal of information may be given in this way; but I have a strong opinion that this is not sufficient to make chemistry a really effective instrument in a school. I believe that the boys must work and experiment for themselves in order to make the knowledge their own. The most satisfactory results would, I cannot but think, be obtained by making the lessons alternately oral and practical—the teacher explaining at one lecture certain principles and processes, and the boys themselves at the next attempting to carry these out experimentally in the laboratory, of course with sufficient assistance and supervision. Professors Eliot and Storer (whose book I find most valuable as an experimental guide with my own pupils) tell me that this is the plan they adopt in America with very large classes, and they speak with enthusiasm of the gratifying results. Our laboratory at Rugby is too small to enable us to carry out this plan fully at present, but a larger laboratory is about to be built, and then we shall give it a fair trial.

There are many boys, however, who go through what is called a course of chemistry at school without ever touching practical analysis, and yet I believe that this is, educationally speaking, the most valuable part of the subject.

I have for years entertained a strong opinion of the benefits to be derived from the study and practice of chemical analysis, simply as a scientific training, independently altogether of any utility in the knowledge obtained. It has always appeared to me to be almost perfect in the variety of mental faculties which it calls into exercise, and in the example it affords of method in the investigation of truth. All this is shown with great force by Professor Williamson in the second number of *The London Student*, and, so far as I am aware, it is the first time that the peculiar claims of this branch of chemistry, in an educational point of view, have been insisted on. I have daily opportunities of watching the interest that many quite young boys take in working out their analyses in our laboratory, and I have often felt convinced that in no part of their school work are their minds more thoroughly in a state of real activity than when so engaged.

I fear that I have already taken up too much of your space, and therefore forbear to go more into detail. I will only remark, in conclusion, that we by no means regard our natural science arrangements at Rugby as complete, since we are greatly hampered for want of more

accommodation in the way of lecture rooms, &c. As soon as our new buildings are erected we shall introduce much more of experimental science. Elementary hydrostatics and pneumatics will then probably take the place of geology in the middle school, and electricity and heat will form part of the regular work of the upper school and sixth form.—I am, &c.,

T. N. Hutchinson, M.A., F.C.S.

Rugby, June, 1868.

MISCELLANEOUS.

Disinfectants at Terling.—*The British Medical Journal* gives the substance of an official report by Dr. R. M. Glover, Medical Officer of the Millbank prison, who was sent down to Terling by the Home Secretary to superintend the application of disinfection by carbolic acid on an extensive scale during the recent epidemic. Dr. Glover says:—My instructions were to assist in the endeavour to arrest the epidemic of intestinal or typhoid fever prevailing in that village, by means of carbolic acid. I was desired to take with me a supply of carbolic acid for immediate use; to superintend its application in the first instance; and to give every information to the local authorities as to its use and management. I now beg to state briefly the means to which I had recourse for carrying out these instructions, and the apparent effect of the use of carbolic acid in arresting the spread of the fever. On Monday, February 17th, with the concurrence of the local authorities, I caused a strong solution of Calvert's carbolic acid to be distributed over the entire village. Large quantities of the solution were poured into the cesspools; and it was freely applied to the filthy yards, courts, and stagnant ditches by which many of the houses were surrounded, as well as to the manure heaps, collections of refuse, and other nuisances with which the place abounded. The village may be said to have been soaked with the acid, and the atmosphere became highly charged with its vapour, which found its way, in very considerable volumes, into the dwellings of the sick and healthy alike. This process has been daily repeated up to the present time; and I have advised the local authorities to continue the use of carbolic acid during the ensuing spring and summer months. Many of the inhabitants at first fancied that the smell of the acid produced headache, and for a few days the inspector of nuisances, who was employed in its distribution, was the most unpopular person in Terling. This objection, however, has been overcome; and at my second visit on the 18th instant, no complaints were made, although the presence of carbolic vapour in the atmosphere was to be detected at a considerable distance from the village. The epidemic prevailing at Terling was the common intestinal or typhoid fever, a preventable disease, which kills annually no less than 20,000 of the population of this country. Out of a population of 900 persons, about 300 have been attacked with intestinal fever since the 4th December, and of this number 41 have died. With the exception of a lull of a few days in the third week of February, fresh cases continued to occur almost daily up to the end of the month, while only two persons have been attacked since the 1st of March. The carbolic acid was first extensively used, as already stated, on the 17th February; and, allowing ten days for the expiration of the period of incubation, or period of latency, there can be no question that the subsidence of the epidemic corresponds, in point of time, with the date at which the purifying influence of carbolic acid might antecedently have been expected to become manifest. That incredible quantities of fœcal matters had accumulated in uncovered cesspools, open ditches, &c., and had soaked into the soil, admits of no doubt; and there can be as little doubt that the decomposition, or, in the language of chemists, the putrefactive fermentation of these matters, was the essential cause of the fever. The special and characteristic chemical property of carbolic acid is the peculiar power which it possesses of arresting putrefactive changes; and it therefore appears to me reasonable to conclude that the extensive use of carbolic acid and the simultaneous disappearance of the disease, are facts which hold the relation of cause and effect. It will not, I believe, be disputed that our knowledge of the causation of fever points to carbolic acid as being the most powerful agent which can be used for the destruction of that specific poison, which, being absorbed into the human organism, sets up the succession of phenomena known as typhoid fever; and it is to be regretted, therefore, that it was not brought into use at an earlier stage of the epidemic. But, however powerful may have been the action of carbolic acid at Terling, its use as a disinfectant can only be looked upon as a temporary expedient for holding pestilence in check until the contemplated and much needed sanitary improvements have been carried into effect.

Discovery of Rock Salt at Dax.—A mine of rock salt, situated in the Department of the Landes, close to the town of Dax, has been conceded by the Imperial Government of France, and a Company has been formed to carry out the concession by the establishment of salt and alkali works. Its existence was discovered in a singular way. A poor man remarking the profits made by many of his neighbours in working some of the numerous hot springs so abundant in the town of Dax, determined to bore in his garden near the ramparts of the town, in search of hot water. Having pierced about 100 feet through sand and clay, alternating with beds of gypsum, he suddenly struck a hard resistant mass, in which he broke off the end of the boring tool; after numerous and fruitless efforts he succeeded in at length recovering the broken bit, and with it brought up a fragment of rock which proved to be nothing more or less than salt. After this discovery he wisely determined to change the plan of his operations, and to try and bore through the salt with water. With this view a pipe was passed down the soil pipe as far as the salt, and a second and smaller pipe was placed within it. The ends of both the pipes were allowed to rest on the salt, and fresh water being poured down the interval between the two pipes, salt water—resulting from the solution of the salt—was pumped up from the inner pipe. Thus by this process of solution the bed of salt was pierced through to a thickness of no less than 50 feet. This layer was subsequently found to overlie a second one, being only separated from it by a thin layer of saliferous clay. A fresh boring has been executed at about one mile from the first, where several layers of rock salt have been pierced, of a total thickness of 131 feet. The average composition of the salt in its rough state shows that it contains 97⅝ per cent of pure salt. Mr. Maxwell Lyte has prepared a report of this discovery, in which he says that it would be difficult to find a spot more favourably situated for the establishment of salt works than that in which Dax lies. Besides the salt in question the neighbourhood contains mineral deposits of much collateral interest and value. Lignite is found in large quantities; and both French and English coals can be delivered at a very low price, so that there is every reason to believe that this commercial enterprise will be at once lucrative and important.

CONTEMPORARY SCIENTIFIC PRESS.

(Under this heading it is intended to give the titles of all the chemical papers which are published in the principal scientific periodicals of the Continent. Articles which are merely reprints or abstracts of papers already noticed will be omitted. Abstracts of the more important papers here announced will appear in future numbers of the "Chemical News.")

Comptes Rendus.

February 17, 1868.

I. Pierre and E. Puchot. "Experimental Researches on the Products of the Distillation of Beet-root," A. Houzeau, "On Peroxide of Hydrogen considered as not being the Cause of the Changes produced in Litmus Paper impregnated with a Solution of Iodide of

Potassium when used as a Test for Ozone in the Atmosphere." RAKOWITSCH, "On the Use of Chloroform for the Qualitative and Quantitative Examination of Rye Flour and Alcoholic Liquors."

February 24.

A. MALLETT, "A Method of Manufacturing Chlorine and Oxygen from Protochloride of Copper." BLONDLOT, "On the Production of Ozone and Phosphoric Acid during the Slow Combustion of Phosphorus." LE RICQUE DE MONCHY, "On a Ferment discovered in commercial Bicarbonate of Soda." MARTIN, "A Method of Preserving Meat by means of Sulphuric Ether."

Annales de Chemie et de Physique.

January, 1868.

C. MARIGNAC, "On the Separation of Niobic Acid from Titanic Acid, and on an Analysis of Æschynite." R. RADAU, "On the Static Barometer." SCHONBEIN, "On the Presence of Ozone in the Atmosphere." F. ROSSETTI, "On the Use of the Thermo-electric Pile for Measuring the Temperature of the Body." A. BARTHELEMY, "On the Estimation of Carbonic Acid in Bicarbonates and Waters by means of Protonitrate of Mercury." MUSCULUS, "On the Hydrates of Stannic Acid." A. BECHAMP, "On the Action of Chalk and the Living Organisms which it contains in Butyrous and Lactous Fermentations."

Bulletin de la Société Industrielle de Mulhouse.

January, 1868.

COUPIER, "On the Red Colouring Matters derived from Toluene." C. KŒCHLIN, "On Pernod's Process for Detecting Adulterations in Garancine and its Derivatives." G. SCHÆFFER, "On the same Subject." E. DOLLFUS, "On Thirault's Safranine, a new Yellow Colouring Matter." G. SCHÆFFER, "On Meister, Lucius, and Co.'s new Aniline Green." COUPIER, "On the Author's Claim to the Invention of Toluidine and Cumidine Red." THIRAULT, "On Safranine Red and Safranine Yellow and on Mureine Grey." O. SCHEURER, "On the Author's Claim to the Invention of a Method of extracting Red, Pink, and Violet Colouring Matters from Garancine." PERNOD, "A Method of producing the Shades of Colour between Black and Violet and Red and Pink from an Extract of Garancine." A. DOLLFUS, "On E. Hofer-Grosjean's New Fusible Alloy of Lead, Tin, and Cadmium." M. ZIEGLIER, "On the Presence of Aniline in the Coloured Fluid ejected by the Sea Hare, *Aplysia depilans*." E. DOLLFUS, "On Thirault's Safranine Yellow." J. MEYER, "On the same Subject." THOMAS, "On a new Colouring Matter extracted from *Sericographis mohitli*." ROSENSTIEHL, "Report on M. Ziegler's Memoir on the Presence of Aniline in the Coloured Fluid ejected by the Sea Hare, *Aplysia depilans*." HOPP, "On an Extract of Horse Flesh." J. LEFORT, "On Buckthorn Seeds from a Chemical and from an Industrial Point of View." C. O'NEILL, "On the Causes of the Explosion of a Boiler used for preparing Resin Soap." ROSENSTIEHL, "Researches on the Composition of Toluene Red." "REBOULLEAU, "On the Growth of Garancine in Algeria." KUHLMANN, "On the Action of Water on Lead." ASSELIN, "On a Process for Refining Vegetable Oils by means of Caustic Soda." J. ROTH, "On the same Subject." GERBER-KELLER, "Remarks on Kuhlmann's Essay on the Action of Water on Lead." KUHLMANN, "Answer to the preceding Remarks." G. SCHAEFFER, "On the History of the Application of Extracts of Garancine as Colouring Matters for Printing Fabrics." ROSENSTIEHL, "Report on Asselin's Process for Refining Vegetable Oils by means of Caustic Soda. Report on the Methods used at Dieuze for utilising Chlorine Residues and Soda Waste."

PATENTS.

Communicated by Mr. VAUGHAN, F.C.S., Patent Agent, 54, Chancery Lane, W.C.

GRANTS OF PROVISIONAL PROTECTION FOR SIX MONTHS.

1082. A. B. Walker, Warrington, Lancashire, "Improvements in the application of hot blast or heated air for evaporating salt, brine, and other liquids, sugar, or chemicals, also for heating gas and oil retorts, raising steam in boilers, boiling worts in breweries and distilleries, which improvements are also applicable for bleaching, tanning, and baking, likewise improvements in apparatus used in some of the aforesaid applications."—March 30, 1868.

1086. W. Austin, Greville Street, Hatton Garden, Middlesex, "Improvements in the composition, boxes, and surfaces for obtaining instantaneous light from chemically prepared matches intended to be ignited by friction."

1095. H. Bessemer, Queen Street Place, Cannon Street, London, "Improvements in the manufacture of malleable iron and steel, in the heating and melting of metals, and in the machinery or apparatus employed for such purposes."—March 31, 1868.

1102. W. Smith, Glasgow, N.B., "Improvements in the manfacture of pig iron."—April 1, 1868.

1117. J. G. Dale, and E. Milner, Warrington, Lancashire, "An improved method of producing white pigments from lead."—April 2, 1868.

1124. C. D. Abel, Southampton Buildings, Chancery Lane, "A new or improved process and apparatus for refining camphor."—A communication from C. E. Perret, Boulevard de Strasbourg, Paris.

1126. J. M'Culloch, Shawlands, Renfrewshire, N.B., "Improvements in utilising old or waste tarpaulin in the manufacture of grease and other useful products, and in apparatus therefor."

1130. J. H. Johnson, Lincoln's Inn Fields, Middlesex, "Improvements in the manufacture of cast steel and malleable iron from cast iron, and in the apparatus or means employed therein."—A communication from F. Ellershausen, Ottawa, Canada.—April 3, 1868.

NOTES AND QUERIES.

Vessel for holding Nitric Acid.—"R. C. M." is desirous of information concerning a vessel to hold about 193 cubic feet = 1,200 gallons of liquid made up of 1-20th of strong nitric acid and 19-20th of water, and to boil this mixture by the aid of steam applied inside, which of course implies that steam from a boiler is brought by means of a pipe ending in the liquid with open orificcs. "R C. M." might make the tank of slate; slabs of this material can be had of any size, and, if properly selected, will answer his purpose admirably; the slabs will of course have to be grooved into each other, and the grooves run tight with a resinous cement. As to the material of the steam pipe "R. C. M." should recollect that neither lead, copper, iron, or tin can in his case serve the purpose, and he would do best perhaps to procure for the inside of the vessel as steam pipe, a tube made of hard pottery ware perforated with small holes; such tube any of the Lambeth potteries, or the works of the Staffordshire pottery district, would make him to order. I suppose "R. C. M." knows that the condensation of steam in the cold fluid will tend to dilute the acid of the mixture, and also to increase the bulk of the fluid inside the tank, the capacity of which should not be less than 200 cubic feet; a tank 10 feet long, 5 feet wide, and 4 feet deep will answer the requirements, inside measurement.—DR. A. A.

Aniline Green.—Cathartic Acid.—I reply to "Test-tube," concerning aniline green and cathartic acid, the following:—I am not aware that there exists any iodine green; perhaps "Test-tube" may try picric acid, also called carbazotic acid, and indigo solution, as "Test-tube" must be aware that nearly all green dyes are made up from the mixtures of yellow and blue dyes. A genuine green, known as Chinese green, or Lo-kao, is obtained in China from *Rhamnus chlorophorus* and *utilis*; the berries of *Rhamnus catharticus*, a plant met with in Southern Europe, yields the sap-green. "Test-tube" is mistaken as to his notion on senna leaves; the tree which yields these has nothing at all in common with the plant just alluded to, neither has cathartic acid with either the purgative properties of senna or the dye value of divers varieties of the *Rhamnaceæ*. Lo-kao is, at least in France, a regular article of trade, and pretty extensively used at Lyons, Nismes, Rouen, and other centres of textile fabrics and dyeing industry of France.—X.

Cast Porcelain.—Can any one tell me what this material is? It is being manufactured in America on a somewhat large scale, and from some specimens which I have seen, it must be a very beautiful substance. Is it ordinary glass devitrified?—SILEX.

Sulphide of Magnesium.—Can any one furnish me with a practical recipe for the production of MgS?—W. W.

Dead Oil.—A correspondent wishes to know where this can be obtained.

TO CORRESPONDENTS.

Qui Color.—The best book on the subject, with which we are acquainted, is O'Neill's Dictionary of Dyeing and Calico Printing, published by Ireland & Co., Manchester. Messrs. Longmans & Co. have a work on the aniline dyes just ready for publication.

R. E. B.—An Old Subscriber.—Some little time must unavoidably elapse between the delivery and the publication of a lecture, when, as in the instances you refer to, the author himself rewrites the lecture specially for our columns. We imagine that the readers of the CHEMICAL NEWS prefer to wait for a week or two when the advantages to be gained are so great. In scientific lectures a mere shorthand report is not sufficient without the authors' corrections.

Z. Z.—For the processes required for the valuation of manganese see the fourth edition of "Fresenius's quantitative analysis," published by Churchill, page 618.

Communications have been received from W. Woodbury; J. Samuelson; W. White (with enclosure); D. Forbes; A. de Lomonossoff; W. H. James (with enclosure); F. O. Ward; J. N. Vinen; R. E. Bibby; R. Ruding; Rev. T. N. Hutchinson (with enclosure); Dr. B. H. Paul (with enclosure); E. Yewdale; J. Storey & Co.; Dr. J. Blyth (with enclosure); J. S. Brazier (with enclosure); Thos. Taylor (with enclosure); P. Squire; F. Ritchie; D. Penney; P. J. Worsley (with enclosure); the London Stereoscopic Company; Archibald, Walker, and Co. (with enclosure); J. Young; M. Monier (with enclosure); J. Heywood.

MEETINGS FOR THE WEEK.

WEDNESDAY.—Meteorological, 7.
—— Geological, 8.
THURSDAY.—Royal Institution, 3. Sir John Lubbock, Bart., "On Savages."
—— Royal, 8½.
—— Chemical, 8. Professor Wanklyn, "On the Establishment of High Chemical Formulæ." Professor Church, "On some Cornish Minerals."
—— Philosophical Club, 6.
SATURDAY.—Royal Institution, 3. Sir John Lubbock, Bart., "On Savages."

THE CHEMICAL NEWS.

VOL. XVII. No. 446.

ON THE

ESTIMATION OF SULPHUR IN MINERALS CONTAINING IRON.

By M. W. EGGERTZ,
Professor in the Fahlun School of Mines.

FIVE grammes of the mineral, ground as finely as possible in an agate mortar, are treated with chlorate of potash and hydrochloric acid in the same manner as in the case of iron. After desiccation and the employment of hydrochloric acid and water, the insoluble substances may be sulphates of lead, lime, baryta, strontia, silicic acid, and undecomposed mineral. By stirring well and filtering the liquid whilst warm the two former salts likewise may generally, however, be dissolved.

The filtration should be performed through strong paper, or rather through a double filter, to prevent the pulverised mineral from passing through. When the clear portion of the solution has been poured upon the filter, add to the insoluble matter 5 c.c. of hydrochloric acid and 5 c.c. of water; then leave the mixture for two hours on the water-bath at a boiling temperature, and, if care be taken to stir well, the sulphate of lime will be completely dissolved. Wash the insoluble portion in warm water, and pour it on a filter, taking care to place below a flask specially to receive that portion of mineral which may have passed through the filter. The precipitation of the sulphuric acid is effected as already described.* After adding chloride of barium and cooling the solution, add 10 c.c. of ammonia.

To dissolve the sulphate of lead, which may occur in the insoluble portion, remove this from the filter with the end of a feather, introduce it into a flask, and pour over it 10 c.c. of concentrated acetate of ammonia. The solution after having been strongly agitated and heated in the water-bath, is carefully poured on to a filter. Then wash the residue with a little acetate of ammonia, and repeat the treatment until a few c.c. of solution, acidulated with a little acetic or hydrochloric acid, is not rendered turbid when warmed with chloride of barium solution. The filtrate is then diluted, slightly acidified, and the sulphuric acid precipitated by means of chloride of barium. After the sulphate of lead has been removed, there may still occur sulphates of baryta and strontia in the insoluble portion, although these have not hitherto been met with in the iron ores of Sweden. To decompose these salts the residues must be dried, heated to redness, and weighed, and then fused with five times their weight of pure dry soda. The mass is digested with water over a water-bath, the liquid filtered, and the residue washed with warm water. The silicic acid is separated from the solution, which contains the silicate, the carbonate, and the sulphate of soda, by adding hydrochloric acid and drying on the water-bath. After filtering, precipitate the solution with chloride of barium.

To ascertain if the iron mineral contains gypsum or other soluble sulphates: take 5 grammes, place them in 20 c.c. of hydrochloric acid and 60 c.c. of distilled water, and digest them for three hours on the water-bath, with frequent agitation. The filtered solution is mixed with chloride of barium and 15 c.c. of ammonia; then proceed as already described. If there are found in the mineral grains of iron or copper pyrites, or galena, their presence will only give in this operation traces of sulphuric acid.

To ascertain the accuracy of the process which has just been described, the following experiments have been made:—

* See CHEMICAL NEWS, this Volume, No. 439, p. 207.

I. A sulphide of iron (as usually employed in the preparation of sulphuretted hydrogen) was dissolved in the manner above described, in hydrochloric acid, water and chlorate of potash, in a flask furnished with a glass tube leading into a solution of sulphate of copper. No trace of precipitate, nor any colouration which would indicate the presence of sulphide of copper, could be detected in the solution; so that it is certain that no loss of sulphur could take place on dissolving iron or minerals in this manner.

When the experiment was repeated, but with this alteration, that the solution of chlorate of potash, during the addition of hydrochloric acid, was not kept well boiling, the sulphate of copper solution was rendered turbid, showing the necessity of resorting to complete ebullition.

II. The sulphide of iron treated in the same apparatus with aqua regia (composed of equal parts of nitric and hydrochloric acids) requires a more equal temperature and a stronger ebullition to prevent any precipitation of copper. Moreover, as the solution of iron by chlorate of potash takes place promptly, and as the presence of nitric acid is prejudicial both for the solution of the mass after drying in the water-bath (owing to the formation of subnitrate of iron), and also for the precipitation of the solution by chloride of barium, preference should be given to the employment of chlorate of potash.

III. One gramme of pure pyrites was treated in the above described manner, and the sulphur which separated at first was completely dissolved during the drying of the liquid on the water-bath. Upon precipitating with chloride of barium, a quantity of sulphate of baryta was obtained, exactly corresponding to the amount of sulphur in the pyrites.

IV. Many experiments were made with the filtered liquids obtained from solutions of iron which had been freed from sulphur by excess of chloride of barium. After standing for several days these have been brought to ebullition and mixed with 1 c.c. of an aqueous solution of sulphate of soda, containing a quantity of sulphuric acid, corresponding to 0·0001 gramme of sulphur. At the end of at least 24 hours a feeble precipitate is observed, which proves that the sulphuric acid is perfectly separated from the iron. This precipitate is rendered distinctly visible when the contents of the flask are stirred by moving a glass rod circularly round the liquid, for the sulphate of baryta then mounts in a small whirlpool and falls again on to one spot. When, also, 50 c.c. of the solution of sulphate of soda are added to the filtered liquids, the exact weight of sulphate of baryta has been obtained. If, in the filtered liquids, there were 10 c.c. of free hydrochloric instead of 5, two days sometimes elapse before the precipitate from the solution (containing 1 c.c. of the sulphate of soda solution) becomes visible. Consequently if there are more than 5 c.c. of free hydrochloric acid in the solution, there must be added for each c.c. in excess (to the solution augmented by chloride of barium and then cooled) 1 c.c. of caustic ammonia, weighing 0·95 gramme, and the hydrochloric acid will be in this way nearly neutralised. The precipitation takes place more easily than when the iron solution is used, if 1 c.c. of the aqueous solution of sulphate of soda is added to a mixture of 150 c.c. of water and 5 c.c of hydrochloric acid; this shows that the solution of the iron mineral renders the precipitation more difficult.

V. In many experiments in which the sulphate of baryta was washed for a long time on a filter, both with warm and cold water, it was impossible, upon evaporating a drop of the filtrate on a watch glass, to avoid traces being left on the latter. Some drops of a solution of chloride of barium and a little hydrochloric acid having been added to the filtrate always produced a feeble white precipitate by the action of heat. For this reason the washing should only be continued until a drop of the washing water leaves on evaporation on glass an almost interceptible white ring.

VI. 4·9 grammes of carbonate of lime, and 0·1 gramme

of pyrites submitted to the same process of solution gave by evaporation on the water-bath a perfectly clear solution; after drying, 6 c.c. of hydrochloric acid, and 50 c.c. of water were added to the residue, and there was thus obtained a considerable mass of gypsum. On leaving the liquid for an hour on the water-bath and increasing the quantity of water, the gypsum was completely dissolved.

0·1 gramme of gypsum was dissolved in 2 c.c. of hydrochloric acid and 4 or 6 c.c. of water (or better still in 10 c.c of acetate of ammonia). The solution was left for half an hour on the water-bath with frequent agitation. It could then be diluted with water, at pleasure, without the gypsum being precipitated.

VII. 0·5 gramme of sulphide of lead (galena) were completely dissolved in the same manner, and the solution brought to dryness on the water-bath. A little water was added to remove the chloride of potassium, and evaporated after solution of this salt; the sulphate of lead which remained was then completely dissolved in 20 c.c. of acetate of ammonia, and precipitated with acetate of baryta containing a little free acetic acid. The sulphate of baryta, which was of the proper weight, was of a grey colour, but became whiter without alteration of weight by being heated to bright redness for some time with access of air.

0·1 gramme of sulphate of lead dissolves without rise of temperature in 2 c.c. of acetate of ammonia; chloride of lead dissolves in it still more easily. The solution may be diluted with water without any precipitation taking place.

0·1 gramme of sulphate of lead was dissolved on the water-bath by violent agitation for half an hour with 4 c.c. of hydrochloric acid (of 1·12 sp. gr.) diluted with 2 c.c. of water; but the sulphate commenced to crystallise as soon as the liquid was cooled to 60° or 70°.

VIII. 4·7 grammes of an iron mixture containing a known quantity of sulphur having been mixed with 0·1 gramme of pyrites, 0·1 gramme of sulphide of lead, and 0·1 gramme of gypsum, treated in the same manner as the iron mineral, gave exactly the quantity of sulphate of baryta which it should have yielded by theory. The precipitate from the solution in acetate of ammonia only weighed a few milligrammes.

IX. An aqueous solution of sulphate of soda, accurately standardised, and then chloride of barium, were added to a solution of soluble glass acidulated with hydrochloric acid. The weight of the sulphate of baryta which was formed showed that the gelatinous silicic acid in solution in the acid does not interfere in these experiments.

X. 0·1 gramme of pure quartz was fused with 0·5 gramme of anhydrous soda in a platinum crucible. The mass was completely soluble, without heating, in 5 c.c of water, and the liquid remained clear even in the water-bath.

XI. 0·1 gramme of sulphate of baryta was fused in a platinum crucible with 0·5 gramme of carbonate of soda; the mass, treated with water, was poured upon a filter and washed. The carbonate of baryta remaining on the filter was brought to a red heat, and then dissolved in dilute hydrochloric acid without the least residue. The same experiment was tried with sulphate of strontia. It follows, therefore, that these two salts may be completely decomposed by fusion with carbonate of soda.—*Moniteur Scientifique.*

ON THE

ESTIMATION OF SULPHUR IN COAL GAS.*

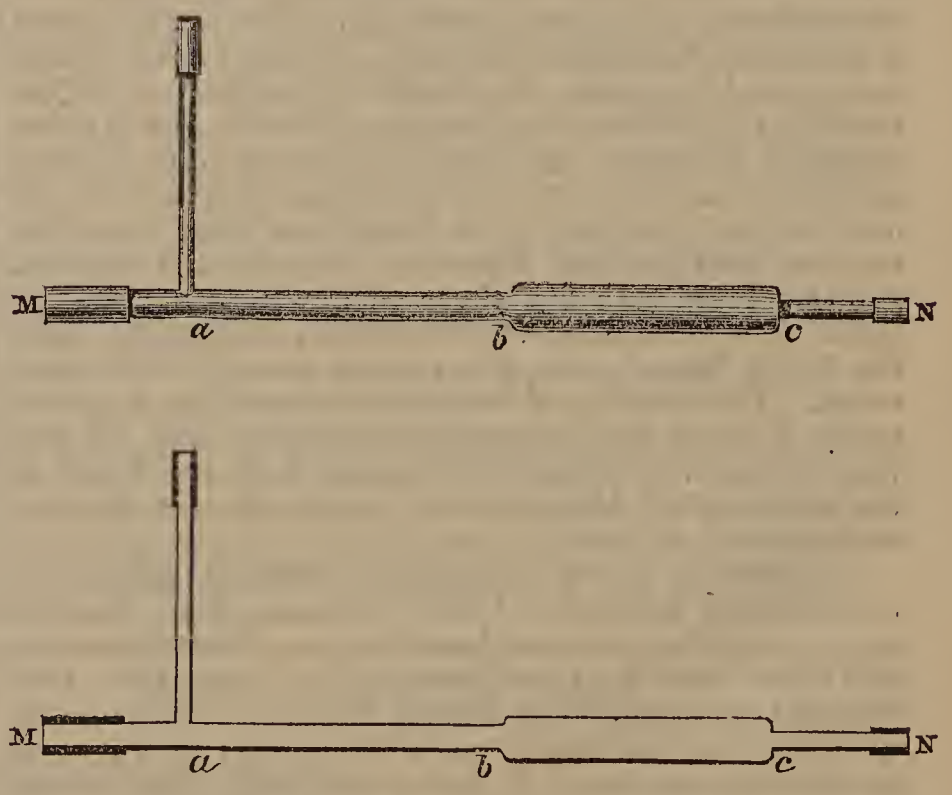
By WM. VALENTIN, Esq.,
Principal Assistant in the Royal College of Chemistry.

In an article "On a New Method for Estimating Sulphur in Coal Gas," published in the *Journal of Gas Lighting* of January 7, 1868, I drew the attention of gas engineers to the defects of the method now generally in use for estimating the sulphur in gas. I stated that at some future time I would submit the two methods—viz., that described by me, which, for shortness sake, I will call the *platinum process*, and the *lime* (soda-lime) *proccss*, to some further comparative experiments.

Since writing these lines I have had occasion to make a series of experiments on the estimation of sulphur, both in the form of volatile sulphur compounds and that of sulphates contained in grains, such as wheat, peas, beans, &c., and have for this purpose combined the two methods of combustion—viz., over spongy platinum and over pure soda-lime perfectly free from sulphuric acid. To effect this I employed a tube made of thin platinum, in which the combustion of the volatile sulphur compounds was accomplished by passing them over spongy platinum, and then over a layer of soda-lime. The air which was required to burn the volatile organic matter was supplied through a narrow platinum tube, which branched off from the main tube a little way anterior to where the layer of spongy platinum was placed. An aspirator drew the required amount of air through the tube, and by means of an adapter-tube and flask containing some solution of *pure* caustic soda, any sulphuric acid could be arrested that might possibly escape the absorptive and fixing action of the caustic alkalies placed in the platinum tube. After a number of most carefully conducted experiments, I satisfied myself that not a trace of sulphuric acid escaped from the platinum tube, and that, in fact, my precaution of having a solution of a powerful absorbing agent in front was superfluous. *Soda-lime absorbs and arrests every trace of sulphuric acid.* Acting upon this observation, I at once adopted the same mode of proceeding for the estimation of the sulphur in coal gas. Thus, instead of carrying on a series of comparative experiments in order to establish the claims of the lime process over the platinum process—the so-called Letheby process, I believe, may be left out of consideration altogether, after the evidence that has been brought forward by various investigators, showing its utter worthlessness as a process for accurately estimating the sulphur in coal gas—I resolved upon combining the advantages of perfect and easy combustion possessed by the platinum process with those for complete absorption of the sulphuric acid held out by the soda-lime process.

To this end I employed a platinum tube,* MN, Fig. 1 (¼ size), 13 inches in length, which could be heated on a small Hofmann's combustion furnace, in the place of

Fig 1.



the porcelain tube described in a former article. The portion of the platinum tube marked *a b*, 5 inches in length and three-eighths of an inch in diameter, is charged

* Reprinted, by permission, from the *Journal of Gas Lighting*.

* I am indebted to my friend Mr. E. Matthey, of the firm of Messrs. Johnson and Matthey, of Hatton Garden, for a platinum apparatus of admirable make and great neatness.

with a cage constructed of a double roll of fine platinum gauze and filled with spongy platinum. The wider portion of the tube, *b c*, 4 inches in length and five-eighths of an inch in diameter, contains the soda lime. The air requisite to completely burn the gas enters through a narrow glass tube, connected by means of a small cork at M with the wide end of the platinum tube. A cap of an alloy of silver and copper soldered to this part of the tube strengthens it sufficiently to prevent the thin platinum from being injured by a tightly fitting cork. The supply of gas to the tube passes through the narrow tube four inches long and three-sixteenths of an inch in diameter, likewise capped, which is seen to branch off at a right angle from the portion of the main tube next to the anterior part of the platinum cage containing the spongy platinum. The products of combustion are allowed to escape at N, through the narrow platinum tube *c* N. Connections for supplying gas and air are made by narrow non-vulcanised black india-rubber tubing.

I find it most convenient to supply the air under slight pressure by means of a gasholder, from which the air is expelled by displacement with water. Such gas-holders are found in most gas-works. A Low's motive-power meter may also be employed with great advantage, as it is capable of being regulated so as to give a supply of air sufficient for one experiment, whilst a small gas-holder would require to be refilled with air once or twice during an experiment. The pressure from the gas-mains is at all times sufficient to send the gas through the platinum tube. The respective proportions of gas and air are best regulated by means of meters, when the pressure-gauge must of course show the same heights of water column, or the air may also be adjusted without having to pass through a meter merely by using a compression-cock on the narrow india-rubber tube, as described in a former paper.

Preparatory to an experiment the platinum tube is charged with pure soda-lime, by dropping in a lump sufficiently large to easily block up the narrow exit-tube. The whole of the wide portion of the tube, *b c*, is then gradually filled with loose pieces of soda-lime, of a size to enable the operator speedily to shake out the charge of soda-lime when the combustion is over. By gently tapping the platinum tube, held in an upright position whilst it is charged, the layer of soda-lime shakes down pretty completely, and is yet sufficiently porous to allow of a free and easy passage for the gaseous products of the combustion. The cage of spongy platinum is next introduced, and pushed down past the narrow branch tube. The gas and air connections are then made, and the platinum tube is placed upon a small combustion furnace, and heat applied to it from *a* to *c* by turning on the required number of gas burners at once. The platinum tube is best protected from the action of the gas flames by being kept imbedded in asbestos loosely spread in a thin layer along a trough made of tinned sheet iron. The supply of gas to the clay gas-burners should at no time be so great as to cause flames to shoot up above the burners or the tiles which cover the furnace. A dull red heat is sufficient, especially as much heat is generated inside the platinum tube by the combustion of the gas and air within the pores of the spongy platinum.

From ·5 to 1 cubic foot of gas can conveniently be burned per hour, requiring from 5 to 10 cubic feet of air for its complete combustion. An experiment can thus be done in three to four hours, since as a rule 2 to 4 cubic feet of gas burned in the manner described yield sufficient sulphuric acid for an accurate weighing in the form of sulphate of barium.

The experiment over, the tube is allowed to cool in a slow current of air alone. The two ends are disconnected, and the cage of spongy platinum drawn out by means of a copper wire having a little hook at one end. A stout bit of platinum wire in the form of a loop or ring may also be attached to the cage, for the more ready removal of the platinum cage. The latter is placed into a good-sized test-tube, and treated repeatedly with boiling distilled water, acidulated in the test-tube with a little dilute hydrochloric acid, in order to remove a little sulphuric acid which the spongy platinum retains. The soda-lime is next shaken out into a high beaker, and the tube washed out with hot dilute hydrochloric acid. This is most conveniently done by moving the platinum tube, held in a horizontal position, with its contents of dilute acid backwards and forwards in a small Bunsen gas flame. The rinsings are poured over the solid soda-lime contained in the beaker. Loss from spirting must be guarded against by rapidly covering the beaker with a large watch-glass after each addition of hydrochloric acid, as long as an effervescence takes place. The soda-lime is completely dissolved by the application of a gentle heat, and the carbonic acid must be entirely driven off before the sulphuric acid can be precipitated by means of a solution of chloride of barium. On gently heating for some time, the precipitate falls out readily and completely, and may be filtered off after standing for a short time, and ignited and weighed as sulphate of barium.

I need not explain that the sulphuric acid can also be estimated volumetrically by means of a standard solution of chloride of barium.

In conclusion, I will shortly point out the advantages of the new method* of estimating sulphur in coal gas over that now in use, and known as Dr. Letheby's sulphur test, viz.:—

1. *Perfect* combustion of the gas in a *close* vessel, at a very high temperature.
2. Complete and easy absorption of the sulphuric acid generated by the oxidation of the bisulphide of carbon, in the *same* vessel in which it was generated.
3. No loss from imperfect condensation.
4. Possibility of completing an experiment in a few hours' time, or, as seems most desirable, during the time of the evening when the greatest consumption of gas occurs.

ON SOME EFFECTS OF THE HEAT OF THE OXY-HYDROGEN FLAME.†

By WILLIAM ODLING, M.B., F.R.S.

I.

Chemical changes, whether of combination or decomposition, result in the production of new bodies which, under the conditions of the change, have for the most part a greater stability than the original bodies.

One evidence of this greater stability is afforded by the development of a quantity of heat—the heat of chemical action—from the produced bodies having a smaller potential heat than the original ones.

It results, both from reason and experiment, that in order to undo or reverse any definite chemical action, just so much heat must be directly or indirectly expended as was evolved by the original action.

For the same quantity of heat evolved, the resulting temperature varies with the mass and kind of matter heated, and with the rapid or gradual evolution of the heat.

When the evolution of heat is instantaneous, the resulting temperature may be calculated from the quantity of heat evolved, and the mass and specific heat, &c., of the matter heated.

By a unit of heat is meant the quantity of heat necessary to raise the temperature of one kilogramme of water one degree Centigrade, or more accurately from 0° to 1°.

* Mr. Hartley, of the firm of Messrs. Wright and Co., 55, Millbank Street, Westminster, is prepared to supply the apparatus for the new sulphur test.

† A lecture delivered before the Royal Institution, Friday, May 22, 1868.

II.

Every 18 grammes of water is a combination of two 1-gramme proportions of hydrogen H, with one 16-gramme proportion of oxygen O; and by the combination of two grammes of hydrogen with sixteen grammes of oxygen, there are developed 68 units of heat.

Of these 68 units of heat, however, little more than 57 units are really due to the chemical action,—nearly 11 units of heat being evolved by the contraction of the original mixed gas into two-thirds its volume of steam, and by the further condensation of the resulting steam into 18 cubic centimetres of water.

While the quantity of heat evolved by the combination of a given quantity of oxygen and hydrogen is invariable, the intensity of the heat may vary from a scarcely recognisable rise of temperature up to the highest temperature of the oxy-hydrogen blowpipe flame, capable of fusing platinum and silica.

A most remarkable effect of the intense temperature resulting from the combination of oxygen and hydrogen into water, is the partial decomposition of water into oxygen and hydrogen, discovered by Mr. Grove in 1846.

At this high temperature, hydrochloric acid and carbonic anhydride gases also undergo partial decomposition, into hydrogen and chlorine, and into carbonous oxide and oxygen respectively.

Upon what do these singular decompositions by heat, of bodies formed with great evolution of heat, depend; or with what class of chemical phenomena may they be associated?

III.

Under certain familiar conditions, chemical action seemingly takes place to its utmost possible extent in a single direction only, with production of a maximum amount of the substance that is formed with maximum evolution of heat.

For example, taking atomic proportions in grammes, the heat of formation of chloride of zinc, $ZnCl_2$, is 101 units, and the heat of formation of chloride of copper, $CuCl_2$, is 60·5 units. Hence, with chlorine in solution and excess of both copper and zinc, there is finally produced the maximum possible amount of chloride of zinc and no chloride of copper.

Again, an addition of sufficient zinc to solution of chloride of copper, there is complete combination of chlorine with zinc and complete separation of chlorine from copper, *i.e.* complete burning of the one metal and complete unburning of the other.

IV.

But under simpler though less familar conditions, chemical action habitually takes place in more than one direction simultaneously, with production of correlative products in varying proportions.

Thus, with hydrogen and excess of both chlorine and oxygen, although the heat of formation of oxide of hydrogen H_2O is 57 units, and the heat of formation of chloride of hydrogen 2HCl, is only 47·5 units, yet, in this case, the hydrogen does not combine with the oxygen to the exclusion of the chlorine, but divides itself between the oxygen and the chlorine in proportions which vary with the conditions of the experiment.

In accordance with this result it is found that, at the same red heat excess of chlorine, will effect the partial decomposition of water with extrusion of oxygen; and conversely, that excess of oxygen will effect the partial decomposition of hydrochloric acid with extrusion of chlorine.

So that, beginning with the two chemical substances, water and chlorine, or beginning with the two chemical substances, hydrochloric and oxygen, or beginning with the three chemical substances, hydrogen, chlorine, and oxygen, there exist, at a full red heat, the four chemical substances, water, hydrochloric acid, chlorine, and oxygen; the proportions of the four substances depending certainly upon the relative quantities present of the elements concerned, and most probably also upon the temperature of the experiment.

Similarly, beginning with the one chemical substance, water (Grove), or beginning with the two chemical substances, oxygen and hydrogen (Bunsen), there always exist, at a sufficiently high temperature, the three chemical substances, water, oxygen, and hydrogen.

Although, by exposure to a red heat, the electrolytic mixture of oxygen and hydrogen gases becomes completely combined, or transformed into water, yet, as recently shown by Bunsen, at the high temperature of 2024 degrees, only one-half, and at the still higher temperature of 2844 degrees, only one-third of the mixture undergoes combination, the other one-half or two-thirds remaining in the state of mixed gas.

V.

Chemists are acquainted with many reciprocal actions comparable with those of chlorine upon water, and of oxygen upon hydrochloric acid, the most familiar instance being probably the decomposition of ignited oxide of iron by hydrogen with extrusion of iron, and the converse decomposition of oxide of hydrogen by ignited iron with extrusion of hydrogen.

Similarly, sodium will decompose the oxides of carbon, while carbon will decompose oxide of sodium; and just as a sufficient excess of chlorine may be made to effect the almost complete decomposition of a given quantity of water, so may a sufficient excess of carbon (or carbonous oxide) be made to effect the almost complete decomposition of a given quantity of sodium oxide or zinc-oxide, as in the ordinary processes for obtaining the two metals; notwithstanding that, for an equal consumption of oxygen, the respective combination heats of sodium and zinc exceed by far the combination heat of carbon or carbonous oxide.

Again, although the combination heat of oxygen and carbonous oxide is 68 units, while that of oxygen and hydrogen is only 57 units, yet, as was shown by Bunsen many years ago, upon exploding a mixture of oxygen with a joint excess of carbonous oxide and hydrogen, the oxygen does not attach itself exclusively to the carbonous oxide, but divides itself between the carbonous oxide and hydrogen in a ratio determined by their relative proportions.

EXPERIMENTS WITH PAPER FILTERS.

By CHARLES E. AVERY,

Student in the Massachusetts Institute of Technology.

THE filter most commonly employed in analytical laboratories is a circular piece of paper folded twice upon itself into the form of a quadrant, and supported on a glass funnel with straight sides. This filter, though commendable in so far as it is capable of supporting the weight of a considerable column of liquid without breaking, is objectionable, inasmuch as liquids cannot pass through it so rapidly as is desirable. Since at almost every point the paper is in close contact with the glass, but little of the liquid can flow off between the filter and the sides of the funnel.

Several schemes have at various times been proposed for opening water-ways between the glass and the paper; the interposition of straws, glass rods and splinters of wood between filter and funnel, as well as fluted funnels and plaited filters are all devices looking to this end.

The advantages of the plaited filter are so great that some chemists prefer to use it, even in quantitative analysis, instead of the common form, in spite of its greater liability to break, and the difficulty of washing the precipitate.

In the laboratories of Profs. Lawrence Smith and Ordway, among the most accurate of American analysts, plaited filters are said to be employed to the almost total exclusion of the plain form.

Another excellent method of increasing the speed of filtration, first suggested in America by the German chemist, Fleitmann, consists in placing one plain filter within another of coarser fibre; for instance, a fine plain filter of Swedish paper may be placed within another plain filter of coarse German paper, supported, as usual, on a funnel.

In experimenting upon these various forms of filters, it occurred to me to fold the plain qualitative filter in two operations instead of one. In place of folding the filter doubled upon itself down the middle in the usual way, I proposed to turn down on each side of the paper a fold equal to one-quarter of the semi-circle, and then to fold the sectors of 45° arc thus formed back upon themselves.

The filter is then opened without disturbing the folded portions, and placed upon the funnel. In this form the triple side of the plain filter is broken up, and the folded portions keep open passages, instead of hindering filtration.

This filter, as tried against the plain form, gave, 1st, 133 : 100. 2nd, 111 + : 100. 3rd, 205 + : 100.

Two plain filters ran equally in several trials; each was changed into the other's funnel, and No. 1 ran 33 per cent less than No. 2. No. 1 was dried and folded into my form; remaining in the same funnel, it ran 32 per cent faster than the other. Both filters were then opened, and showed no tear or weakness when held against the light.

As these filters gave different results in different funnels, I thought I would ascertain the cause. The water seemed to be retarded in its passage by the attraction of the glass; therefore, those funnels having the greater portion of the paper free from the glass would be the best; that is, *a broad-throated funnel, other things being equal, will filter faster than a narrow-throated funnel.*

To test this point I selected two *large* funnels; No. 1 had three times as broad a throat as No. 2. With the first filters they ran:

117 : 100 123 : 100 133 : 100 118 : 100

The reason for this low difference was found in a thin spot near the point of No. 2.

Other sets of filters gave:

2nd set	292 100	318 : 100	
3rd set	288 100	335 : 100	
4th set	300 : 100	burst.	
5th set	384 : 100	407 : 100	482 : 100
6th set	242 : 100		

In the last set a porous filter, though off the same sheet as No. 1, was given to No. 2. Throughout the whole series of experiments every fair advantage was given to the weaker party, it being the first filled and the last emptied.

To make assurance doubly sure, I tried filters in like funnels, stopping the pores of the paper at various points. Paraffin applied whilst liquid was the substance first used to prevent filtration.

Two filters were chosen from the same sheet and of as uniform a texture as possible. No. 1 was stopped over one-third the radius from the point. No. 2, all but one-third the radius at the point.

They filtered at nearly the same rate, No 1 slightly the faster. The paraffin made the paper stiff, and as water does not adhere to it, free passage was allowed between it and the funnel to the water of the upper part of No. 2.

Here we see that one-ninth of the surface of the filter, when free, did as much work as eight-ninths adherent to the glass. The experiment was repeated with glycerin instead of paraffin. No. 1 ran 28 per cent the faster. It might be objected that glycerin would wash out; so a pap of paraffin and spirits of turpentine was used to repeat the experiments. Each filter, after being painted, was wetted to prevent the spread of the mixture by capillary action. The trials were not sufficiently numerous to find a true mean, but the free point invariably ran the most—from 4 or 5 per cent excess to 100 per cent. The point was assumed to be a circle one-third the radius of the filter.

I understood the idea of the Fleitmann filter to be this, that, likening a plain filter to a peat bed resting upon an impermeable sub-soil, it might be compared to a porous substratum interpolated between the swamp and the clay bottom.

To test this idea a Fleitmann filter was made and wetted, carefully patting down and smoothing out any irregularities. It was tried against a plain filter which was placed in a funnel with but two-thirds as wide a throat as that of the Fleitmann. It ran 114 : 100; that is, the passages kept open by the elasticity of the paper, the creases and abutting edges liken this filter to tile drainage.

To increase the size and number of passages I tried putting the inner filter into a plaited filter of coarse paper. Changing the filters after each trial, I found this form gave the following results as compared with the plain filter, calling the latter one hundred.

1st trial	184 : 100	4th trial	166 : 100
2nd ,,	201 : 100	5th ,,	170 : 100
3rd ,,	250 : 100		

I afterwards found a thin spot in the plain filter of the fourth trial.

Next a precipitate of sulphate of calcium was tried; the filtrates were as 131 : 100. On weighing the precipitates collected they were as 200 : 100.

This form of filter was abandoned, since it does not filter as fast, is not as strong, takes more time to make and care to use than the form next to be described. It is, however, better than the plain filter as regards speed of filtration, equal to the plaited in this respect, and stronger than it.

To admit the use of very broad-throated funnels the number of outside filters was increased to two; these over-coats were pierced with long narrow apertures running from the point to the circumference.

In the following experiments, the improved filter was tried against the *plaited* form. The improved filter, on account of its great strength, was allowed a funnel with a throat once and a half as broad as that given to the plaited filter. It would have borne one three times as broad.

1st trial	121 : 100	3rd trial	112 : 100
2nd ,,	125 : 100	4th ,,	115 : 100

The fifth trial was made with a precipitate of sulphate of calcium; the filtrates were 82 : 100. Here the new filter seemed at fault, but on weighing the precipitates collected they were as 140 : 100. The new filter being last filled, got the thicker portions each time, whilst the plaited filter got the weak top liquor.

The outer jacket is cut by folding the filter as a plain filter and taking out a sliver on each edge; repeating the process increases the number of apertures. If the filters are to be used double, as in broad-throated funnels, the openings should extend nearly to the vertex; if they are used single, in common funnels the openings need not extend so far, and the extreme point may be removed.

A thick porous felt might be used for acid liquors as an outer filter. Cotton cloth would serve in alkaline solutions; gun-cotton could be used with either. If asbestos cloth could be procured it would probably be the best. It might be cleansed by burning, and would be unaffected by anything likely to be filtered in quantitative analysis, nor would it harm the filtrate when estimations are to be made by permanganate of potassium.

Most coarse filter paper is "stuffed" with minera matter; such paper must of course be washed in

acidulated water before being used for quantitative work, where the filtrate is to be saved.

When the precipitate is dry, the outer filters are thrown away, only the fine inner filter being burned.

Plaited and plain filters were tried in like funnels; calling the plaited filters No. 1, we had:

	I.	II.		I.	II
1st trial	116 :	100	3rd trial	236 :	100
2nd ,,	237 :	100	4th ,,	136 :	100

5th ,, with sulphate of calcium precipitate, 231 : 100 the precipitates weighed 166 : 100.

I then tried plain filters off the same sheet against each other in like funnels; usually the results varied but a few per cent, though sometimes much more; the greatest difference noticed was 2 to 1; several times the results corresponded exactly on repeated runs of 500 cubic centimetres.

Experiments were made to determine the difference in efficiency between the single and the triple sides of filters. No. 1 had its triple side covered with paraffin, leaving the single side free. No. 2 had the triple side free, while the single was covered; with paraffin the result was 175 : 100; glycerin was then tried with the resuult 200 : 100, showing the additional paper considerably retarded the flow.

I thought, since the adhesion of the water to the glass is the cause of slow filtration, I might increase the flow by coating the funnels on the inside with paraffin, to which water does not adhere. No. 1, being coated, No. 2, left clean, I got

	I.	II.		I.	II.
1st trial	200 :	100	3rd trial	100 :	100
2nd ,,	184 :	100	4th ,,	137 :	100

The filters in the third and fourth trials were the same, but the funnels were changed about.

The outside or skeleton filters, above described, may be cut the same size as the inner filter; if much smaller the upper part of the inner filter clings to the glass; if larger a part of the precipitate is liable to adhere to the outer filter, and even with great care a part of the precipitate would creep up and be lost.—*American Journal of Pharmacy.*

EXPLOSION OF NITROGLYCERINE, AT SYDNEY, NEW SOUTH WALES.

We have received the report of the Board appointed to enquire into the origin and causes of an explosion which occurred at Sydney, on the 4th of March, which is so full and comprehensive that we have made some extracts from it, for those of our readers who are interested in the subject. A case, containing 100 lbs. of nitroglycerine, had been sent from Messrs. Nobel and Co., of Hamburg, to Mr. Winckler, of Sydney, to whom they had offered the agency. It was laid in a cellar where there was no gas or anything of an explosive nature; but about four days after, this disastrous event occurred. In seeking for the cause of the explosion, the Board, consisting of the Hon. A. Campbell, M.L.C.; Professor J. Smith, M.D., of Sydney University; and Capt. J. McLerie, Inspector-General of Police, reviewed the following suppositions:—

(*a*). Some burglar may have entered the cellar, and in trying to break open the case, in the hope of finding spirits, may have caused the oil to explode. But if so, some remains of a human body would certainly have been found; and besides, it would have been difficult to break into the cellar, and there was nothing to tempt a burglar.

(*b*). Something may have taken fire in the premises and communicated to the oil. But if so, some sign of combustion must have remained besides the charring of the gunny-bags. There were no gunny-bags in the cellar; none, in fact, but in the loft above the store; and we may account for two or three being partially burnt by supposing that the small sample bottles of gunpowder had been broken and ignited by the explosion, and had then ignited the bags; or a box of matches may have been ignited and scattered among them. Whatever caused the charring of the gunny-bags, we are certain that they did not cause the explosion, nor were directly ignited by the explosion; they must have been ignited indirectly, and probably in one of the ways above indicated.

(*c*). The oil may have been exploded by a blow or concussion, as it is admitted by the manufacturers that a blow under certain conditions will cause an explosion. We can imagine only two ways in which such a blow could be given, supposing it proved that no person was in the cellar at the time. First, a stone may have been thrown in from the street; but this is impossible from the relative positions of the window and the box. Second, a heavy body may have fallen on the box. The only thing we could discover that could have fallen on it, was an upright post supporting one of the beams; but a careful inspection showed clearly that this post had been blown away from where it stood, the base being dislodged before the top, and breaking away a portion of the stone on which it had rested.

(*d*). The high temperature to which the oil was subjected in the cellar may have caused some sort of fermentation resulting in explosion. It is asserted by the patentees that it will not explode by the application of a lighted match, but that it will by a temperature of about 360°. But it may be conceived that a much lower temperature may cause a sort of fermentation ending in the same result. But this cellar was cool and well ventilated. The temperature of the air, on the 3rd and 4th of March, was not higher than 77°, and the cellar must have been several degrees lower; on the 22nd it was not higher than 75°, and on the 1st than 71½°. The oil must have been subjected to a higher temperature within the tropics; and on the day on which it was landed and taken to the Queen's Warehouse the thermometer at the Observatory rose to 101° in the shade.

They then report as follows:—

Having thus examined and dismissed various supposable external causes of the explosion, we are impelled to the conclusion that the cause lay within the oil itself—in other words, that it exploded spontaneously. This we allow is an unsatisfactory conclusion, and to many it may appear inadmissible. How, it may be asked, could this oil stand the rough handling and the vicissitudes of temperature that must have attended its progress from Hamburg to Sydney, and then, three days after being quietly deposited in a cool cellar, without any new condition supervening, spontaneously explode. Every effect must have a sufficient cause. To speak of this oil as exploding of its own accord, without external agency, is to endow it with a kind of conscious volition, leading it as it were to commit suicide! We admit the force of the objection, but call attention to the following considerations:—In a complex organic fluid like nitroglycerine, it is clear that the atoms are not in a condition of stable equilibrium,—their strongest chemical affinities are not satisfied—they are held together by some constraint, so to speak—in what we may call an unnatural sort of combination, and out of this combination they have a constant tendency to spring, and rearrange themselves in accordance with their stronger proclivities. For example, we know that oxygen has a much stronger attraction for carbon and for hydrogen than it has for nitrogen, and when the chemical affinities of these are fully satisfied, the carbon would be converted into carbonic acid, and the hydrogen into water in each case by union with oxygen; but in this nitroglycerine there is no carbonic acid or water, and there is good reason for believing that most of the oxygen is combined with nitrogen—hence the justification of the expression used above—that we have here an unnatural sort of combination, in which the stronger attractions are unsatisfied; and these unsatisfied attractions are really the source of the enormous potential energy locked up in the blasting oil.

Further, it is believed that the ultimate particles of all bodies (even the most solid) are in a state of continual motion, and this motion, in the case of compounds in a state of unstable equilibrium, tends to bring about, sooner or later, a condition of stable equilibrium, where the preponderating attractions are satisfied. Such changes (which are familiar to chemists) are usually slow and silent, but sometimes they are sudden and violent. In the case before us we have said that the particles are held by some constraint in an unnatural position; now, it is conceivable that, by the molecular motions spoken of, the constraining force is gradually weakened, until a period comes when it can no longer resist the strong affinities of the atoms, and they rush together suddenly, producing an enormous bulk of gaseous compounds at a very high temperature, and thus developing an immense mechanical force. If we suppose a strong spring to be kept down by a piece of wood in a condition of slow decay, it is clear that, sooner or later—though months or years may elapse—a moment must arrive when the restraining force must give way, and the spring will suddenly develope its long pent-up energy.

It is possible that the changes thus indicated in the nitroglycerine may have been promoted by the high temperature to which it must have been subjected during the voyage, and after its arrival in Sydney; and perhaps in a cooler climate it might be safely kept for an indefinite time; though there is reason to suspect that an explosion of nitroglycerine, mentioned in the newspapers as having occured last November, in Westphalia, was also spontaneous, or without external agency.

As to the question of culpability in connection with this remarkable accident, we have come to the conclusion that no one is particularly to blame. The importers had reason to believe that the blasting oil was not more dangerous than gunpowder, if so much so; and the quantity did not exceed what they would have been authorised by law to store of gunpowder; but among the dozen of Colonial Acts that affect gunpowder, we do not find one that affects the quantity that may be stored of any other explosive substance. Then there was no concealment of the nature of this substance. We find that the "sight entry" describes the cases as containing "blasting oil," and Mr. Winckler explained to the Collector of Customs that it was a substitute for blasting powder. No doubt there was an infringement of the Colonial Act 18 Victoria, No. 21, which provides that no gunpowder, or other explosive material, shall be shipped or delivered, without a plain and durable brand or superscription on the package, showing the nature and quantity of the contents; but who could be proceeded against? The shippers are not in our power, and the captain of the ship that "delivered" the cases knew them only as containing "merchandise." But supposing the case had been legibly marked "blasting oil," there is no reason to suppose that it would have been treated otherwise than it was; for the above-named Act provides only for the branding, and no other Act that we can find refers at all to other explosive materials than gunpowder.

In regard to the expected importation of 200 lbs. of nitroglycerine, we adopt and recommend the suggestion of the Collector of Customs—that the ship having this substance on board should not be allowed to come further up the harbour than Fort Denison, until the nitroglycerine be either thrown overboard, or landed on some unpeopled spot; and we further recommend to the Government to take such steps as may be necessary to ascertain, before arrival, the name of the ship by which this substance has been sent, and to give due warning and instructions to the pilots.

The question of the storage of future importations has engaged our special attention. It may turn out to be a question of no practical importance, as we hope that the serious accident that has occurred will have the effect of altogether stopping importations; but in the mean time we recommmend that an Act should be passed without delay, providing that no nitroglycerine shall be kept in any inhabited house, and that no quantity greater than two pounds shall be stored under any one roof within the city or suburbs unless it be over 100 yards from any inhabited house; and that no quantity greater than 10 lbs. shall be stored within 300 yards of any inhabited house or public thoroughfare. Quantities greater than 10 lbs. must not be stored within the city or suburbs, but must be placed in a proper magazine, in such a position, and at such a distance from habitations or thoroughfares, as would ensure the public safety.

RELATIVE VALUES OF FRENCH AND ENGLISH WEIGHTS AND MEASURES.

By A. A. FESQUET.

WEIGHTS.

Milligramme	=	0·015438395	troy grain
Centigramme ..	,,	0·15438395	,, ,,
Decigramme	,,	1·5438395	,, ,,
Gramme	,,	15·438395	,, grains
,, ...	,,	0·643	pennyweight
,, ...	,,	0·03216	oz. troy
,, ...	,,	0·03527	oz. avoirdupois
Decagramme	,,	154·38395	troy grains
,, ...	,,	5·64	drams avoirdupois
Hectogramme ..	,,	3·2154	oz. troy
,, ..	,,	3·527	oz. avoirdupois
Kilogramme	,,	2·6803	lb. troy
,, ...	,,	2·205486	lb. avoirdupois
Myriagramme ..	,,	26·803	lb. troy
,, ..	,,	22·05486	lb. avoirdupois
Quintal metrique ..	,,	100 kilog. =	220·5486 lb. avoir.
Tonne	,,	1000 ,, ,,	2205·486 ,,

Different authors give the following values for the gramme:—

Gramme =	15·44402	troy grains
,, ,,	15·44242	,,
,, ,,	15·4402	,,
,, ,,	15·433159	,,
,, ,,	15·432	,,

AVOIRDUPOIS.

Long ton = 20 cwt. = 2240 lb. =	1015·649	kilogrammes
Short ton (2000 lb.) .. .. ,,	906·8296	,,
Hundredweight (112 lb.) .. ,,	50·78245	,,
Quarter (28 lb.) .. .. .. ,,	12·6956144	,,
Pound = 16 oz. = 7000 gr. ,,	453·4148	grammes
Ounce ,, 16 drams ,, 437·5 gr. ,,	28·3375	,,
Dram ,, 27·344 grains ,,	1·77108	gramme

Troy (precious metals).

Pound = 12 oz. = 5760 gr. =	373·096	grammes
Ounce ,, 20 dwt. ,, 480 ,, ,,	31·0913	,,
Pennyweight .. ,, 24 ,, ,,	1·55457	gramme
Grain .. .. ,, ... ,,	0·064773	,,

Troy (pharmacy).

Ounce = 8 drams = 480 gr. =	31·0913	grammes
Dram ,, 3 scruples ,, 60 ,, ,,	3·8869	,,
Scruple ,, ... 20 ,, ,,	1·29546	gramme

CARAT WEIGHT FOR DIAMONDS.

1 carat = 4 carat grains = 64 carat parts
,, ,, 3·2 troy grains
,, ,, 3·273 ,,
,, ,, 0·207264 gramme
,, ,, 0·212 ,,
,, ,, 0·205 ,,

Great diversity in value.

Symbols for Abbreviations.

Unit	metre m.	gramme g.	litre l.	are a.
M Myria	Mm	Mg	Ml	
K Kilo	Km	Kg	Kl	
H Hecto	Hm	Hg	Hl	Ha
D Deca	Dm	Dg	Dl	Da
d deci	dm	dg	dl	da
c centi	cm	cg	cl	ca
m milli	mm	mg	ml	

Km = kilometre. Hl = hectolitre.

cg = centigramme. c.cm = $\overline{cm}^{3}$ = cubic centimetre.

$\overline{dm}^{2}$ = sq. dm = square decimetre.
kgm = kilogrammetre.
kg° = kilogramme degree.

No. of Units.	Francs into Shillings.	Pence into Centimes.	Shillings into Francs.	Pounds sterling into Francs.
1.	0·79365	10·5033	1·2604	25·2080
2.	1·58730	21·0066	2·5208	50·4160
3.	2·38095	31·5099	3·7812	75·6240
4.	3·17460	42·0132	5·0416	100·8320
5.	3·96825	52·5165	6·3020	126·0400
6.	4·76190	63·0198	7·5624	151·2480
7.	5·55555	73·5231	8·8228	176·4560
8.	6·34920	84·0264	10·0832	201·6640
9.	7·14285	94·5297	11·3436	226·8720
10.	7·93650	105·0330	12·6040	252·0800

Conventional value of 1 shilling .. .. = 1·26 franc.
Mint value (previous to 1818) .. .. .. ,, 1·24 ,,
,, (after 1818) .. .. .. ,, 1·16 ,,
1 franc = 100 centimes = 20 sous.
5 centimes = 1 sou. 10 centimes = 1 decime = 2 sous.

No. of Units.	Inches into Centimetres.	Feet into Metres.	Miles into Kilometres.
1.	2·539954	0·3047945	1·6093
2.	5·0799	0·6095890	3·2186
3.	7·6199	0·9143835	4·8279
4.	10·1598	1·2197680	6·4373
5.	12·6998	1·5239724	8·0466
6.	15·2397	1·8287669	9·6559
7.	17·7797	2·1335614	11·2652
8.	20·3196	2·4383559	12·8745
9.	22·8596	2·7431504	14·4838
10.	25·3995	3·0479450	16·0930

No. of Units.	Square Ft. into Square Metres.	Cubic Ft. into Cubic Metres.	Pounds per square Inch into Kilogrammes per square Centimetre.
1.	0·09290	0·028314	0·0702774
2.	0·18580	0·056628	0·1405548
3.	0·27870	0·084942	0·2108322
4.	0·37160	0·113256	0·2811096
5.	0·46450	0·141570	0·3513870
6.	0·55740	0·169884	0·4216644
7.	0·65030	0·198198	0·4919418
8.	0·74320	0·226512	0·5622192
9.	0·83610	0·254826	0·6324966
10.	0·92900	0·283140	0·7027740

No. of Units.	Kilogrammes per sq. Millimetre into Pounds per sq. Inch.	Pounds into Kilogrammes.	Long Tons into Tonnes of 1000 Kilog.
1.	1425·45	0·4534148	1·015649
2.	2850·90	0·9068296	2·031298
3.	4276·35	1·3602444	3·046947
4.	5701·80	1·8136592	4·062596
5.	7127·25	2·2670740	5·078245
6.	8552·70	2·7204888	6·093894
7.	9978·15	3·1739036	7·109543
8.	11403·60	3·6273184	8·125192
9.	12829·05	4·0807332	9·140841
10.	14254·50	4·5341480	10·156490

Thermometers.

Celsius or Centigrade.	Fahrenheit.	Réaumur.
− 15° C.	+ 5° F	− 12° R
,, 10	,, 14	,, 8
,, 5	,, 23	,, 4
0	,, 32	0
+ 5	,, 41	+ 4
,, 10	,, 50	,, 8
,, 15	,, 59	,, 12
,, 20	,, 68	,, 16
,, 25	,, 77	,, 20
,, 30	,, 86	,, 24
,, 35	,, 95	,, 28
,, 40	,, 104	,, 32
,, 45	,, 113	,, 36
,, 50	,, 122	,, 40
,, 55	,, 131	,, 44
,, 60	,, 140	,, 48
,, 65	,, 149	,, 52
,, 70	,, 158	,, 56
,, 75	,, 167	,, 60
,, 80	,, 176	,, 64
,, 85	,, 185	,, 68
,, 90	,, 194	,, 72
,, 95	,, 203	,, 76
,, 100	,, 212	,, 80

1° C = 1° 8 F = $\frac{9}{5}$° F = 0° 8 R = $\frac{4}{5}$° R.

1° C × $\frac{9}{5}$ = 1° F | 1° F × $\frac{5}{9}$ = 1° C | 1° R × $\frac{9}{4}$ = 1° F
1° C × $\frac{4}{5}$,, 1° R | 1° F × $\frac{4}{9}$,, 1° R | 1° R × $\frac{5}{4}$,, 1° C

Calorie = Unit of heat.
,, ,, Kilogramme degree = Kg°.
It is the quantity of heat necessary to raise of 1° C. the temperature of 1 Kilogramme of distilled water.

Kilogrammetre = Kgm. = the power necessary to raise 1 Kilogramme 1 metre high in 1 second. It is equal to $\frac{1}{75}$ of a French horse-power.

FOREIGN SCIENCE.

Paris, June 17, 1868.

Estimation of phosphoric acid.—Oxidation of alcohol.—Aniline black varnish.—Treatment of beet-root pulp.—Extraction of sugar from beet-root juice and molasses of all descriptions. Academy of Sciences: Combustion of oil.—Chemical harmonica.

Reference has already been made to M. Schlœsing's process of determining phosphoric acid; the following is the manner in which the experiment is made:—A tube of green glass is fashioned before the blowpipe, so that there is first a part (A) of 30 centimetres in length, which rests horizontally upon wire gauze, and where the reactions take place; then a part drawn out of about 15 centimetres in length, inclined downwards and backwards, after which the tube is continued horizontally, and with the original diameter, for the length of 10 centimetres. Thus there is formed a kind of bulb (B), terminated by a recurved point, destined to condense the chloride of phosphorus. At the end of the part A, a tuft of asbestos is placed, and upon this pure chloride of potassium, decrepitated and coarsely powdered: this chloride occupies from 12 to 15 centimetres, and is maintained in its place by another very small tuft of asbestos. Afterwards the porcelain boat, containing the phosphide in fragments, with a last tuft of asbestos, is introduced. Finally, the end of the tube is closed by a cork carrying a little piece of tube. In the bulb, B, a little water is placed, and a vertical tube containing fragments of moistened porcelain is attached, where phosphoric vapours not condensed in B will be condensed. After this arrangement comes a little washing flask, serving to show the current of chlorine. After having heated the

chloride of potassium, and driven every trace of moisture out of A, by a current of dry air, the chlorine is admitted; but the porcelain boat is not heated till the tube has been swept through with dry air. As soon as the reaction commences, a red liquid is condensed round the boat, and spreads over the chloride of potassium. This chloride ought to be heated, but only near the boat, to a sufficiently high temperature to melt the double chloride, otherwise the tube will become closed up. Towards the end of the operation the heat is raised a little, without at the same time allowing it to reach dull redness, for at this degree of heat the phosphorus exchanges its chlorine for the oxygen of the silica of the glass, and forms phosphates. The perchloride of phosphorus is condensed at the end of the tube A; by application of a gentle heat it is chased into the bulb. The experiment is finished when the least trace of condensation is no longer observed. A constant but feeble excess of chlorine should be maintained throughout, and the operator requires to have the chlorine easily at command; for this, the arrangement with a couple of tubulated flasks and pinchcocks is convenient.

To determine the phosphoric acid condensed in the bulb with hydrochloric acid the glass is cut in the narrowed part, the liquid transferred to a porcelain dish, and the washings of the bulb and the tube with porcelain united, nitric acid added, and the whole evaporated. The hydrochloric acid, decomposed towards the end of the operation, is eliminated without loss. All that is now required is to estimate free phosphoric acid in the presence of nitric acid, and for this purpose M. Schlœsing employs nitrate of silver.

It has been frequently remarked by vinegar makers that the acetification of alcohol in the casks does not proceed regularly, and that the corresponding strength of acid is not obtained. To facilitate oxidation, M. Artus dissolves 15 grammes of dry chloride of platinum in 2·5 kilogrammes of alcohol, and in the solution places 1·5 kilogramme of pieces of wood charcoal in pieces of the size of a nut; he then calcines in a covered crucible. This done, he takes 0·75 kil. of platinised carbon, and strews it upon a wooden cover pierced with holes, placed over the open top of a vinegar cask, 2 metres in height and of from 0 m. 80 to 0 m. 90 in diameter; the vinegar is not allowed to moisten directly the cover with the platinised carbon. After five weeks' work, the platinised carbon is calcined afresh. The action of this carbon is very remarkable, the acetification is effected more rapidly and completely than in operating in the ordinary way, and the vinegar possesses a more agreeable flavour.

An aniline black varnish, of recent Parisian production, is the following:—In a litre of alcohol 12 grammes of aniline blue, 3 grammes of fuchsine, and 8 grammes of naphthaline yellow, are dissolved. The whole is dissolved by agitation in less than twelve hours. One application renders a white object ebony black; the varnish can be filtered, and will never deposit afterwards. The three colours are not destroyed, for each can be separated by analysis with the characteristic properties.

A modification in the treatment of beet-root pulp has been introduced by M. Champonnois. The experiments relative to the process were made in the laboratory of MM. Périer and Persoz, and subsequently repeated in the laboratory of the *Conservatoire des Arts et Métiers*. The quantity of beet-root operated upon in each case was two kilogrammes. This amount was grated, 30 per cent of water added, and the pulp pressed in the usual way. The juice was filtered through paper, concentrated, with addition of 1 per cent of fine animal black, purification which is considered equivalent to an ordinary filtration, working with coarser material. This syrup was concentrated to 22° Beaumé, filtered, and heated to 115° C., then left on a stove during five or six days; the crystallised mass was then freed from mother liquor. For the second, and for all following operations, this drained syrup, or molasses, was diluted with about 60 per cent of water for the weight of beet-root; this solution heated on the water-bath, and mixed with the pulp of 2 kilogrammes of beet-root, was maintained from ten to fifteen minutes at a temperature of 70° to 80°. The mass was then treated precisely as in the former case. The smallest particles of syrup adhering to the vessel crystallise completely and in large crystals. In the regular process of manufacture, the syrup removes about double the amount of albumen and salts, retained by the pulp; all these things are separated in purification, or remain in the molasses, and are consequently lost as alimentary matters. The pulps obtained in the new process, on the contrary, retain all these matters, and are much smaller in weight than the ordinary pulps; they can then, being returned to the farm, yield valuable materials, particularly the salts, which are rapidly removed from the soils upon which beet-root is cultivated; even this is the case when the quantity of ordinary pulp proportional to the beet-root which the matters in the soil have produced, is restored to the soil, for this proportion corresponds only to one-third, at the most, of the useful principles contained in the beet-root. By the continued recharging of the syrups, the work relating to bye-products is suppressed, work which requires a large outlay in cisterns and very large refineries. The only inconvenience that is met in the process advocated, resides in the proportion of water, which is double that commonly employed; but the increase in expense is alone due to carbon, and amounts to 1 franc more for 1,000 kilogrammes of beet-root: it is true that the apparatus destined to contain the juice is required to be somewhat larger. These facts are insignificant in comparison with the advantages of the new method, considered both in regard to the farm and the factory.

Apropos of the beet-root manufacture, M. Leplay's method of extracting sugar from juice as well as syrups and molasses of all descriptions, may be here referred to. M. Leplay sought to extract the sugar from the matters in question, by transforming it into insoluble sucrate of lime, which has not yet been made on an industrial scale. This combination is effected in the saccharine fluids treated, less by an addition of ready formed free lime, than by the aid of solutions of calcareous salts, and particularly of chloride of calcium, and of caustic soda, which precipitates the lime, and this combines and is precipitated with the sugar. The sucrate of lime, after precipitation, is decomposed by means of carbonic acid, the soda in the solution regenerated, and the carbonic acid obtained as a secondary product in the formation of the chloride of calcium. A few additional statements of the author will render the precise nature of the process more evident.

When a solution of sugar in water is saturated with all the lime which it is capable of absorbing, and boiled, there is formed a white precipitate of sucrate of lime, which is redissolved on cooling. The quantity of sugar thus eliminated is only a small proportion of that present, and the greater part remains in solution when the precipitate is separated from the liquid during ebullition. Feebler still, is the proportion of sugar that can be extracted from beet-root juice or molasses, and the more impure the saccharine fluid is, the less considerable is the separation obtained by this means. On the contrary, the whole of the sugar may be precipitated in the state of sucrate of lime, when in the solution already saturated with lime, a fresh quantity of lime is separated in the nascent state, then the precipitation is independent of the degree of purity of the saccharine solutions. Besides the juice of the beet-root, the molasses of sugar refineries are capable of treatment by the process. The quantity of soluble salts of lime present in the syrups, &c, exerts an influence on the proportion of calcareous salt which should be added; as these salts contain organic acids, the soda is no longer found in corresponding quantity, as chloride of sodium, but as carbonate of soda in the ash obtained from the mother liquor,—an important advantage. The previous saturation of the liquor submitted to treatment by pure lime, before the addition of the calcareous salt, is then essential, since by this means a portion of the calcareous salt which would be otherwise required, is replaced by

the cheaper material, lime. The sucrate of lime precipitated is separated from the liquor, washed with water, and decomposed by carbonic acid.

M. Scheurer-Kestner, in his memoir "On the combustion of oil," presented to the Academy, states that his researches are divided into three parts: (1) chemical study of the gas resulting from the combustion of oil, estimation of the combustible gases, and of the lampblack; (2) calorimetric studies, heat from the combustion of oil, relation between the chemical composition and the calorific power; (3) calculations and practical results, study of the distribution of caloric in the heating of steam generators. The lampblack was estimated by causing a certain volume, 50 or 100 litres, of the gaseous products of combustion to pass through a glass tube filled with asbestos, which retains this carbon completely. The tube was afterwards heated to redness, a current of oxygen passed through, and the carbonic acid formed collected in potash. It results from the analysis of the gas, that when the supply of air is insufficient, that is to say, when the gaseous products only contain six to ten per cent of air in excess, the loss of carbon in the state of combustible gas represents about 20 per cent of the carbon of the oil consumed, and that this loss diminishes considerably when the burnt gases contain 20 to 50 per cent of air in excess, becoming then reduced to 4 or 5 per cent. The loss of hydrogen is more considerable: it oscillates between 10 and 20 per cent of the hydrogen contained in the oil, and depends less on the supply of air. In fact, the disengagement of hydrogen takes place by distillation. The presence of acetylene, as well as of other hydrocarbons, has been proved. Many tables of analyses occur in the memoir. The oil experimented with was that of Ronchamp.

The following occur among the conclusions to a note "On the chemical harmonica," by M. Terquem. Relatively to the form of the flame, three cases may present themselves:—(1.) If the flame be long enough, and the current of air of little intensity, the vibratory motion does not reach the base of the flame; examined by the turning mirror, the reflected image presents the appearance of a continuous sinusoïdal curve. (2.) If the current of air, and consequently the vibratory motion, be more intense, the flame vibrates in its whole length, and can even become completely extinguished; in this case, with the turning mirror, the images of the flame are quite distinct, the gas re-lights itself by reason of the high temperature reigning at the point. (3.) If the vibratory motion become still more intense, there is observed in the disengagement tube a little reversed flame, which, seen in the turning mirror, alternates with the external flame; this almost exceptional phenomenon was the point of departure in the theory proposed by Schrötter.

CORRESPONDENCE.

UTILISATION OF WATER POWER.

To the Editor of the Chemical News.

Sir,—The question of our coal supplies must interest every one who has the prosperity of his country at heart; and Professor Jevons's lecture at the Royal Institution, printed in your Journal of 8th and 15th May, is calculated to produce, if not fear, at least thoughtfulness for the future.

It seems to me that one very obvious source of power, continually passing before our eyes as if inviting thoughtful consideration, has been thrown very much to one side as unworthy of taking a place beside steam. I refer to water power. This neglect may have been of little consequence hitherto, when coals were so plentiful and cheap; but now, when there is every prospect of coals becoming both scarce and dear, and when our growing knowledge of the convertibility of force tends to render the cheapness of the first power everything, and direct application a secondary matter, this neglect becomes a matter of importance.

Every one living in the neighbourhood of a river must be struck by the vast amount of force that passes *unutilised* down the river course during floods. Reckoned up in horse power or foot pounds it must be something enormous even in our smaller rivers. Can this force be *utilised?* The Dutch have wrested land from the depths of the sea. Is it too great a task for the English to hold back the winter floods from the ocean, and only let them out after performing their alloted task? In truth this has been done to a small extent already in our various water works, and I would suggest its being carried out to an extent worthy of the energies of this great nation meeting a threatened difficulty, and subduing the forces of nature to its own use. With the aid of the capital and enterprise of the nation, I should think there is no scientific difficulty in constructing large reservoirs at the sources of our principal manufacturing rivers, supplemented, if necessary, by smaller reservoirs along their courses at suitable points. In our northern rivers, at any rate, there are many natural basins which could with little difficulty be turned into reservoirs capable of storing abundant supplies for the largest manufactories, and keeping these supplies constant during the whole year. By this means one great objection to water power, namely its variability, would be removed.

In the application of water power also much might be done. We are merely on the threshold, so to speak, of our knowledge of the convertibility of force, and for those purposes where the direct application of water power would not be advantageous, it might be converted economically into any other force, such as electricity or heat, without making an impossible demand on our scientific skill.

Mr Jevons seems to think electricity can never supersede coal, because coal is likely to be the cheapest source of electricity, but if electricity were derived from water power this difficulty would be removed.

Were some such plan as I have sketched adopted, this country would be in possession of a power which would last while the sun shone and rivers flowed, and the seats of industry would not be the plains of Africa or Australia, as Professor Jevons hints, but would still be in "this cloud obscured Isle,"—and because it is cloud obscured.

I am &c.,

W. M. Durham.

Currie, 10th June, 1868.

MISCELLANEOUS.

Destruction of Nobel's Nitroglycerine Manufactory.—By telegram from Stockholm we learn that Nobel's nitroglycerine manufactory was blown up on the afternoon of the 10th inst. Fifteen persons were killed and several seriously injured, and great destruction was caused in the neighbourhood of the works.

Chemical Society at Newcastle-upon-Tyne.—We hear that Mr. R. Calvert Clapham and Mr. Freire Marrico are making arrangements to establish a Chemical Society at Newcastle. There are a considerable number of practical chemists in the neighbourhood, and this movement will, in all probability, be well supported. Such a Society, well managed, cannot fail to promote the advancement of practical and theoretical chemistry, and we trust that our columns will soon record the proceedings of the Newcastle Chemical Society, in addition to those of the other learned societies.

A New Lamp.—The French, who were always strong in "lamps," have lately brought out a new invention, which is said to be as brilliant as the oxy-hydrogen and lime lights, while it has the recommendation of being much less costly. Coal gas, intimately mixed with air, is urged with gentle pressure along a tube, and made to pass through a metallic plate, pierced full of minute holes. By this means a vast number of jets are obtained, which, after being driven through a fine tissue of platinum wire, are lighted in the ordinary way. The platinum soon acquires a white heat, and gives out so brilliant a light that it cannot be supported by the naked eye. About one metre of gas is consumed per hour. It is called the *Bourbouze* lamp.—*Oil Trade Review.*

On the Means of Recognising the Source of an Alcohol.—The source of an alcohol is usually ascertained by pouring a small quantity into the palm of the hand, and allowing it to evaporate; as the alcohol is more volatile than the empyreuma, the odour of the latter reveals the origin of the alcohol when the evaporation is almost terminated. But this process is very imperfect, as the alcohol may dissolve fatty substances from the hand, which will modify its odour. It is better to operate in a glass or porcelain capsule; but the following plan is still more safe:—Mix the alcohol with an equal quantity of ether, and then add a volume of water equal to that of the mixture. The ether dissolves the empyreuma, and carries it with it when separating with the rest of the liquid. Then evaporate this ether in a porcelain capsule, and the residue gives the empyreumatic odour so characteristic that it cannot be mistaken. Rum, arrack, cognac, potato, or wood spirit may thus be easily distinguished. The test only occupies a few minutes; but rectified ether must be employed, as common commercial ether also leaves an odourous residue on evaporation.—*Neue Gewerbeblätter aus Kurhessen.*

Dinner to Mr. Watts.—On the 15th instant a complimentary dinner was given at the Freemasons' Tavern to Mr. Henry Watts, F.R.S., by several of his friends and coadjutors, in celebration of the completion of his "Dictionary of Chemistry." The chair was taken by Dr. Odling, and there were also present Dr. Roscoe, Dr. Guthrie, Mr. Hanhart, Prof. Cary Foster, Dr. Atkinson, Mr. F. Field, Dr. H. Müller, Mr. David Forbes, Mr. A. P. Price, Dr. M. Foster, Dr. Russell, Dr. Gilbert, Dr. Redwood, and some others. Mr. De la Rue, Dr. Frankland, Mr. Abel, and Mr. Greville Williams were unfortunately prevented from attending. In proposing the toast of the evening, the chairman remarked upon the advantages which occurred to chemists from their having compressed into five goodly volumes an accurate abstract of the immense mass of chemical knowledge which had gone on accumulating up to the present time, and he complimented Mr. Watts upon the thoroughness and conscientiousness with which this part of his labours had been performed—that he had not been content with giving crude abstracts of scarcely more crude material, but had produced a complete system of singularly well digested abstracts, bearing upon them the stamp of his own individuality. The chairman further observed that, in addition to its being a storehouse of carefully abstracted knowledge, the dictionary was, in addition, a collection of high-class treatises upon all subjects of great chemical interest; and, although many of these treatises had been contributed by friends and associates of Mr. Watts, who had been glad to support him in his arduous undertaking, others of them, and these not the least meritorious, were the work of Mr. Watts himself, and gave evidence alike of his extensive knowledge and clear philosophical conception. The health of Mr. Watts was then drunk with much enthusiasm; other toasts followed, including "Success to the Messrs. Longmans," and the party broke up after an evening of much enjoyment.

CONTEMPORARY SCIENTIFIC PRESS.

(Under this heading it is intended to give the titles of all the chemical papers which are published in the principal scientific periodicals of the Continent. Articles which are merely reprints or abstracts of papers already noticed will be omitted. Abstracts of the more important papers here announced will appear in future numbers of the "Chemical News.")

Journal für Praktische Chemie.

Nos. 23-24. 1867.

G. ROSE, "On the Preparation of Crystallised Bodies before the Blowpipe (Continuation). On the Behaviour of Titanic Acid towards Borax, and on the Preparation of Rutile and Amorphous Titanic Acid. On the Behaviour of Oxides of Iron towards Borax, and on the Preparation of Crystallised Hæmatite and Magnetic Iron Ore. On the Behaviour of Titaniferous Iron Ore towards Borax, and on the Preparation of Crystallised Titaniferous Iron Ore and Titaniferous Magnetic Oxide of Iron. On the Preparation of Rutile by Fusing Titanic Acid and Titaniferous Iron Ore with Phosphate of Soda and Ammonia." R. HERMANN, "Answer to C. Marignac's Remarks* on the Author's Researches on the Atomic Weight of Niobium and Ilmenium. On Rewdanskite, a new Nickel Ore, and on the Extraction of Nickel from that Mineral." F. VON KOBEL, "On Glaucodote from Hakansbö, Sweden." HENNING, "On the Theory of the Removal of Sulphur from Gas by Oxide of Iron and of the Revivification of the same." HILGER, "On the Chemical Composition of the Shell and some soft Parts of living Brachiopods." A. FROEHDE, "On the Identity of Hydrocarotine and Cholesterine." A. E. NORDENSKOLD, "On the Selenium Minerals from the Copper Mine of Skrikerum, in the Province of Calmar, Sweden."

No. 1. 1868.

A. MULLER, "On the Dialytic Solution of Caseine and Starch." A. CLAUS, "On the Behaviour of Acroleine towards Hydrate of Potash. Contributions to the Knowledge of Oxanilic Acid."

Dingler's *Polytechnisches Journal.*

No. 3. 1868.

A. OTT, "On the Preparation of Phenic Acid." H. PERUTZ, "On the Detection of Butyric Acid in Glycerine, and on its Separation from that Substance." A. SPAN, "On G. C. Nöllner's Method of Estimating Nitric Acid."

Annales du Génie Civil.

February, 1868.

A. D'ADHEMAR, "On the Deposits of Phosphate of Lime at the Island of Sombrero, Antilles." STOSS, "On the Use of Oxide of Chromium for Polishing Steel." ZIEGLER, "A Substitute for Animal Charcoal." SCHNEIDER, "A Method of Purifying Coal Gas by means of Iron Filings." GONDOLO, "A Modification of Boussingault's Process for Manufacturing Oxygen and Nitrogen by passing a Current of Atmospheric Air over Caustic Baryta." SCHWARZ, "A Process for Manufacturing Seltzer Water from Carbonate of Magnesia without the Use of an Acid." PICARD, "A Process for Extracting Grease from Waste Leather." AUBERTIN and BOBLIQUE, "On obtaining Phosphorus from Bones by means of Silica and Pounded Charcoal."

Comptes Rendus.

March 2, 1868.

JUETTE "On the Estimation of Tartaric and Malic Acid by means of Iron, Aluminium, Manganese, &c." J. PERSONNE, "Chemical Researches on the Composition of Roasted Coffee." DESCHAMPS, SEN., "On the Volatile Oils contained in Brandy.—Contributions to the Knowledge of Iodide of Potassium and some other Iodides."

March 9, 1868.

H. DEVILLE, "First Memoir on the Physical Properties and Calorific Power of Petroleum and other Mineral Oils." FIZEAU, "On the Dilatation of Paraffin and Naphthaline." E. FREMY and TERREIL, "On a general method for the immediate Analysis of Vegetable Tissues." A. PAYEN, "On the Properties and on the Extraction of Diastase." A. W. HOFMANN, "On the Member of the Naphthalic Series which corresponds to Benzoic Acid." ISNARD. "A process for Determining the Equivalent of Aluminium." BECHAMP, "On the Propionic Fermentation of Succinic and Malic Acid."

Monatsbericht der Koniglich Preussischen Akademie der Wissenschaften zu Berlin.

November, 1867.

RAMMELSBERG, "On the Composition of the Salts of Periodic Acid."

Archives des Sciences.

February 15, 1868.

C. MARIGNAC, "On the Reduction of Niobium and Tantalum Compounds."

Annalen der Chemie und Pharmacie.

February, 1868.

R. FITTIG, A KÖBRICH, and T. JIKE, "On the Decomposition of Camphor by Chloride of Zinc in Fusion." J. H. EATON and R. FITTIG, "On the Cyanogen Compounds of Manganese." O. MALIN, "On Isodulcitic Acid.—Contributions to the Knowledge of Camphor." H. HLASIWETZ and A. GRABOWSKI, "On the Decomposition of Camphoric Acid by Caustic Potash in Fusion." W. HEINTZ, "On the Action of Iodide of Ethyl on Glycol Compounds and Diglycolamidic Acid Compounds, and on a new mode of formation of Diethylglycocol and Ethylglycolamidic Acid." C. HERRMANN, "On the Constitution of Ferrocyanide of Cadmium and Potassium." A. CLAUS, "On the Reduction of Oxalic Acid."

* See *Journal für Praktische Chemie*, vol. 101, p. 464.

Poggendorff's *Annalen der Physik.*
No. 1. 1868.

A. Forster, "An account of some Methods of Preparing Phosphorescent Compounds." T. Wimmel, "On the Melting Points of Fats, and on their behaviour during Solidification." C. Schultz, "On the Constitution of the Sulphates." C. Von Hauer, "On the Hydrates of the Double Sulphate of Cadmium and Potash." W. Wernicke, "On Gilding Glass Specula."

Bulletin de la Société Industrielle de Mulhouse.
February. 1868.

A. Scheurer-Kestner, "Memoir on the Extraction of Sulphur from Soda Waste."

PATENTS.

Communicated by Mr. Vaughan, F.C.S., Patent Agent, 54, Chancery Lane, W.C.

GRANTS OF PROVISIONAL PROTECTION FOR SIX MONTHS.

3463. S. Perkins and W. Smellie, Gorton, near Manchester, "Improvements in the manufacture of malleable metal of a steely quality, partly from Bessemer "scrap" or other Bessemer metal."—December 5, 1867.

3499. L. Rose, Leith, N.B., "An improved mode of preserving vegetable juices."—December 9, 1867.

3517. A. M. Clark, Chancery Lane, "An improved process for the production of tin so as to render it applicable for coating metals, and for other purposes."—A communication from J. Feuquières, Boulevart St. Martin, Paris.—December 11, 1867.

3542. E. R. Sintzenich, Duke Street, St. James's, Middlesex, "Improvements in the treatment of gutta-percha, india-rubber, Honduras gum, and the other allied gums, for the production of a preparation or preparations applicable as a varnish, cement, paint, stain, water and air-proof material, and for other purposes."—A communication from D. Reed, New York.—December 13, 1867.

3559. J. Hargreaves, Appleton-within-Widnes, Lancashire, "Improvements in the manufacture of soda and potassa."

3556. A. M. Clark, Chancery Lane, "Improvements in the extraction of ammonia from fermented and other liquids, and in the regeneration of the agents used in such extraction."—A communication from A. Coste and L. T. De Rosnay, Boulevart St. Martin, Paris.—December 14, 1867.

3581. W. Huskisson, jun., Swinton Street, Gray's Inn Road, Middlesex, "Improvements in the manufacture of bicarbonate of potash and soda."—December 16, 1867.

3652. F. A. Abel, Woolwich, Kent, "Improvements in the production of explosive compounds."

3657. A. M. Clark, Chancery Lane, "An improved colouring matter, chiefly applicable for dyeing and printing purposes."—A communication from J. T. Coupier, Boulevart St. Martin, Paris.

3663. J. Addie, Coatbridge, Lanarkshire, and F. Kolm, Westminster, Middlesex, "Improvements relating to blast and cupola furnaces."

3667. G. J. Hinde, Wolverhampton, Staffordshire, and T. C. Hinde, Ynispenllwch, near Swansea, Glamorganshire, "Improvements in the manufacture of iron and steel, and in furnaces and apparatus used in the said manufacture."—December 24, 1867.

112. T. Whitwell, Stockton-on-Tees, "Improvements in furnaces."—January 11, 1868.

215. J. H. Johnson, Lincoln's Inn Fields, Middlesex, "Improvements in furnaces."—A communication from H. Ross, Pittsburgh, Allegheny, Penn., U.S.A.—January 21, 1868.

493. W. R. Lake, Southampton Buildings, Chancery Lane, "An improved mode of, and apparatus for, extracting and condensing the volatile portions of ores."—A communication from F. Formhals, San Francisco, California, U.S.A.—February 13, 1868.

731. T. Johnson, Runcorn, Cheshire, "Improvements in treating ores to obtain copper therefrom, and in evaporating brine."—March 3, 1868.

759. W. Hunt, Castleford, Yorkshire, "Improvements in treating certain compounds of copper and iron, and in utilising residual products obtained in heating compounds of copper and iron."—March 5, 1868.

785. J. Houston, Glasgow, N.B., "Improvements in the manufacture of composite candles, and in the machinery or apparatus employed therein."—March 6, 1868.

830. C. Attwood, Wolsingham, Durham, "Improvements in the production or manufacture of steel and iron of a steely character."—March 10, 1868.

891. W. E. Newton, Chancery Lane, "Improvements in the manufacture of illuminating gas in the distillation of hydrocarbons, and the making of gaseous fuel for heating purposes."—A communication from L. Stevens, Washington, Columbia, U.S.A.—March 16, 1868.

923. B. E. R. Newlands, F.C.S., Charlton, Kent, "Improvements in treating and obtaining products from spent oxide of iron which has been used in gas works or otherwise, to separate sulphur from sulphuretted hydrogen, and in the purification of sulphur obtained from such oxide of iron."—March 18, 1868.

1078. J. H. Johnson, Lincoln's Inn Fields, Middlesex, "Improvements in the manufacture of cast steel, and in furnaces to be employed therein, which furnaces are also applicable to the remelting of metals generally."—A communication from F. Ellershausen, Ottawa, Canada.—March 30, 1868.

1130. J. H. Johnson, Lincoln's Inn Fields, Middlesex, "Improvements in the manufacture of cast steel and malleable iron from cast iron, and in the apparatus or means employed therein."—A communication from F. Ellershausen, Ottawa, Canada.—April 3, 1868.

1137. H. Cochrane, Middlesbrough-on-Tees, Yorkshire, "Improvements in blast furnaces."—April 4, 1868.

1222. T. Forster, Streatham, Surrey, "Improvements in the manufacture of compounds of india-rubber, gutta-percha, balata, parkesine, solid paraffin or vegetable oils, and vegetable fibre."—April 13, 1868.

1137. H. Cochrane, Middlesbrough-on-Tees, Yorkshire, "Improvements in blast furnaces."—April 4, 1868.

1157. J. Radcliffe, Consett Iron Works, Durham, "Improvements in processes and means employed in the manufacture of iron and steel."—April 6, 1868.

1181. J. James, Ebbw Vale, Monmouthshire, and T. Jones, Govilon, Monmouthshire, "Improvements in the manufacture of iron into semi-steel or steel."

1184. W. E. Newton, Chancery Lane, "Improvements in the manufacture of salts of soda."—A communication from J. M. Gattman, New York, U.S.A.

1187. V. Gallet, Lavausseau de Benassais, Vienne, France, "Improvements in the manufacture of steel."—April 8, 1868.

1195. A. H. Still and D. Lane, Cork, "Improvements in the manufacture of gas."—April 9, 1868.

1222. T. Forster, Streatham, Surrey, "Improvements in the manufacture of compounds of india-rubber, gutta percha, balata, parkesine, solid paraffin or vegetable oils, and vegetable fibre."—April 13, 1868.

INVENTION PROTECTED BY THE DEPOSIT OF A COMPLETE SPECIFICATION.

1167. A. L. Holley, Harrisburgh, U.S.A., "Improvements in the manufacture of iron and steel."—Recorded April 7, 1868.

NOTICES TO PROCEED.

3270. G. Fitt, Limehouse, Middlesex, "Improvements in the manufacture of artificial manure."—Petition recorded November 18, 1867.

3288. Baroness C. de Lavenant, Brixton Road, Surrey, "Improvements in coating metals and metallic articles for the purpose of protecting or preserving the same from oxidation and decay, also in the materials, machinery, and apparatus to be employed therein."—Nov. 20, 1867.

3318. P. Salmon, Great Smith Street, Westminister, Middlesex, "Improvements in the manufacture of gas, and in the treatment and application of such and other gases for cooking, warming, lighting, generating of steam, and other purposes, combined with their use as a motive power, previous to being burned or consumed, and improvements in the apparatus for these purposes."—November 23, 1867.

3340. J. P. Smith, Glasgow, "An improved mode of coating and uniting metals with metals."—November 26, 1867.

5360. H. F. Gardner, Boston, U.S.A., "Improvements in the means of, and apparatus for, treating metals and minerals in order to produce their oxides or other chemical or mechanical combinations, and to separate metals from their ores or from their alloys."—A communication from *Z.* A. Willard, Boston, and W. G. Adams, Franklin, Mass., U.S.A.—November 27, 1867.

NOTES AND QUERIES.

Cast Porcelain.—In answer to "Silex" I am happy to be able to afford him the information he wants. Some specimens of this material were exhibited before the last meeting of the American Pharmaceutical Association, and the mode of manufacture described. I quote the following from their proceedings:—By a fusion of 1 part of cryolite with from 2 to 4 of pure silex, a beautiful glass is formed, susceptible of mould and polish, and capable of being manufactured into an endless variety of useful and ornamental articles, and probably many utensils for chemical and pharmaceutical use will be made of it. A company has been operating in Philadelphia for some time past, on an experimental scale, entitled the "Hot Cast Porcelain Company." The results have been so satisfactory that they have now taken a large establishment, and will be prepared to carry on the manufacture quite extensively. The cost is, at present, from 10 to 20 per cent higher than ordinary flint glass. The ware seems to be stronger than glass.—Pharmaceutist.

TO CORRESPONDENTS.

Dead Oil.—A correspondent who wrote about dead oil last week is informed that several communications are waiting for him at our office.

A. Deck.—Medlock's bisulphite of lime solution may be obtained of the manufacturers, Messrs. Bailey and Son, Wolverhampton, or Lawrence Pountney Hill, London, E.C.

Dyeing.—A correspondent who enquires on this subject, but gives no name, is informed that no information on the subject named is at present in our possession.

T. D. Read.—The number of the *Technologist* shall be sent.

A. N.—A sample of benzoic acid is waiting for this correspondent at our office.

Communications have been received from H. Porter; G. Bird; C. Greville Williams, F.R.S.; J. Attfield (with enclosure); T. D. Read, Montreal; McDougall, Bros.; Dr. Odling, F.R.S.; H. W. Whiteman, U.S.A.; J. A. Wykes; J. Barrow; A. McCulloch (with enclosure); A. Deck; T. Winter; Dr. F. C. Calvert, F.R.S.; H. Seward; F. O. Ward; J. Parnell (with enclosure); Rev. A. Rigg (with enclosure); E. Dolby; Cornish, Bros.; W. Darling; J. C. Lee (with enclosure); E. Wheeler, jun.; C. J. Ellam (with enclosure); L. Demuth (with enclosure); Sir Charles Taylor; R. Hogg (with enclosure); A. Gow (with enclosure) Dr. Jaeger.

THE CHEMICAL NEWS.

Vol. XVII. No. 447.

OBSERVATIONS ON FERRIC HYDRATE.

By Professor ATTFIELD, Ph.D.

In a memoir, noticed in the Chemical News of June 12 as having been recently presented to the Academy of Sciences, M. Jeannel, in allusion to the fact that ferric hydrate is not always soluble in acids, states that the incomplete solubility is, in his opinion, generally due to the influence of traces of sulphates. He says, according to the Paris correspondent of the Chemical News, "sesquioxide, precipitated from the persulphate, is always to a certain extent insoluble, or yields unstable salts; the same is the case with the sesquioxide precipitated from the perchloride, when this has been contaminated by sulphuric acid, or equally when the alkalies employed as precipitants have been so contaminated, or, finally, when the ferric hydrate, precipitated from pure solutions by pure alkalies, has been washed with common water." This explanation does not accord with my experience of the properties of ferric hydrate and oxyhydrates. Firstly, in England the ferric citrates and tartrates used in medicine are successfully made in large quantities by dissolving ferric hydrate, prepared from ferric sulphate, in solutions of the respective acids and acid-salts: Secondly, I have frequently seen moist ferric hydrate perfectly dissolve in solutions of acids or acid-salts, even though the precipitate has been washed with common water containing sulphate of calcium, a final washing with distilled water having, for various reasons, been neglected. Thirdly, I have often noticed that pure ferric hydrate, soluble when freshly precipitated, becomes imperfectly so if long kept, moist or dry. It is true that when alkali is added to solution of ferric sulphate, instead of the latter to the former, an insoluble oxysulphate is precipitated, and a similar compound may, possibly, be formed under other circumstances; but ferric hydrate, properly prepared and fairly washed, is readily soluble if only it be used in the moist and recently-precipitated condition, with a solution of acid or acid-salt which is not too weak, and the mixture be not boiled or even strongly heated for any considerable length of time. The fact is that ferric hydrate, even though kept under water, decomposes after a time, or more quickly if heated, losing the elements of water and becoming an oxyhydrate, a body insoluble in weak acids and, also unlike ferric hydrate, incapable of acting as an antidote to arsenic, that is, incapable of forming ferrous arseniate.

It may be useful again to draw attention to the decided alteration in properties which ferric hydrate spontaneously undergoes when exposed beneath the surface of water or when boiled with water, as evidence that this substance ($Fe_2 6HO$) is a true analogue of hydrate of sodium ($NaHO$), &c., and not a hydrous ferric oxide ($Fe_2O_3, 3H_2O$). It is more reasonable to suppose that in acquiring new properties ferric hydrate becomes changed to new compounds than to consider that the changes result from the loss of a portion of water already existing as water. Between ferric hydrate ($Fe_2 6HO$) and ferric oxide (Fe_2O_3) there would appear to be several oxyhydrates, analyses, &c., of most of which have already been given in the Chemical News (xvii., 56) by Brush and Rodman.

1. Fe_4 12HO.
2. $Fe_4 O$ 10HO.
3. $Fe_4 O_2$ 8HO.
4. $Fe_4 O_3$ 6HO.
5. $Fe_4 O_4$ 4HO.
6. $Fe_4 O_5$ 2HO.
7. $Fe_4 O_6$

In the above formulæ, No. 1 represents two molecules of ferric hydrate; Church found a stalactite of true ferric hydrate, native, in Cornwall, and Wittstein gives a similar formula to fresh artificial ferric hydrate. No. 2 is the only oxyhydrate, in this series, still unknown, unless, indeed Haughton's Kilbride mineral contains this body. No. 3 is brown iron ore from the Hüttenrode Hartz. No. 4 is the formula of a limonite and of artificial ferric hydrate altered by age—described by Wittstein as having a crystalline structure. No. 5 is the mineral gothite, and also the dried oxyhydrate commonly used in pharmacy. No. 6 is turgite, hydro-hæmatite, or the mineral from Salisbury, Conn., analysed by Brush and Rodman. No. 7 represents two molecules of ferric oxide.

ON THE RESOLUTION OF THE SOUNDING FLAME.*

By Prof. FRANCIS H. SMITH.

By those who have no mirrors, lenses, or revolving apparatus, and who find a difficulty in properly moving the eyes to and fro before the flame, the intermittent character of the latter, when sounding, may be exhibited by simply shaking a chalk crayon near it, and noting the marked change assumed in the appearance when the flame passes from silence to song.

With a revolving apparatus, however, the following form of experiment will be found satisfactory:—To the margin of a blackened disc of cardboard was cemented a silvered glass bead. This was set into rapid rotation in a dark room, and in close proximity to the flame in the glass tube. While the flame was silent, there was presented to the eye fixed upon the revolving bead a complete luminous circle, which, when the flame began its song, was broken up into detached beads of light. By increasing the velocity of the disc, these brilliant points, or lumps, were reduced in number, and stretched into separated luminous arcs. The tube used was 3 feet long, and it was found possible to secure such a speed as to have only five luminous arcs. If the revolving mechanism be furnished with a counter or register, like the syren, we have here a simple and ready method of determining the number of vibrations per second of a sounding flame. For this purpose the bead should be light and the disc small. For lecture-room illustration the silvered ball should be large, so as to give a larger image of the flame and a more voluminous bright circle.

To illustrate the use of the method suggested above, let me cite the following measurements:—Employing a glass tube $3\frac{1}{8}$ feet long, with an average diameter of nearly $1\frac{1}{16}$ inches, and keeping the bead whirling so as to present five stationary luminous arcs, the first and second observations gave each 1576·8 rotations of the disc in 43 seconds. The third and last gave 2014·8 rotations in 56 seconds. Giving to each observation the same weight, we have 182·2 vibrations of the sonorous flame per second.

The syren, applied to the same problem, gave 177·7 vibrations per second; but it must be added that I found it impracticable to keep my syren exactly in unison with the flame at so low a note. Beats were heard during almost the entire period (90 seconds) of the experiment.

The note in question, compared with those of a set of excellent tuning forks made by Richie, of Boston, was nearly F.

Again, the luminous arcs were, as nearly as I could judge, about 48° each in extent. This being so, the light of the sounding flame endured *conspicuously* at each pulsation $0^s{\cdot}00366$, or $\frac{1}{273}$ of a second.

It was noticed, too, that each arc varied in brightness, the point of maximum illumination being, not at the centre of the arc, but beyond it; so that it would appear that in

* From a communication to J. D. Dana, dated University of Virginia, Feb. 29, 1868.

each luminous interval, the flame lost its light more rapidly than it acquired it.

I have spoken of the luminous arcs as though they were absolutely detached from each other. So they appear to a careless observer. A close scrutiny, however, revealed an extremely faint and fading thread of bluish light uniting them. Hence, in this case at least, we cannot accept Dr. Tyndall's conjecture that there is an *absolute* extinction, periodically, of the singing flame.

I have applied the revolving silvered ball to the solution of the following problems:—

(1.) *To adjust two sounding flames to exact unison*, or to test the perfection of accord between two apparently unisonant flames.

The revolving ball presented to the two flames, gives two intersecting circles of images, which, in case of exact unison, are equal in number, and present identically the same retrogradations, stations, and advances, during the variable motion of the ball.

(2.) *To determine the exact, or the approximate value of the musical interval between two sounding flames.* Adjusting the velocity of the ball so that each flame gives a circle of *stationary* images (luminous arcs), the relative number of these images will express the interval required. Thus, for one pair of flames, I found the interval to be 2 : 1 ; for another pair, 4 : 3.

If, when one set of images is stationary, the other set has a slow motion, the relative number of the images will give an approximate value of the interval, the direction of the motion with respect to that of the ball determining whether the value is too large or too small.

For these applications it is obviously unnecessary for the rotating mechanism to be furnished with a register. Indeed a kaleidophone might be used for the same purposes, though with some inconvenience. The silvered balls I have employed vary from half-inch to 2 inches in diameter.

It is scarcely necessary to apprise the readers of this Journal, that the method herein set forth is a simple modification of an experiment devised and described by Prof. W. B. Rogers.*

In the progress of these experiments, it was often noticed that the luminous arcs, or stretched images, of the sonorous flame, were placed in echelon. The reason is manifest, when the silvered ball is revolved slowly and close to the flame. The separate images are then found to be inclined in the direction of revolution, their inclination augmenting with the velocity of the convex mirror, and the slope of the after edge of the image being steeper than that of the front edge. The same tilting of the images occurs, as is well known, in resolving the flame either by Wheatstone's or Tyndall's processes, when the mirror, plane or concave, is rapidly turned. I am not aware that the significance of this fact has attracted attention. Does it not indicate that the flame both recovers and *loses* its light progressively from base to summit, the loss being more rapid than the recovery, and that at no single instant of its history, is the sounding flame such in *size* and *form*, as it appears to be when steadily viewed ? Moreover, if the flame, in any case, be rekindled from above, as it must be, if it is ever absolutely extinguished while sounding, it would seem that its image on its first edge at least should be tilted *in a direction opposite to the motion of the mirror.*

I shall conclude these notices by stating a fact bearing upon the theory of these flames. I have found no difficulty in causing the flame to sing when inserted quietly into a horizontal tube, *carefully leveled.* Tubes of various lengths and diameters were used. In narrow horizontal tubes, the vibrations are soon arrested by the accumulating products of combustion. In a tube $1\frac{3}{4}$ inches in diam. and 3 feet long, the flame sang for an indefinite time. While it was sounding, no drifting of smoke previously introduced into the tube, or of a column of smoke rising past its distant end, could be detected. It is easy to pass from the ordinary *erect* position of the tube and gas jet, through all grades of inclination to *the inverted position of both, without cessation of the sound*, and without disturbing the *axial* position of the flame.

If by maladroit handling the flame is silenced at the critical attitude, it will be observed that the latter is below the horizontal position.—*Am. Journ. Sci.*, May, 1868.

* *American Journal of Science*, vol. xxvi, p. 10.

ON THE

ACTION OF FERROCYANIDE OF POTASSIUM ON MONOCHLORACETIC ETHER.

By O. LOEW,

Assistant in the Laboratory of the College of the City of New York.

Kolbe was the first to prepare cyanacetic acid, which is especially interesting from its transformation into malonic acid by treatment with potassa. In a similar manner Heintz prepared sulphocyanacetic acid by boiling monochloracetic ether with sulphocyanide of potassium. This led me to try the action of ferrocyanide of potassium upon monochloracetic ether, to ascertain whether the radical ferrocyanogen can participate, as such, in the reaction. I boiled monochloracetic ether, dissolved in alcohol of 90 per cent, with powdered ferrocyanide of potassium for 4 to 6 hours. An action gradually took place, by which chloride of potassium was formed, together with another light blue amorphous substance, which became of darker colour on exposure to the air, and was undoubtedly Prussian blue.

When the liquid was filtered and boiled with caustic potassa, ammonia was developed ; after the latter ceased to be evolved, the liquid was mixed with sulphuric acid and agitated with ether. On evaporating the ether I obtained white crystals, having the appearance of malonic acid. This substance was converted into the lead salt by precipitation with acetate of lead : 0·2997 grm. yielded 0·2904 grm. sulphate of lead = 66·99 per cent Pb. The malonate of lead contains 66·99 per cent Pb.

The reaction is therefore not analogous to that above cited, but the radical ferrocyanogen is broken up into cyanide of potassium and cyanide of iron. The former yields, with chloracetic ether, chloride of potassium and cyanacetic ether, while from the last named body malonic acid is produced.—*Am. Journ. Sci.*, May, 1868.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, June 18.

Dr. Warren de la Rue, F.R.S., &c., President, in the Chair.

The minutes of the previous meeting were read and confirmed, and several donations to the library acknowledged. Mr. Wm. Hustler, of Falmouth, was formally admitted a Fellow of the Society, and Messrs. H. B. Riddell and S. Alexander Sadler were duly elected. The following gentleman was proposed for election:—W. J. Palmer, M.D., F.R.C.S., Surgeon Bombay Army, and Additional Chemical Examiner to the Government of India.

The President reported the steps taken towards providing superior accommodation for the Society in the new buildings at Burlington House; this statement was well-timed, and appeared necessary in consequence of the front wall in Piccadilly having been parcelled out for sale by public auction. It is understood, however, that the

Italian colonnade will be preserved by special order of the First Commissioner of Her Majesty's Works.

Dr. J. H. Gladstone made a statement by way of postscript to his recent paper "*On the Tetraphosphoric Amides*." When nitrate of silver was added to tetraphosphopentazotic acid, $P_4N_5H_9O_7$, some hydrogen and nitrogen disappeared, and a difficultly soluble salt was formed, containing:—

$$P_4N_4H_4Ag_2O_7.$$

If the acid corresponding to this, viz.:—$P_4N_4H_6O_7$, be taken from the constituents of two atoms of the above pentazotic acid, there remain the elements of another compound, $P_4N_6H_{12}O_7$, which forms with silver a soluble salt. A special experiment, undertaken for the purpose of elucidating this point, gave 151 instead of 162 parts of silver salt from 100 parts of the first-named acid, a result close enough to warrant the assertion put forward by the author in explaining these somewhat complicated changes.

Professor J. A. Wanklyn read a paper "*On High Chemical Formulæ—the ground on which they rest: Examples, Mellisic Alcohol, Cerotic Acid, &c.*" The author insists upon the importance of establishing the composition of organic bodies of high formulæ, not only by reference to the method of ultimate analysis, but by studying a wide range of chemical reactions and decompositions. The highest members of the fatty series were selected for criticism by Mr. Wanklyn, who showed that Sir Benjamin Brodie's analyses of mellissic alcohol, published in 1848, led not only to the formula adopted, $C_{30}H_{62}O$, but would stand good for all compounds lying between the terms $C_{28}H_{58}O$ and $C_{37}H_{76}O$, which contain 81·95 and 82·84 per cent of carbon respectively. The theoretical number for the adopted formula being 82·19, and Sir B. Brodie's results ranging, for carbon, from 82·01 to 82·77 per cent. Hydrogen stands within even closer limits of variation, differing only by 0·03 per cent in the two extreme compounds already named. The same remarks were applied to mellissic acid and its silver salt; the analysis of the latter was said to agree equally well with formulæ differing from each other by CH_2, thus:—

C_{29} requires carbon	63·85	C_{30}	64·38
H_{57}		H_{59}	
Ag	19·89	Ag	19·30
O_2..		O_2..	

The author concludes by a recommendation to employ the method of titration of the ether, converting the mellissic alcohol, for instance, into a suitable ether, then liberating the acid, and determining it as a baryta salt. 100 parts of acetate of mellissyl treated in this manner should yield 26·56 parts of acetate of baryta, if its composition be

$$\left.\begin{matrix}C_{30}H_{61}\\C_2H_3O\end{matrix}\right\}O;$$

or 25·81 parts, in the event of its formula being

$$\left.\begin{matrix}C_{31}H_{63}\\C_2H_3O\end{matrix}\right\}O.$$

Professor Wanklyn referred to Berthelot's researches on glycerin as illustrating the advantage of pursuing this line of investigation.

The President said he had a clear recollection of the first appearance of Sir Benjamin Brodie's paper on Chinese wax, which was at the time considered to be one of the most masterly treatises in the then comparatively new department of organic chemistry. He could not help regretting that Professor Wanklyn had applied the criticism of modern science to an investigation published twenty years ago, for it should be remembered that the progress of chemistry had made great strides during this interval. Chemists generally were aware that it is difficult to fix the rational formula of a body from the evidence afforded by combustion alone; but, so far as Sir Benjamin's resources enabled him to proceed, there did not then appear to be any direction in which he had not pushed his enquiries.

Dr. Odling said it was already well known that the method of ultimate analysis failed altogether in discriminating between bodies containing high proportions of carbon and hydrogen. He did not think that Professor Wanklyn had acted wisely in selecting Sir Benjamin Brodie's investigation for the purpose of this kind of criticism, for whilst he questioned the accuracy of published results he had no fresh evidence to offer. It might happen that the acetate of mellissyl was not easily prepared, and we must then have proof that no disintegration of the alcohol occurred; in fact, for his own part, Dr. Odling considered that the *onus probandi* lies entirely on the other side.

Dr. Hugo Muller made enquiry respecting the mode of preparing the acetate, and the criterion by which its perfect purity might be ascertained. If a little of the alcohol were left unattacked, the fusing point would be considerably raised; even in the comparatively simple case of the decomposition of iodide of ethyl by a silver salt a certain amount of destruction always occurred at the outset, and the change did not exactly coincide with the demands of theory.

Professor Wanklyn, in reply, would refer his enquirers to Sir Benjamin Brodie's original paper, for an account of the preparation of the acetate. It might be made by passing hydrochloric acid through a solution of the mellissic alcohol in acetic acid. The speaker's object in bringing forward this subject for discussion was to show that the methods of investigation formerly in use did not lead to decisive results, and yet the conclusions were reproduced in the chemical manuals without anything to show that they were not so well established as lower terms in the same series. He would again insist upon the propriety of continuing these investigations in the manner pointed out by Berthelot, notwithstanding the objection and abuse which that author had since had to contend with.

Dr. Odling reminded the last speaker that his idol was not altogether infallible, since compounds of glycerin with acids were described, the highest member of which (containing 4 atoms of acid) had subsequently to be withdrawn.

Professor Wanklyn said this was no fault of the method, but only an error on the part of the operator, which might with greater care have been avoided.

Dr. W. J. Palmer then read a paper "*On the Production of Saltpetre in India*," which led to an animated discussion on the process of nitrification.

Dr. F. Guthrie exhibited "*An Instrument for Maintaining a Constant Temperature*," where gas is available; and the business of the evening was brought to a conclusion by the reading of some Mineralogical Notices, embracing "*An account of Chalybite, Diallogite, and Woodwardite*," by Professor A. H. Church, M.A. [Abstracts of these papers will be given next week.]

The meeting being the last of the session was adjourned, at a somewhat late hour, until the first Thursday in November.

CORRESPONDENCE.

ON NUMERICAL FORMULÆ.

To the Editor of the Chemical News.

Sir,—I have found the following plan to be of service as a means of fixing the atomic weights in the memory, and possibly it may be found useful for the like purpose by some of your numerous readers.

With this object in view, I use occasionally formulæ constructed without any letters whatever, the atomic weight of each element being used as its symbol. Between each set of figures a stop is placed; brackets are made use of when requisite, and the number of atoms or

molecules entering into any reaction is always represented by a small figure.

I subjoin a few formulæ, and an equation constructed on this plan:—

Hydrochloric acid	1.35·5
Water	1_2.16
Ammonia	1_3.14
Marsh-gas	1_4.12

The action of iodine on caustic potash is represented thus:—

$$1276+(1.39{\cdot}1.16)_6=39{\cdot}1.127.16_3+(39{\cdot}1.127)_5+(1_2.16)_3.$$

Or, iodine *plus* caustic potash gives potassium iodate, potassium iodide, and water.

While upon this subject I may mention that the formulæ of compounds of carbon, hydrogen, oxygen, and nitrogen with each other, when required to be written down rapidly, may be conveniently represented by using the figures only which indicate the number of atoms in the ordinary formulæ. Of course the figures must always be written in one and the same order. For example, the first figure may be used to express the number of atoms of carbon; the second, the hydrogen; the third, the oxygen; and the fourth the nitrogen. Should one or more of these elements be wanting, a cipher, or ciphers, may be used to supply its place, and thus indicate the meaning of the following figures.

Formulæ constructed on this last plan are not, as a rule, more than half the length of those in which symbols are used, and may be read by any one with the greatest ease.

In constructing tables containing a large number of formulæ of organic compounds, this omission of the symbols will be found to economise space.—I am, &c.,

John Newlands, F.C.S.

Oberlahnstein, on the Rhine,
June 8th, 1868.

SALT AND RADICAL.

To the Editor of the Chemical News.

Sir,—In continuation of the subject for which you have kindly allowed me space in your journal (No. 443), I beg to make a few remarks upon the meaning of the terms salt and radical. What is a salt?

Although the term salt is in constant use in the text-books, there is perhaps no chemical expression so little understood by schoolboys as this. One would naturally suppose that a word so frequently used possessed some definite meaning; yet the boy who takes up his text-book to find a definition of the word salt would be woefully disappointed, the fact being that at present chemists cannot state clearly, shortly, and concisely what they mean by the word; and hence the absence of any adequate definition, and the present cloudiness of ideas concerning it.

But, Sir, I am fain to believe that if that mischievous substantive "acid" were deprived of all chemical significance, and be used in chemistry only as in common life, to denote an acid taste, just as we use the term sweet, some reasonable, if not absolutely correct definition of the term salt may be arrived at. Something, at least, sufficiently definite to enable the master to teach by, and to satisfy the natural craving of the student for a meaning to the word he is compelled to use.

Before I endeavour to define the term salt, I must ask permission to consider the chemical bearing of the term radical.

1st. Every element is a radical, because it can, without suffering decomposition, enter directly into combination to form a characteristic class of salts. This fact is not broadly and plainly stated in our text-books.

2nd. Although every element is a radical, every radical is not an element. To chemists this assertion may seem self-evident and gratuitous, but we are apparently afraid to make a full admission of the meaning of the word radical in our text-books, and surely we cannot be too plain and simple with beginners.

For all purposes of elementary teaching I would broadly define a radical to be any body that, without itself suffering decomposition, is capable of entering into chemical combination with another.

Further, radicals are either simple or compound, real or imaginary. Simple radicals are the elements; compound radicals are such bodies as Cy, &c.; and imaginary radicals, as SO_4, NO_3, &c., are those which have never been isolated, and, like atoms, only exist in the mind and imagination of the chemist, created for his own use and convenience, but rendering the acquirement of the science by the young much more difficult than it would be without them.

If these views of a radical be correct, a salt will be any compound formed by the union of two or more radicals, and will take its characteristic place and name from the radicals composing it. From this definition it would result that a compound radical is already a salt. If, however, a compound be capable of uniting with another, or taking part in a chemical equation without itself suffering decomposition, it should receive the name radical, compound radical, or salt radical, to distinguish it from a true salt.

Thus HCl, HNO_3, and H_2SO_4, are true salts composed each of more than one radical. If these bodies are salts with a sour taste, let us teach that they are such, and not that they are a special class of chemical compounds capable of generating salts; for radicals, not acids and bases, are the bodies of which salts are composed.

There is, perhaps, nothing more difficult to give than good definitions, and I am well aware that my own are very likely to provoke severe criticism. I trust, however, that the propriety of having something more teachable with regard to the terms commonly used in chemistry, will recommend itself to all, and that whatever my shortcomings may be in this attempt to obtain a general accord as to the terms taught in our schools, and the meanings to be attached to them, my labour will not have been in vain if only something definite be settled upon.—I am, &c.,

Thomas Wood, Ph.D., F.C.S.

Laboratory, 23, Grove Street,
Lisson Grove, N.W.

UTILISATION OF WATER POWER.

To the Editor of the Chemical News.

Sir,—In the Chemical News of September, 1863, appeared a letter from me on the conversion of water or wind power into light and heat by means of magneto-electricity; permit me to give a few considerations as the result of subsequent attention to the subject. The magneto-electric machine is the cheapest known source of electrical power, and would effect an immense saving of coal for lighting purposes, A good and cheap self-acting electric lamp is, however, necessary for this, and attention should be drawn to the subject. Magneto-electric machinery, driven by steam power, would save at least half the coal now used in gas works, while producing more light. Details could be furnished on this point shewing a much greater probable saving than one half.

Magneto-electric machinery driven by water power would, of course, save all the coal used in gas works. There are many districts with reliable water supply in which this great result might be obtained.

On the appearance of Professor Way's mercurial electric-lamp, you showed in a leading article the ease and advantage with which conducting wires might be fixed from a central light-producing magneto-electric factory, along streets and into houses. Would it not be true economy to capitalise some portion of our coal by employing steam power to excavate tidal basins at some of the many suitable points around our coasts for utilising tidal force in

driving various forms of machinery, thus exchanging a fleeting for a permanent motive power? The conclusions of Mr. Jevons add force to this suggestion.

Mr. W. M. Durham, in his interesting letter last week, advocates the construction of reservoirs for storing winter floods and storm waters in rivers for motive purposes. The entire subject well deserves attention.—I am, &c.,

G. Alex. Keyworth.

Hastings, June 22nd, 1868.

PS.—As regards economy of fuel, it would be important to know whether the plan suggested, I think by Professor Tyndall, has been tried of covering the boiler and other heated parts of the steam engine with an iron cased lining tightly filled with powdered gypsum. —*G. A. K.*

MISCELLANEOUS.

Death of Mr. Capel Henry Berger, F.C.S.—An inquiry was held on Tuesday evening last by Mr. Richards, deputy coroner, at Sion-house, Lower Clapton, relative to the death from the inhalation of poison of Mr. Capel Henry Berger, aged 28 years. Mr. C. Berrow Berger, Sion-house, said that deceased lived with him, and was a colour manufacturer. He suffered for a fortnight past from a very severe toothache, but a dentist advised him to preserve the tooth and bear the pain. He was an accomplished chemist, and he tried all sorts of things to allay his sufferings. On Sunday last while at church he had to sit in a great draft, and that brought on a relapse of the pain. In the afternoon he went to his room, according to his custom, and bolted himself in for the purpose of spending some time in devotion. When his sister called him down to tea, she could not make him hear, and ultimately witness broke open the door, and found him lying dead on the floor upon some flexible tubing which communicated with a bottle of carbolic acid. His face was quite black, and he had vomited. It was clear that he had died from the carbolic acid, but he had not committed suicide. Dr. J. B. Metcalf said that deceased had fixed an elastic tube, 10 feet long, to a large glass jar of carbolic acid, and had then evidently seated himself in a chair, and had inserted the end of the tube in his mouth, for the purpose of allowing a drop of the liquid to fall on the tooth. He had a brass regulator on the tube to control the quantity of the acid, but it did not act efficiently, and the volatile poison overcame him, and he became giddy and fell. Being alone in the room the poison continued flowing into his mouth, and the heart's action was stopped, and he died. The remedy which he tried was a new one, and the deceased was in the habit of recommending it to his friends. It should never be used without medical assistance. The jury returned a verdict of "Accidental death from inhaling carbolic acid as a cure for the toothache."

Conversion of Mesitylenic Oxide into Mesitylene and $C_{10}H_{14}$.—A. Holtmeyer. Commercial acetone was saturated with chlorhydric acid, the mixture left to itself for a week, and then washed with water. The insoluble portions were dried and distilled, and the fraction boiling between 90° and 100° C. treated with alcoholic potassic hydrate. An oil was thus obtained from which at 128-130° mesitylic oxide distilled off. This oxide, after further purification, was mixed with half its volume of sulphuric acid, and after two days distilled. Two fractions were collected, one at 163-166°, one at 195°. The first was mesitylene—this shows that mesitylic oxide occupies an intermediate position between acetone and mesitylene :—

1. $2(C_3H_6O) - H_2O = C_6H_{10}O$.
   Acetone. Mesitylic oxide.
2. $3(C_6H_{10}O) - 3H_2O = 2(C_9H_{12})$.
   Mesitylene.

The second, a hydrocarbon of the composition $C_{10}H_{14}$. This could be converted into the bromo- and nitro-compounds, $C_{10}H_{12}Br$ and $C_{10}H_{11}(NO_2)_3$; a sulpho acid was also obtained, the baric salt of which corresponded to the formula $(C_{10}H_{13}SO_2O)_2Ba$.—*Ibid.*, N.F., iii., 688.

Venom of Toads.—The Toad, formerly considered as a creature to be feared, does in reality possess a venom capable of killing certain animals and injuring man. This poison is not, as is generally thought, secreted by the mouth : it is a sort of epidemic cutaneous secretion, which acts powerfully if the skin be abraded at the time of contact. Dogs which bite toads, soon give voice to howls of pain. On examination it is found that the palate and tongue are swollen, and a viscous mucus is exuded. Smaller animals coming under the influence of the venom undergo true narcotic poisoning, soon followed by convulsions and death. Experiments made by MM. Gratiolet, Cloez, and Vulpian, show that the matter exuding from the parotid region of the toad becomes poisonous when introduced into the tissues. A tortoise of the species *Testudo Mauritanica*, lamed in the hind foot, was completely paralysed at the end of fifteen days; and the paralysis lasted during several months. Some savages in South America use the acid fluid of the cutaneous glands of the toad instead of the curara. The venom exists in somewhat large quantities on the toad's back. Heated with ether, it dissolves, leaving a residuum; the evaporated solution exhibits oleaginous granules. The residuum contains a toxic powder sufficiently strong, even after complete desiccation, to kill a small bird.—*British Medical Journal.*

Odoriferous principle of Fleur de Garance and Garancine Alcohol.—It is a well known fact, that when ground madder root is prepared into garancine, or fleur de garance, a saccharine liquor is obtained which on fermentation and distillation yields an alcohol which is contaminated with what, for years past, was considered to be methyl alcohol. More recent researches have brought out the fact that the peculiar odour of the alcohol obtained from the washing liquors of the garancine and fleur de garance works is due to acetic ether, and far more largely to aldehyd. The impure madder spirit, as ordinarily met with in trade, is an excellent source from which to prepare and obtain large quantities of aldehyd ammonia by the following method :—From 20 to 30 litres of the impure spirit are placed in a distilling apparatus provided with a long metal (copper) worm, and heated to 60° or 70° C. while at the same time a rapid current of air, or carbonic acid, is passed over the liquid in the still; the distillation is continued until the distillate ceases to give with solution of caustic potash a darkish colouration. To the distillate twice its bulk of water is added, while in order to decompose the acetic ether, finely powdered hydrate of baryta is added with continuous agitation of the liquid until a decided alkaline reaction sets in; the excess of baryta is removed by carbonic acid. The aldehyd is afterwards separated from the liquid by careful distillation on a water bath, and purified by combining it with ammonia. If the impure madder spirit is agitated with sodium amalgam, perfectly pure alcohol is obtained; since the aldehyd is by this means hydrogenised to alcohol, and the acetic ether is decomposed into alcohol, while acetate of soda is formed.

TO CORRESPONDENTS.

Communications have been received from J. H. Freeman, F.C.S. (with enclosure); Dr. J. Attfield (with enclosure); Dr. F. A. Genth (with enclosure); G. W. Wigner (with enclosure); Philip Holland (with enclosure); Messrs. Townsend and Adams (with enclosures); Prof. C. W. Eliot; Beer and Co.; F. C. Calvert and Co. (with enclosure); Dr. R. Angus Smith, F.R.S.; Prof. Heaton (with enclosure); Howard Grubb; J. Newlands (with enclosure); Dr. T. Wood (with enclosure); G. A. Keyworth (with enclosure); P. Le Neve Foster; Bradburn and Co.; W. Bailey and Son; J. L. Denman; W. B. Pinman (with enclosure); J. I. Headley; G. Marrison, Tasmania; Capt. W. A. Ross; Cornish, Bros. H. H. Watson; W. Smith.

Several letters, notices, and papers for publication included in the above acknowledgements are unavoidably postponed till next week

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